REVIEW SYMPOSIUM

WE HAVE ALWAYS BEEN . . . CYBORGS

Andy Clark, *Natural-Born Cyborgs: Minds, Technologies, and the Future of Human Intelligence*. Oxford: Oxford University Press, 2003. Pp. viii + 229. US$26.00 HB.

By Terry Dartnall

This book is an exploration and popularisation of 'active externalism' or the 'extended mind' hypothesis (Clark, 1997, Clark and Chalmers, 1998, Dennett, 1996, Donald, 1991, Hutchins, 1995). I begin with some background about active externalism and the two principal arguments for holding it: the *parity argument* and the *complementarity argument*. This book develops the complementarity argument. I think that both arguments are problematic. But I go on to suggest that this fascinating book provides us with preliminary grounds for endorsing a version of active externalism that is stronger and stranger than the one Clark actually defends.

Active externalism is the belief that mind extends into the world, beyond the skin-and-skull boundary. Clark and Chalmers (1998) say that *cognitive processes* extend into the world when we use pen and paper to work something out, or when we use a computer, or even when we use language, which Clark thinks was the first technology. Also, *cognitive states* extend into the world when we use physical objects, or data-structures such as chips or CD-ROMs, as *external memory stores*.

In the past, Clark has given us two reasons for subscribing to this thesis. The first (and original) reason was the *parity argument*: if something counts as cognitive when it is performed in the head it should also count as cognitive when it is performed in the external world. Suppose you have to rotate images of geometrical shapes on a computer screen. You can rotate them using a neural implant in your head, or you can rotate them using a 'rotate' button in the world. Presumably we will say that the implant case is cognitive – so why isn't the button case cognitive as well? Clark and Chalmers say that

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epistemic credit is due where epistemic actions are performed, regardless of whether they are performed in the head or in the world.

This, however, only covers *cognitive processes* and Clark and Chalmers admit that the processes might be in the world whilst all of our "truly mental states – experiences, beliefs, desires, emotions, and so on" might be in the head (1998, p. 12). They therefore take the parity argument a stage further and argue that *cognitive states* can be constituted partly by features of the environment.

This brings us to the strange case of Otto's notebook. Otto suffers from Alzheimer's disease. He hears that there is an exhibition at the Museum of Modern Art. He consults his notebook, which says that the Museum is on 53rd Street. He walks to 53rd Street and goes to the Museum. Clark and Chalmers say that the notebook plays the same role for Otto that biological memory plays for the rest of us. It just happens that "this information lies beyond the skin" (p. 13).

Clark and Chalmers' claim is not only that Otto's belief is out there in the world. It is that he believed the Museum was on 53rd Street before he looked it up, courtesy of the "functional isomorphism" between the notebook entry and a corresponding 'entry' in biological memory. This parity argument is in effect a challenge: if we can come up with a non-question-begging, relevant difference between the entry in the notebook and the entry in biological memory we should say what it is.

Adams and Aizawa (2001) take up the challenge. They say that one thing that characterises a belief in the head, as opposed to a belief in a notebook, is original intentionality – intrinsic, non-derived content. Beliefs in notebooks derive their meanings from conventions or social practices, which ultimately derive from the capacities of cognitive agents. Beliefs in the head, on the other hand, are not derived from anything. They have intrinsic, non-derived content. If we take intrinsic content to be the mark of the mental (which they say is a "rather orthodox theory of the nature of the cognitive", p. 52) then it just so happens that everything that is mental is in the head.

In his *Metascience* review of Clark's 1997 book, *Being There*, Gerard O'Brien (1998) pointed to other relevant differences. Most obviously, beliefs in the head are causally active. My belief that there is an exhibition at the Museum of Modern Art, plays a causal role in my going to the Museum. Entries in notebooks, on the other hand, are causally passive: "The information they encode does not do any work unless we bring them under the gaze of our perceptual equipment. At this point the recorded information does become causally

active, but only because it is now re-coded elsewhere – namely, inside our skulls'' (O'Brien, 1998, p. 82).

In his reply to O'Brien, Clark admits that there is a ''potential tension'' between two components of the extended mind story. The parity argument stresses ''the way that extra-neural elements can play a role similar to internal ones (as in talk of external memory, etc.)'' (Clark, 1998, p. 99). But in *Being There* he had provided another, ''more interesting and plausible argument'' that turns on the way ''external elements may play a role different from, but *complementary to*, the inner ones' (ibid., my emphasis) This is the *complementarity argument*, and it now moves into the ascendancy. Clark says: ''The argument for the extended mind *turns primarily* on the way disparate inner and outer components may co-operate so as to yield integrated larger systems capable of supporting various (often quite advanced) forms of adaptive success'' (ibid., my emphasis). This is the argument of *Natural Born Cyborgs*, to which we now turn.

From Bioborgs to Cyborgs

Clark starts with Clynes and Kline's original 1960s vision of a cyborg. This was ''a human agent with some additional, machine-controlled, layers of automatic (homeostatic) functioning, allowing her to survive in alien or inhospitable environments'' (p. 32). Clark thinks that this vision was too narrow, because it ''restricted the imagined cyborg innovations to those serving various kinds of bodily maintenance'': the real human-machine symbiosis ''expands and alters the shape of the psychological processes that make us who we are''. It provides an ''array of resources to which biological brains, as they learn and grow, will *dovetail* their own activities''. This leads to the creation of ''extended computational and mental organisations: reasoning and thinking systems distributed across brain, body, and world'' (pp. 32–33).

This complementarity claim has two intimately interrelated components, which I shall call the 'bioborg' and the 'neural opportunism' arguments. The bioborg argument is that we are already bioborgs (my term), with modular brains and on-board robot-like devices (let's call them 'biobots') that we can launch to do our bidding. This leaves us well placed for mechanical augmentation, for a meshing between our onboard abilities and technologies out there in the world.

Our bioborg nature is beautifully illustrated by Aglioti's extension of the Titchener Circles Illusion (p. 101). This illusion makes us *consciously* see a circle as larger than it really is, but when the circle is replaced by a plastic disk and we reach for the disk our thumb and forefinger form the right-sized aperture to pick it up. A plausible explanation is that we have two incoming visual pathways and the information processing associated with one of them is fooled by the illusion whereas the information processing associated with the other is not. The information that comes in through the ventral stream is routed through consciousness, reasoning and memory. The information processing associated with this pathway is fooled. The information that comes in through the dorsal stream is devoted to the fine control of ongoing action. It is not routed through consciousness and the brain is not fooled. The brain does not report this incoming information to consciousness. It just gets on with the job, launching the biobot 'pick up the disk' routine.

The neural opportunism argument is that our brains use enduring features of the world as external memory stores that we can consult as needs dictate. When we scan ('saccade') around a room our eyes return to the same place time and again. They do this because *that is where the information is*. We do not need rich inner models. We only need a broad idea of what's out there, plus the ability to zoom in and retrieve detailed information when we need to – on a need-to-know basis. This gives us the best of both worlds. It is less demanding on memory but enables us to command a rich database – that is *out there in the world*. This opportunism has operated in consort with our bioborg brains to create cognitive technologies that complement our basic cognitive abilities.

Clark thinks that language was the first such technology. Language gives us a "cognitive short-cut" (p. 70). Research on chimpanzees shows that the use of plastic tokens enables them to reduce high-order abstract problems to lower-order problems that their brains can handle. Chimps were trained to associate a plastic token, such as a red triangle, with any pair of identical objects (such as two shoes) and to associate a differently shaped token with any pair of different objects (such as a beer can and a banana). The chimps could then solve the more complex problem of categorising pairs-of-pairs of objects in terms of *higher-order* sameness or difference. They could classify shoe-and-cup (different) as *the same higher order relationship* as beer-can-and-banana (also different). They could do this because

both pairs would get the same kind of token. All they had to do was to compare the tokens.

Clark thinks that words work in the same way. They label complex concepts and enable us to 'freeze' our thoughts. This enables us to *think about* our thoughts and ideas. (Is it a good idea? What are my reasons for believing it?)

Language was the first phase of our cyborg existence. We needed to be smart in order to develop it, but it was our ability to turn thoughts and ideas into new stable objects that ''started the real cognitive snowball rolling'' (p. 83). We were then drawn upwards in a virtuous circle as one technology led to another. We have upgraded our mindware from speech to writing, through increasingly flexible forms of printing, and are about to engage in an intimate relationship with machines.

Language is what Clarks calls a 'transparent technology'. We are so well integrated with it that it is almost invisible in use. We are surrounded by such technologies (pens, watches, telephones etc.) and are rapidly developing new ones. Mobile phone technology will be extended to give us cheap, tiny cameras that will beam information to family and friends whilst we are shopping. Houses will have wall-mounted weather displays. Implants in our bodies will communicate with one another about where we are and how we are. Augmented reality will overlay our experience of the world with personalised information, beamed to us by satellite. Lost on campus we will enter 'library', don an eyeglass, and see a green arrow pointing to the library.

Clark claims that we are so well integrated with these technologies that the problem-solving system is the biological system plus the technology. Suppose that somebody asks you if you know the time. You say that you do and *then* you look at your watch. You say that you know it because you know that you can easily find it out. What does the knowing, according to Clark, is the extended system of you-and-your-watch. Again, suppose you have a chip on your shoulder that gives you easy access to information about women basketball players. Clark says that there is no relevant difference between accessing the information on the chip and accessing it in long-term memory. You *know* the information because you have easy access to it, and what does the knowing is you-and-the-chip.

We need to throw two more things into the mix. The first is *constructive learning*: the cortex is plastic and the environment can build neural substructures 'on the hoof', altering our internal architecture.

The second is our long childhood, which gives our adaptable brains time to mesh with out-there technologies.

Cognitive States Versus Cognitive Content

What are we to make of all this? Let's go back to the parity argument. O'Brien says that beliefs in the head are causally active whereas diary entries are not. I think that this is a symptom of a greater divide – between a cognitive state and the content of a cognitive state. In one sense, my belief that there is beer in the fridge is a cognitive state. It is in my head. It is the state of *believing* that there is beer in the fridge. Beliefs in this sense can be strong and passionate, sincere or insincere, short-lived or long-lasting. They can be causally active. But in another sense, my belief that there is beer in the fridge is the *content* of my cognitive state. It is *what* I believe. It is a proposition that can be written down in a public, communicable symbolism that expresses not only the content of my belief but the content of yours as well. Beliefs in this sense can be true or false, tautologous or contradictory, subscribed to by one or many, but they cannot be strong and passionate, sincere or insincere, short-lived or long-lasting. Nor can they be causally active. My state of believing that there is beer in the fridge might cause me to go into the kitchen, but the *content* of my belief, the proposition, divorced from the cognitive state, cannot cause anything.

Another way of distinguishing between state and content is to show that different states can have the same content. We can believe and fear the same thing – that there is no more beer left in the fridge, for instance.

If we confuse these different senses of belief we will end up saying that a belief is sincere and tautologous, or that it is contradictory and four years old, which is like the old joke that Gilbert Ryle used as an example of a category mistake: "she came home in a flood of tears and a sedan chair". Frege was one of the first to draw the state/content distinction: "a proposition may be thought, and again it may be true; never confuse these things" (Frege, 1967).

Clark and Chalmers begin by arguing that cognitive processes can extend into the world, but admit that "truly mental states – experiences, beliefs, desires, emotions, and so on" might be in the head (p. 12). Consequently, they take their argument a stage further and

argue that cognitive *states* can extend into the world. But the distinction between state and content undermines their argument, because the natural thing to say is that cognitive states are in the head, even though their content may be in the world. Otto's diary contains the *content* of his cognitive state, but the state itself (once he has read his diary) is in his head. Similarly with the complementarity argument: Clark says that cognitive states can mesh with cognitive technology, which can determine the content of the states. OK. But why does this mean that our *states* extend into the world?

Clark says that when we mesh with our technologies we form an extended system, and that this system is the new self. There is a weak and a strong claim here. The weak claim is that we form an extended system. The strong claim is that this extended system is the new self. Consider the strong claim first. One of Clark's examples is piloting a commercial jet-liner, which he says is a task in which "human brains and bodies act as elements in a larger, fluidly integrated, biotechnological problem-solving matrix" (p. 25). But when we see Bert flying his Boeing overhead we do not say: "There goes Bert, with his four engines". We do not think of Bert, when he is flying his Boeing, as Bert-and-his-Boeing (and just plain Bert, when he is not).

The weak claim is equally problematic. If I dig a hole in my garden with a spade, it is not the coupled system of me-and-the-spade that does the digging. My-spade-and-I do not get the prize for 'best hole in the garden'. I get the prize, even though I could not have done the digging without the spade. By the same token, when I use pen and paper to complement my on-board pattern-recognition abilities, it is not the coupled system of me-and-the-pen-and-paper that performs the operation. It is just *me*, using the pen and paper. As Adams and Aizawa say, "mere causal coupling of some process with a broader environment does not, in general, thereby extend that process into the broader environment" (ibid., p. 56).

Now, it seems to me that we can abandon both of these claims and leave a version of the complementarity argument intact. We can still say that our on-board pattern-recognition abilities mesh with cognitive technologies (just as I mesh with my spade). We can even say that these technologies determine the content of our cognitive states. Reading an entry in my diary will determine the content of at least one of my cognitive states at the time. A central thesis that Clark wishes to defend is that we mesh with cognitive technologies, which determine the content of our cognitive states – as in the case of the diary. In this sense, of entwining and enmeshing, we can still be

cyborgs, whilst still being old-fashioned skin-bags with our cognitive states inside our skulls.

SEND FORTH THE AVATARS!

Things are not so simple, however. I introduced the distinction between state and content to force us to ask whether cognitive *states* can extend beyond the skin-and-skull barrier. Although the lines of thought I have examined so far give us no reason to think so, Clark nevertheless provides other unsettling evidence that they can.

Let me explain my strategy here. This book provides us with good preliminary grounds for defending a version of active externalism that is stronger than the version Clark actually defends. That is, there is evidence in the book for entertaining such a position, even though Clark does not do so himself. Consequently I find myself in the strange position of criticising the claims he does make, whilst endorsing a stronger claim that he does not make. I may, of course, have misunderstood his reasons for advancing the evidence in question. My guess, however, is that he does not adequately distinguish between state and content and, consequently, does not ask whether cognitive states (not content, but *states*) can cross the skin-and-skull barrier. I am inclined to think that they can. Let us look at the evidence.

First, Clark relates Daniel Dennett's story, "Where Am I?". Dennett's brain is removed and put in a vat, but it is kept in radio contact with his body and senses. When the nurse wheels him in and he sees his brain in the vat he feels that he is *outside* the vat, looking at his brain. (His body is later trapped by a rockslide, entombed far beneath the Earth's surface. At first he feels trapped, but as the radio links give out he feels himself relocated into his brain.)

Clark also tells us about the two-feet-long nose. You are blindfolded and sit behind someone. A volunteer takes your right hand and uses it to tap and stroke the nose of the person in front of you, whilst tapping and stroking your nose with your left hand. If this is done in perfect synchrony there is a 50% probability that you will feel that your nose is two feet long. A similar scenario is that you look at an unattached, dummy hand. Somebody taps the dummy hand and your hand in perfect synchrony. Many subjects experience sensation in the dummy hand.

These experiments illustrate Ramachandran's Principle, which is that the brain depends on *perceived correlations* to construct a model

of (and hence a sense of) bodily bounds and locations. In the nose case it depends on the correlation between tapping a nose and feeling your nose being tapped. Another way of saying this is to say that the brain will give us a sense of presence if it gets the right kind of feedback from its actions – if the feedback is fast and fluent, if actions are reciprocated, if there is flow and continuity. If these conditions are satisfied our brains can project feeling and sensation beyond the limits of the biological body, to get a new kind of body image that includes non-biological components. Clark says that the big lesson is that "embodiment is essential but negotiable" (p. 114). Humans are never disembodied intelligences. All the evidence underscores the importance of touch, motion and interaction.

It seems to me that embodiment is *highly* negotiable. Suppose that Dennett's eyes are divorced from his body but kept in radio contact with his brain. His brain is in the vat and his body is in bed. The nurse wheels his eyes in on a very small trolley. He now sees his brain in the vat and his body in bed. His cognitive states and his sense of himself are outside his body, where his eyes are.

Similarly, when you experience a sensation in the unattached dummy hand you experience a sensation that is outside your body. When you have a pain in your foot the pain really is in your foot. Nobody wants to say that it is in your brain. By the same token, when you feel a sensation in the dummy hand the sensation really is in the hand, not in your brain.

Fast, fluent feedback, reciprocal action, and flow and continuity are constraints that, in principle, are easily satisfied. Suppose we send out sensory avatars. Our eyes go to Titan and our ears go to Europa, whilst our brains stay at home in their concrete bunkers. Our sense of our location now depends on the sensory modality we focus on. We will need feedback and reciprocity, but this can be achieved. In fact we only need the *semblance* of feedback and reciprocity. This can be achieved by emulator circuits, which Clark discusses (p. 106). We use such circuitry in our everyday interactions with the world, to speed up our responses to external stimuli by anticipating what they are going to be. The input from our remote sensors can be veridical even if the reciprocity is somewhat contrived. What matters is that we will have a perfectly veridical sense of 'being there', even though our physical presence is minimal.

I suspect that our physical presence can become vanishingly small, with multiple sensory avatars (located in different places) enabling us to switch our sense of location at will. And why have real avatars? Why

not have virtual ones, which would give us the input that we would get *if* we (or they) were at the location? This information might be retrieved from memory or it might be a prediction based on a general knowledge of the area (think of the complex calculations involved in predicting the weather; then think of a similar science of *perception prediction*). This information, suitably packaged with feedback and reciprocity, would give us a feeling of being present at the location.

What about our bodies back home? Our sense of ourselves would go outwards whilst the information-processing wetware remains behind. From our distant eyries on Titan we might see relays of our ancient brains and think how quaint they are. But if we achieved this level of technology it is unlikely that we would trust our survival to anything as unreliable as our ancient wetware. If the brain really is a tool-kit of devices we will probably replace them with more reliable ones, and plenty of backups. These need not be stored in a single, central location. Some might go forth with the avatars, for ease of communication, whilst others remain behind. Some might belong to a single, shared resource, so that our minds mingle together (think of the World Wide Web). We can imagine these things. Clark says that our sense of ourselves is a construct. I do not see why our sense of ourselves, or the location of our cognitive states, has to be tied to our bodies. Or that we will need bodies in any strong sense, since our components can be distributed over many locations.

These are heady issues (or not . . .). They arise when we ask what I think is the key question: can cognitive *states* be outside the head? Talking about cognitive content is not going to cut the mustard.

I began this review by saying that *Natural-Born Cyborgs* is an exploration and *popularisation* of active externalism. This is a very readable book, pitched at a popular level. It is anecdotal and upbeat and taps into the spirit of the times. It is also a very funny book, in which Andy Clark's inimitable personality and endless optimism shine through. I am sure other readers will enjoy it as much as I did.

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To automobile drivers, who can literally *feel* themselves extend outward into the tyres, the claim that human minds and selves are extended, distributed entities not circumscribed by the body is an unremarkable fact. Among philosophers of science, on the other hand, it amounts to an ontologically preposterous suggestion. In a previous book, *Being There: Putting Brain, Body and World Together Again* (Clark, 1997), Andy Clark championed this view, arguing that minds are not disembodied, atemporal, filing cabinets that reason; they are not even isolated brains in a vat . . . or a skull. Instead, minds are complex, deeply contextual and embodied systems of brains (central nervous systems, actually) coupled to the rest of the body and the real-world environment in which they are situated and on which they act.

Already in the earlier book Clark had argued (1997, p. 66) that the world functions as more than just external memory for the human brain; certain operations such as tool use systematically *transform* the problems posed to individual brains such that a qualitative ratcheting up results. It is this feature of the complex adaptive system that is the human mind that is the focus of Clark's latest book, *Natural-Born Cyborgs*. In his new book, Clark's goal is to show that the real human mind is already (and always has been) a biotechnological matrix of brain *and* a myriad of non-biological props, scaffolding, instruments and resources such as pen and paper, words, diagrams and tables, cell phones, electronic files, and the like. Clark intends to co-opt the term cyborg away from its often-frightening science fiction connotations to show that this ability to import the world into its operation is precisely the human brain's central feature and indeed, what make humans human.

Fully crediting earlier work such as Merlin Donald's *The Origins of the Modern Mind* (Donald, 1991), Clark focuses on the historical procession that began with language and printing and lead to the more recent digital encoding of text, sound and image. These external scaffolds do far more than merely allow for the external storage and transmission of ideas. They constitute a cascade of 'mindware upgrades': ''cognitive upheavals in which the effective architecture of the human mind is altered and transformed'' (p. 4). We have imported those props and tools into our mental apparatus such that it is now something qualitatively different than what it was before. Human minds *are* that biotechnological matrix all the way down. Minds are

therefore *extended* entities (Clark and Chalmers, 1998) that are not coterminous with the body's 'skinbag'. Although specific thoughts remain tied to individual brains, earlier notions of mind as a central planner that bases its decisions on an on-board data bank of Platonic Form-like representations must therefore be discarded and replaced by the notion of mind as associative pattern recognition device that operates by scaffolding itself onto the external environment. "The biological brain participates in potent and iterated loops through the cognitive technological environment. These loops can now be seen to consist, in many cases, in the use of the stable external environment as a source of complementary capacities to those provided by the biological brain" (p. 75). As a result, the brain's own activity "is deeply informed by the specific properties of the external media" (p. 76).

The main obstacle to the claim that minds are soft assembled, self-organized systems of processes characterized by feedback loops that criss-cross the organism and its environment is the Western conceptual framework for which the furniture of the world is just that: static objects with rigid boundaries. With the view that the mind and its self are *soft assembled,* a "control-sharing coalition of processes" (p. 138), the need for a process philosophy becomes pressing. But since Heraclitus (with the exception of the oft-unintelligible neologisms of Alfred North Whitehead), a metaphysics that takes seriously processes exhibiting distributed control has, by and large, been absent from Western ontology.

As evidence that the boundary of the self has never been rigid, Clark refers us to V.S. Ramachandran's experiments, none of which rely on technological prostheses, which show that it is possible to make participants feel that their nose is two feet long, or that the desktop is a part of them. If such experiments establish that our sense of bodily bounds and even our self is a *construct* based on the *perceived correlations* between observations and felt sensations, then the use of artificial prostheses to produce *telepresence* would not be a qualitative change, only the latest addition to what has always been a continuum. Once one begins adding technological prostheses, and in particular so-called *teleoperation* systems that provide the user with a wide spectrum of sensory feedback, the potential to produce genuine *telepresence* appears.

Because of his emphasis on the *transformative* potential of this "ability to enter into deep and complex relationships with nonbiological constructs, props, and aids", Clark recognizes that the real

mind–body problem is really the mind–body-*scaffolding* problem: how *do* feedback loops between brains, bodies and context result in a qualitatively distinct, causally effective entity, the human mind? No non-linear dynamical system is just dropped into an environment that then affects it as an old-time mechanistic cause. (This is what behaviourism thought, wrongly.) Feedback loops to and from the environment embed such systems in their surroundings and in their history in such a way as to make them essentially contextual and historical entities. Clark rightly notes, accordingly, that the "flow of reason and the informational transformations that criss-cross brain and world" (p. 69) must be integrated in such a way as to cohere as a *mind*. But what kind of causality enables feedback loops to produce a phase change that results in a higher-level integrated phenomenon? Recent books such as *Sync*, *Linked*, and others have begun to address this question, but not directly from a philosophical perspective. Explaining how feedback loops into and out of the central nervous system self-assemble into a coherent but levels-organized whole with emergent properties is now philosophy of mind's next big challenge, along with a more sophisticated conceptual analysis of the type of causality involved.

For such integration to occur, containment within a physical boundary is, Clark argues, less important than the coherence, transparency, and seamlessness of *information flow*. If I can control an arm directly as a basic action (such as occurs in healthy subjects when they will to move their arm), then it is plausible to claim it as part of me, whether artificial or not. To become a part of the agent (read, of his/her mind and self), external scaffolds must become *transparent*, and for artificial technology to become as transparent as, say, language use, extensive and multi-sense feedback will be required. Once in place, the *location* of the data on which the construction is based will be seen to be irrelevant. All that matters is that the data be available for immediate use (in a basic act kind of way). "What *matters* are the complex feedback loops that connect action-commands, bodily motions, environmental effects, and multisensory perceptual inputs. . . The biological skin-bag has no special significance here. It is the flow that counts" (p. 114).

Clark notes the case of Australian performance artist, Stelarc, who, in addition to his normal two arms, has a third arm in the form of an electronic prosthesis that is controlled via EMG signals detected by electrodes placed on his legs and abdomen (p. 115). Stelarc reports that he "does not feel that he 'operates' the third hand. Instead he

simply uses it as he does the other two''. In other words, Stelarc does not move his arm and hand *by* doing something else: in the language of action theorists, he moves his arm directly, as a *basic act*. Miguel Nicolelis' dramatic neurobiological experiments on macaques recently published in a new journal with free online access at Carmena et al, (2003) suggest that this ability may extend to monkeys, not just human beings. In those experiments, monkeys with electrodes implanted in areas of the brain that control arm motion learn that by ''thinking through the motion'' they can move a robot arm much as if they were moving their own arm.

As another dramatic illustration of transparent information flow, Clark describes the work of the team led by Roy Bakay of Emory University in Atlanta, Georgia. Two glass cones were implanted into a paralyzed stroke victim's motor cortex. The cones were''coated with special neurotrophic chemicals . . . which prompt nerve growth, [thus helping] the cortical neurons grow into the glass cones and then attach themselves to small electrodes inside. The implants transmit signals to an amplifier (worn as a cap) which relays them to a computer. The patient, with effort and practice, is able to use the neural signals to control simple cursor movements [on the computer screen]'' (pp. 121–122). But, and here's the key point, the patient does not just ''think, 'cursor, right.' Instead it is like willing a body part to move''. Information flow is transparent whenever the behaviour is performed as a *basic act*.

Still, how does the higher order neural pattern *mean* the command ''move arm!''? Clark insists that neural patterns do not mean anything intrinsically; they count as meaning ''move that arm'', say, only because they bring about *just* that kind of result (p. 123). And he describes the higher order command/intention 'move arm' as *cascading* downwards to lower level neural processes. Clark rightly insists that most of the time our intentional acts do not issue from a conscious process of deliberation, one which culminates in an act of will: most of the time, once a high level decision is made – go to store for milk – we just let the world make the lower level decisions for us (go in car versus go by bus, say). But the thorny issue of semantics (or qualia) cannot be skirted indefinitely: how are semantics embodied in a neural network such that, qua meaningful, they exert causal power? How is it that the high-level decision – go to store for milk – *modulates* the cascading information flow such that the behavioural outcome satisfies the intent's meaningful command (while at the same time allowing the world to handle the details)? Ever a philosophical

problem, even before the addition of technological prosthesis, how is the concept of responsible agency to be fleshed out when these new devices are added to the equation? Why makes *this* particular neural network *me* such that behaviour that issues from that network counts as *my* action?

Natural-Born Cyborgs proves once again that Clark is among the most imaginative, creative thinkers around, and a joy to read, especially with Susan Greenfield's much more appalled interpretation of the same phenomena as counterpoint (Greenfield, 2001). My only complaint about this provocative book is about the careless editing: the Index contains numerous errors (the entry for Merlin Donald refers, for example, to pp. 37 and 38, which contain references to Donald Norman); 90% of the Index consists of proper nouns only (requiring the reader to leaf through the entire book to locate discussions about particular topics); and all bibliographic information is incorporated into a Notes section at the back of the book, instead of as a stand alone Bibliography.

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The key point is stated early and often in *Natural-Born Cyborgs*: humans are a matted tangle of technologically interwoven orderings of perception, movement, memory and planning. Andy Clark writes (pp. 32–33):

> My claim ... is that various kinds of deep human–machine symbiosis really do expand and alter the shape of the psychological processes that make us who we are ... The moral, for now, is simply that this process of fitting, tailoring, and factoring in leads to the creation of extended computational and mental organizations: reasoning and thinking systems distributed across brain, body, and world.

A radical re-evaluation of our sense of self *does* lie at the core of this book. To understand self as a contingent patchwork of systems of perception and movement, held together by borrowing support from relatively stable non-living assemblages, is potentially radical. Think of it: plans, actions, our sense of time, memory, our awareness of self

and other, perhaps any idea of a world as such would slide into a drifting mist, a semi-waking state, life by fits and starts, if not for the scaffolding of relatively more durable yet plastic internal and external orderings and supports on which cognition constantly unfolds and props itself up. The orderings can materialise in practices of speaking and listening as well as in artefacts such as tools or built environments. Clark's argument centres on contemporary technologies because they are often represented in public discussion and popular culture as a threat to who we are (such as Prince Charles' recent cry of alarm that nanotechnology might turn the world into 'a grey slime'). He wants to counter that threat by showing it is founded on a misapprehension of self. The threat appears in different guises: sometimes as a literal takeover or colonisation by machines which render some aspect of human intelligence redundant, sometimes as a subtle and even intimate permeation of our relations to each other, of memory or of perception that blinkers us in certain ways (as in Michael Crichton's recent novel, *Prey*). These fears about technology are not new, but Clark's take on them is somewhat novel. He re-interprets them as stemming from an ignorance of the distribution of human intelligence across all boundaries between self and world, mind and body. In other words, the image of a malignant Borgish symbiosis with machines is a misunderstanding – the very possibility of self, however it has been experienced, depends on a panoply of interlocking, non-unified, material assemblages.

There are several senses in which *Natural-Born Cyborgs* tends, however, to domesticate the figure of the cyborg as it has developed over the last two decades in cultural and feminist studies of science and technology. Although cyborgs have become mainstream in many ways, they still have the potential to trigger shifts in our sense of self and world. I wonder whether *Natural-Born Cyborgs* neutralises some of those potentials.

Is it a Question of Opacity Versus Transparency?

A key contrast between opacity and transparency runs through much of the argument in the book. The contrast describes different ways in which technologies can present themselves as either obstacles that frustrate our purposes or as something with which we merge and augment perception, action or imagination. The most

profound human–machine mergers do not have to literally penetrate the skin, Clark argues. Skin or the 'ancient biological skin-bag' is basically a distraction. Profound mergers are ones that become invisible or transparent in a different way: they dovetail or interlock with specific cognitive attributes or capacities of the human organism. Cold War high-tech machinery with its flashing lights and dials is *opaque* because it ''keeps tripping users up . . . and thus remains the focus of attention even during routine problem-solving activities'' (p. 37). Bad technologies, of which PCs are *the* contemporary example when they lock up, crash, or reveal some hitherto unknown complication, are not 'human-centred' because they resist the merger. Good transparent technologies disappear in use. A phone or book are technologies that have become more or less transparent because we don't have to think how to use them. Many studies of human–computer interaction, and in general, ethnomethodologically-influenced research into technologies, treat this distinction as axiomatic. (The distinction was, as Clark acknowledges, proposed by the human-computer interface expert, Donald Norman.) Clark develops the distinction further by providing a much deeper theoretical framework.

Is the distinction between opacity and transparency tenable? There are several difficulties worth noting here. For any particular technology, we need to ask: opaque or transparent for whom? The Cold War military technologies that Clark finds attractively opaque may not have been opaque for the technicians and engineers who worked with them. Yet there may have been good reasons for them to be made opaque for everyone else. So too PCs (Intel-based, not Apple!) are not opaque for everyone. The example that Clark uses in a later chapter of Linux, the 'open source' operating system, is the paradigmatic contemporary case of hypercomplex technical opacity being transformed into an object of intense collective interest for a certain cultural-technical niche. Technical subcultures associated with Linux keep their technical objects – a generic, commodity computing platform – at the centre of attention in various ways ranging from hardware modifications through to constant re-figuring of the 'look and feel' of the graphic user interface. Transparency and opaqueness are not intrinsic to the technology. They relate to social, cultural, political and economic projects in which zones of opacity or transparency serve different functions. A relative opacity can be extremely useful in regulating who gets to play around with and alter the technology.

Apple computers are a case in point. The Apple computers that Clark mentions he uses were advertised globally a few years ago using a television and print campaign that went simply by the name 'Switch'. No computers were shown in the ads, only people talking about how easy Apple computers are to use. Most of the ads used a simple white background. Apple Corporation signified the transparency of their machines partly by keeping the computers outside the ads. Transparency is a design goal that Apple computers may reach more successfully than their competitors' machines. They represented that transparency almost literally through the white background. Yet that transparency costs a lot financially and culturally: Apple's machines are virtually blackboxes. The company has come close to financial disaster because of the full proprietary control Apple exercises over what goes into the box. The seething market for upgrades, modifications, substitutions and conversions that characterises the more generic, messy world of Intel/AMD-based PCs has never really developed around Apple. Apple, through its superior design and style values, makes technical objects that are culturally stable and that freeze the relation between producer and consumer in certain important ways. Again, the question around Apple would be: transparent for whom?

The contingency of transparency can be illuminated from a different angle through two examples given early in *Natural-Born Cyborgs* – those of Lolo the microchipped cat, and the comic book topless dancer. The cat

> Lolo is, by all accounts, a disappointing cyborg. He incorporates a nonbiological component, conveniently placed within the relatively tamper-proof confines of the biological skin (and fur) bag . . . It would make no difference to *this* intuition, surely, were we to implant the bar code chip as deeply as we like. (p. 22)

Is Lolo a disappointing cyborg by all accounts? Penetration of the body, as Clark argues, is not the defining feature of a cyborg (although I would be extremely wary about dismissing it as quickly as Clark does: skin is heavily coded and experienced as corporeal boundary; if we deny significance to skin we have no way of making sense of the many different skin-related images and practices of contemporary cultures). The issue here is also not transparency, because Lolo's chip is, I agree, pretty transparent to Lolo. Indeed, Lolo's world – a territory littered with paths, signals, patrol schedules and observation points, hunt and chase manoeuvres, repeated encounters with other animals, human or non-human, his rhythms of

sleep and wakefulness, the rapid transitions between napping, grooming and snacking – is not tangibly altered or augmented by the chip. Lolo may be a cyborg, but his circuits do not run, in the main, through that chip lodged somewhere around his shoulders. The chip is so transparent to his milieu that he does not care about it. Only within the world of pet protection and animal welfare organisations does it form a link that may be vital. A magnetically-activated cat-flap, on the other hand, which lets Lolo into the house but keeps other cats out is definitely an affordance Lolo knows how to use as he bolts in late at night, neighbouring tomcat hot on his tail.

By contrast, the comic-strip image of a topless dancer with a barcode tattoo across her breasts is definitely more disturbing to us. Why is it more disturbing than the hypodermic implant of a chip? If the image is not disappointing as a cyborg by contrast with Lolo, it is because the tattoo suggests that sex working has become caught up in world of online transactions, supply chain management, computerised inventory and business-to-business communications, all entwined with the hardware of bar-code scanners, networks, and databases. In other words, her body would become transparently available as an object of transactions, just like a bag of crisps or six pack of Guinness beer on the shelves of the local supermarket. ''The deeply transformed kind of human existence'' (p. 23) which the bar-code suggests also perhaps reminds us of past occasions – the camps – when human skin has been tattooed with codes and numbers.

What makes images of technology powerful? The image of the dancer was important enough to have it reproduced in the book. An image of technology can signify a mixture of transparencies and opacities that may not be intrinsic to the technological artefact itself. Yet those transparencies and opacities are indissociable from it. The mixture matters. It structures and de-stabilises the processes of symbiosis and augmentation. Images of technology show that the transparency and opacity, affordance and frustration associated with a specific technology come from unstable and fluctuating surges of social, cultural, political, personal, economic, and sexual power relations materialising in technological form. This might seem like a highly 'cultural' response to the transparency–opacity contrast, but it seems impossible to me to discount its relevance in any revaluation of anxieties concerning contemporary technology. At every level of the human–machine symbiosis, we need to ask: transparent for whom? Opaque for whom?

Is There Anything non-technological?

In important respects, *Natural-Born Cyborgs* is less concerned with the relation between mind and world than with using the notion of a distributed, scaffolded self to palliate anxieties about what technology is doing to us. The palliative takes the form of a vision of transparent technologies "apt for the most profound and enduring kinds of interweaving into our lives, identities and projects, and into our constantly constructed sense of place, presence, and self" (p. 58). Consistently, the stress falls on the 'interweaving' or 'dovetailing' of technologies and cognitions. An important condition of possibility for any of this interweaving or dovetailing (which, thankfully, is not done once and for all but continually iterated and varied) lies in the plasticity and hybridity of the human organism. A pleasurable virtue of this book is to show that wherever we look for specific human attributes, whether it is human vision, memory, learning, reason, or language, we find unexpected and contingent couplings of heterogeneous internal and external subsystems rather than fixed functions exercised by particular organs.

But in saying all that, quite a high cost must be paid. Throughout *Natural-Born Cyborgs*, an almost universalist 'Swiss Army knife' concept of technology operates at multiple levels. Not only is Clark describing contemporary technologies such as ubiquitous, mobile, or wearable computing, he is describing *everything* technologically. So pervasive and continuous is this mode of speaking in the book that it would be easy to cite dozens of examples. For instance, see the grey-boxed text titled 'The Early Adopter's Dream Technology' (p. 80), in which language is described as a technology: "a fully portable, shareable resource, . . . this new piece of kit was, in fact, so simple that even a child could use it". Moving downwards from language, a "technology [that] changed us beyond recognition" (p. 80), a reader would be hard-pressed to find a single instance where the phenomenon, object, cognition, thought, or event under analysis was not framed as a technical process or system of some kind: sight, writing, sense of place and time, self as the sum of what I control (p. 130), Tools-R-Us (p. 137).

Is there any limit to the concept of technology being relied on here? If everything can be understood as a technology, device or machinery – language, vision, reason, particular parts of the brain – then in what sense is anything *not* technology? Is technology not a

new monism? The broader question here is whether there is any possibility of thinking outside technology. Put in catty terms, aren't we in Lolo's position here? Once everything is technological through and through, once there are tools, devices, systems, interfaces, etc. all the way down ("it is just tools all the way down" (p. 136)) and wherever we look, then do we become a 'disappointing cyborg' too? The implant is deep, but it does not matter all that much for most purposes.

Admittedly, this extension of the technical operation is needed to lever us away from the naturalised inside–outside split that has locked down our main concepts of self. The lack of any sharp ontological distinction between the differently situated technical operations, some situated in the environment, some involving neuromuscular habits, means that 'tasks' can be 'farmed out' (p. 103) in many interesting ways. But does the continuum of technical operations, some infrastructural, some intercorporeal, some deeply wired in the organism, end up becoming totally undifferentiated? I think it tends to. This both poses a problem and points the way for future work.

In principle, everything can be thought of as a technology. Significantly, however, we do not actually treat everything as a technology. The continuum of technical operations and objects is deeply fissured by social, cultural, and material processes. In practice, certain kinds of objects count as technology and others do not. The processes whereby some things appear as technology and others do not are powerful. They deeply shape lives, institutions, politics and imaginings. (For instance, try to imagine the organisation of a contemporary university in the absence of an idea of 'Technology'). Technology, despite its appeal as a general name for how we do things, is a heavily freighted term. Showing that almost anything can be understood as a distributed technical operation does not really help show how the term technology comes to be so loaded.

As to future work to shed the burden of an undifferentiated technological continuum, one possibility is already signalled in the book, but is left dangling. Clarke points out that there are relevant differences between neurons and a telepresence system "*only insofar as* they affect the timing, flow and density of informational exchange" (p. 103). The italicised "only insofar" is unfortunate. It makes "timing, flow and density" seem like relatively minor technical issues. The attention to timing could in fact be cranked up significantly as a way of extending the reach of the technical without levelling out

differences between technical operations. Perhaps 'timing is everything'. Rather than an undifferentiated spectrum of technical operations, questions of timing make a difference between transparency and opacity, between feelings of being there or feeling shut off (as in the case of telepresence technologies). Another symptom of a growing awareness of timing appears in post-genomic biology where models of information flow sensitive to the developmental rhythms of organisms have become a research hot-spot (hence proteomics, glycomics, and so on).

Is Usability All We Want?

Ironically, despite its generous reliance on the technical as defining human specificity, *Natural-Born Cyborgs* sometimes runs the risk of a narrow-aperture conception of technology. The focus consistently rests on ICTs and their usability. The advocacy of user-friendly consumer electronics, mobile communication devices and www-based technologies propels the discussion along certain tracks that are both reassuringly familiar (from Nokia, Sony or Apple ads?), and yet hard to put the brakes on without sounding impractical or obstructive. How could we object to the value of usable, robust systems that support valued projects? Nevertheless, it may be that we should question the value of usability and robustness. Usable for what or for whom? Much artistic response to new technologies has critically reflected on the goals of usability and their limitations. 'Usability' and 'the user' are not neutral terms. They carry implicit understandings of self, action and world with them.

Even in a wide-ranging discussion in the second last chapter of the particular fears and anxieties associated with contemporary technologies (overload, narrowing, alienation, uncontrollability, and more), *Natural-Born Cyborgs* surprisingly leaves the obvious question of biotechnology – as in GM crops, new reproductive technologies, biopharmaceuticals, stem-cells, tissue-engineering – aside. Two questions concerning biotechnology come to mind. Why have these technologies been left out of the 'future of human intelligence'? And where would such technologies fit in the benign vision that Clark offers? He suggests that we need a more 'biological' relationship with our technologies (p. 176):

What matters is not that we be micromanaging every detail of every operation, but that the surrounding systems provide usable, robust support for the kinds of life and projects we value. This is *precisely* the goal of human-centered technologies anyway. The trick, then, is to acclimatize ourselves to a much more *biological* relationship with our technologies.

Certainly, 'micromanaging' sounds irksome and I have sympathy with the hope for a 'more biological' relationship with our technologies, but the question is: which biology? Biology is not a unified field, as the many disputes and struggles between different domains – ecology, ethology, developmental biology, population genetics, molecular genetics, physiology, and so on – attest. Some of the struggles between different biologies actually concern the potentials and limits of technological control (for instance, the debate over the extent of cross-pollination between genetically modified crops and weeds brings ecology into conflict with molecular biology). Moreover, much biological research is increasingly enmeshed with technological innovation. On this score, the hope of a more biological relationship does not actually simplify the matter any more than the plea for transparent technologies. Biology already harbours all the same ambiguities and conflicts Clark hopes to set to rest. So biology is no salvation.

And why the omission of biotechnology more generally from the future of human intelligence? It could be that biotechnologies will acclimatise us to a *more biological* yet *opaque* relationship to our technologies because we will be eating, absorbing, metabolising, and having them grafted into us at a depth which destabilises our sense of self ('germ-line modification'). But all this is currently a matter of major cultural, political, and economic contestation. Perhaps we will need to acclimatise ourselves to a much more technological relationship to our biologies through medical, pharmaceutical and food biotechnologies. But leaving all that aside, there is certainly no risk that we will be micromanaging every detail of these technologies, because they fall outside the scope of individual control. Only through a network of relations with genetic counsellors, doctors, geneticists, health insurance companies, families and friends can the decision to select an embyro free of certain genetic disorders for implantation (the technique of Preimplantation Genetic Diagnosis-PGD) be effectively taken and put into action. Guided by *Natural-Born Cyborgs,* our position with respect to technologies such as PGD would be much more like Lolo and his implant chip than it is like our relation to a PC or PDA. I think *Natural-Born Cyborgs* leaves

biotechnology in the same position as Lolo's ID implant chip. It's there, inside the fur-bag, but it feels hard to interact with it. Perhaps today, only when we turn to biotechnologies does the question of usability and user open up again and become less transparent. Another question for future work would be whether the scaffolding-of-cognition arguments can be translated into these less device-based fields.

Is a Feeling for Technology Spurious?

The topic of minds and technologies becomes much more difficult to negotiate if the technologies in question already interact intimately with our biology in ways that we simply cannot calculate. We do not actually need to resort to biotechnology to raise these questions. In general, intelligence in *Natural-Born Cyborgs* is only incidentally emotional or affective. The sophisticated arguments that wash away boundaries between self and world, mind and body, brain and environment, leave feeling, affect and emotion intact. While the problems of acting in the world draw body and world together in highly concrete arrangements in which perception is no longer reception but active movement, feeling, affects and emotions remain mired in receptivity and passivity. Yet how do we account for something like an eroticisation of technology (p. 37) or, for that matter, an idealisation of technology as a structure of feeling? Do the concepts of negotiable body-image, language as floating-root structure or mangrove, distributed computational and mental organisations, or tools all the way down incorporate an affective component? Throughout the book, Clark works to allay anxieties and fears about contemporary technology on the assumption that if we change our understanding of what really happens when we remember, talk, judge, decide, or even buy some beer, then our misgivings about constantly increased level of interactivity with ICTs could change. What if, however, feelings of pleasure, interest, disgust, fear, excitement, horror, or wonder do not simply come from misunderstandings about the boundaries of self? Could the distribution of intelligence argument be turned around and applied to affect too? What if the shape-shifting capacities of the mind, its capacity to hybridise itself with stuff, is not restricted to intelligence conceived cognitively? This too suggests directions for future work.

Conclusion

I have no problem with accepting the profound mergers of the human brain with 'symbols, culture and technology' (p. 197). That resonates strongly. But sometimes in reading *Natural-Born Cyborgs* I was disappointed by the figure of the cyborg drawn there and what it could do. The pleasures of the cyborg, as Donna Haraway has famously argued, involves some illicit boundary crossings. I did not feel much of that here. The pain of the cyborg, that too matters. In some ways, although there is no room to develop it here, I sense in Clark's book a kind of pre-dotcom crash ebullience about technology. The lag or delay in social and political institutions in relation to technology that it sometimes complains of, seems to me to have affected the book itself. Its figure of the cyborg is in some ways an older one. No problem with older cyborgs: they are often less mean than the newer ones, as the *Terminator* series showed. But as they age, one looks for some more depth. In scrutinising the work done by the contrasts between opacity and tranparency, the generalisation of the technical operation, the narrowing of the scope of technologies to ICT usability, and damping down of affective intelligence, I think Clark leaves us a little 'unscaffolded' on the shifting ground of technology and culture.

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By Steven Mithen

Having read and thoroughly enjoyed Andy Clark's book I am quite convinced that all people alive today are indeed 'natural-born cyborgs'. The question that remains for me, as a prehistoric archaeologist, is whether this is because we are all *Homo sapiens* or because we are members of the *Homo* genus? While Clark subtitles his book *Minds, Technologies and the Future of Human Intelligence*, it raises as important questions about the past as the future – it is a book that I will be encouraging all my archaeology colleagues and students to read. So when Clark begins by writing that his book ''ought to start, perhaps, somewhere on the dusty ancestral savanna'' (p. 8) I wholeheartedly agreed, with no 'perhaps' about it.

The fact that he did not begin on the savanna left many of the most interesting questions (for me) unaddressed. For instance, while Clark can easily claim that modern humans are natural-born cyborgs and that his cat (and presumably all cats) are not, I would have liked to know whether he thought that tool-wielding chimpanzees are 'natural-born cyborgs': do their uses of termite sticks and hammerstones qualify as "deep and complex relationships with non-biological constructs, props, and aids" (p. 5). If not (as I assume his answer would be no), then when did those type of relationships begin in human evolution? I found Clark's use of the term 'human nature' a little broad too: did he mean all *Homo sapiens*, all large brained hominids from, say, *H. heidelbergensis* onwards, all members of *Homo*? In other words what would he have found if he had started his book on the "dusty ancestral savanna", and then tracked human evolution through the droughts and freezing temperatures of the ice ages, through the development of farming and into the modern world? When did the "deep and complex relationships with non-biological constructs, props, and aids" first appear?

It was not, of course, Clark's intention to address such questions and one of the delights of his book is how it is so well focused on the descriptions of recent and future technological developments, many of which I was quite unaware. My point is simply that Clark raises many questions that archaeologists and others concerned with the evolution of the human mind should address and which we would like philosophers, psychologists and others also to comment upon. So rather than providing a formal review of Clark's book (which would amount to little more than eulogising a great read about profoundly important ideas) I want to take this opportunity to reflect on when 'natural born cyborgs' may have arisen in human evolution and use this to comment on some of the ideas within the book.

It was no more than a few months after the publication of my study of cognitive evolution, *The Prehistory of the Mind* (Mithen, 1996), that I realised my ideas had a fatal flaw – or at least a significant omission. That realisation largely arose from reading Clark's 1997 book, *Being There*, and was then consolidated by his 1998 article in *Analysis* with David Chalmers called 'The Extended Mind'. The mistake I had made was to assume that the development of human culture during the last 150,000 years was simply a product of the evolving human mind: I failed to appreciate how the mind was as

much a product of human culture as culture was a product of the mind. Once appreciated, this remarkably simple idea changes the entire manner in which one thinks about cognitive and cultural evolution. It also takes us back to the writings and thought of an earlier generation: in 1936 Gordon Childe, the most distinguished archaeologist of his age, published a short book about human prehistory entitled *Man Makes Himself* – a title that could feasibly have been used by Clark himself (with suitable amendments for political correctness).

The reason this idea is of such potential interest for archaeology is that it may help explain the period of rapid development in human culture that has been termed the 'symbolic' or 'Upper Palaeolithic' revolution at 60,000–30,000 years ago. It is during this time period that we see the first appearance of art, ritual and significant developments in technology. It also broadly coincides with the colonisation of Australia and Europe by *Homo sapiens*, after the species evolved in Africa at least 150,000 years ago and seeming to have lived without any symbolic activity until the first traces of this appear 70,000 years ago.

Archaeologists have debated the cause of the Upper Palaeolithic cultural developments for many years. Some have argued that there was a genetic mutation that led to new brain circuitry, which then provided the capacity for creative thought. Others have argued that this is when language was 'invented', or when populations crossed a demographic threshold so that cultural transmission became sufficiently prevalent to create a ratchet effect for human culture. My own (1996) argument was that this was when 'cognitive fluidity' emerged – the capacity to integrate ways of thought and items of knowledge previously restricted to isolated cognitive domains. Clark's work has suggested another idea – that this is the time at which humans learnt what Daniel Dennett would call a clever trick – they learnt to use human culture to extend their minds and for the first time in human history became 'natural born cyborgs'.

When reading Clark's latest book I kept finding that his descriptions of modern technology seemed entirely appropriate for that of human prehistory after the 'cultural explosion'. So when he writes about ''augmented technology'' in which ''information might appear attached to the space around an individual'' (p. 53), I cannot help but read this as an interpretation of the ice age painted caves in France and Spain in which the images of bison, horse and supernatural beings added ''new layers of meaning and functionality to the daily

world itself . . . a kind of deliberate blurring of the boundaries between physical and informational space''. Indeed we can see the first appearance of art, and especially that of supernatural beings such as the lion-man of Hohenstadel, a carved ivory figure with a man's torso and a lion's head, as 'freezing a thought' not in words but as a material object which then opens up what Clark calls ''second-order cognitive dynamics'' (p. 79) and what I would refer to as 'cognitive fluidity'. The development of needles to make clothing, the creation of the first maps, projectiles and architecture, could all be encompassed in the interpretative framework for technology that Clark provides.

Could it be the case that those previous generations of modern humans – those who lived prior to the Upper Palaeolithic without making art and seeming to lack any cultural innovation – had such thoughts about supernatural beings, but did not have the 'clever trick' of 'freezing' them in stone, ivory or bone to then enable thoughts about thoughts? Perhaps this was also true for pre-modern humans such as the Neanderthals, *Homo heidelbergensis* and *H. erectus*. It seems unquestionable that such pre-modern humans had spoken language in light of the evidence for when the modern vocal tract evolved – at least by 500,000 years ago. But words are transient and of far less consequence than material culture for 'freezing', or in my own terminology 'anchoring' thoughts so that they can be transmitted to others, recalled, manipulated, and used to scaffold more complex thoughts.

Clark does not refer to this key period of human prehistory within his volume and is prone to conflate the origins of speech and written text to make them sound as if they are effectively the same event in prehistory, such as when he refers to speech and texts as providing the ''first phase'' of our cyborg existence (p. 81). We can, however, use these along with the origin of art and of sedentism (see Point 3 below), to identify four stages in the development of the cyborgmind:

1. The origin of language. If pre-modern humans had a linguistic ability by 500,000 years ago (and we could, of course, debate both what we mean by a 'linguistic ability' and the fossil evidence at length), then the sharing of thoughts and the development of a collective mind would have been present: ideas would have arisen that went far beyond the capacity of one mind alone to generate. This may relate to developments such as big game hunting and Levallois technology that are evident at or soon after this time. But, *contra* Clark (p. 83), spoken language did not appear to start a ''cognitive

snowball rolling'' – that appears not to have occurred until a mere 50,000 years ago.

2. The origin of art. The 'invention' of art (a term which could also be debated at length) at some time after 100,000 BC may have provided the means by which thoughts could be frozen/anchored for longer periods of time either on cave walls or in carvings and made available to those not in the immediate vicinity of the spoken word, including those of later generations. This was perhaps the most fundamental change in the nature of being human, the time when humans really began to manipulate and augment their minds – a time when culture became as important as biology for human thought. I would identify this as Clark's ''one large jump or discontinuity in human cognitive evolution'' (p. 78). It was with the origin of art rather than speech or written text that ''the floodgates of self-reflective reason'' were opened.

3. The origin of sedentism. Living in permanent dwellings and settlements was a consequence of farming that developed soon after 10,000 BC, although in some regions of the world sedentary hunter—gatherers developed during the late Pleistocene and early Holocene prior to the invention of farming. The material culture consequences of sedentism are enormous as it allows the accumulation of possessions and the development of monumental architecture. Hence for the first time people permanently surrounded themselves on a day-to-day basis with the means to augment their own thoughts: for the first time human brains became permanently engaged with an all-pervasive cultural world. The most significant impact of this may have been on the manner in which brains grew and developed during child development that, as Clark acknowledges (p. 84), ''structures the brain in quite deep and profound ways''.

4. The origin of writing. The first writing appears at around 3000 BC in Mesopotamia, and would have provided another critical threshold in developing the modern mind by combining the power of language and material culture in one single package.

What of the period prior to the origins of spoken language and art? Fossil evidence suggests that the 'long human childhood' is likely to have appeared with *H. ergaster* at c. 1.8 million years ago in light of significant increases in body size, brain size and the development of full bipedalism (which conspired to result in the birth of infants which were essentially still in a foetal state). Clark refers to the work of Griffiths and Stotz who argued that the long human childhood provides a unique opportunity in which ''cultural scaffolding [can]

change the dynamics of the cognitive system in a way that opens up new cognitive possibilities'' (p. 85). But it is difficult to find significant changes in any aspects of human culture at this date, unless we see the dispersal of *H. ergaster* out of Africa as a direct consequence of 'new cognitive possibilities'.

A significant development in stone tool technology had occurred by 1.4 million years ago with the appearance of hand-axes. These are pear-shaped, symmetrical tools made by the bifacial flaking of nodules or large stone flakes. These remained a pervasive feature of the archaeological record for more than a million years, being made by several different types of human species. Such hand-axes were used for butchering animals and it is difficult to conceive that they were anything other than a seamless extension of the human body. Pre-modern humans most likely used their hand-axes in much the same way that Clark describes a modern human using a hammer – the tool keeps ''flipping between invisibility-in-use and availability for thought and introspection'' (p. 48).

Most archaeologists accept that by the time of hand-axe technology, and most probably by that of the Oldowan prior to two million years ago, our human ancestors were dependent upon material artefacts for survival. This contrasts with the use of termite sticks and hammer stones by chimpanzees that appear to merely supplement rather than direct their foraging activity. And hence it seems likely that the ''well-developed intuitions about physical objects'' (p. 49) evolved in the earliest stages of human evolution at some time between six million and two million years ago. As Clark so effectively explains, such intuitions seem to be playing as crucial a role with regard to the form of the latest technology as they did with that of the stone age.

In summary, had Clark begun his book on the ''dusty ancestral savanna'' I suspect he would have found members of the *Homo* genus who were natural-born cyborgs with regard to their bodies but not to their minds. They would have been using stones, sticks, bones and horn cores to extend their physical abilities in a manner that was, at times, quite 'invisible' to them, in a manner not significantly different to how I use a hammer today or even a word processor. But their minds were still entirely within their skulls and it would appear inappropriate to describe them as having ''deep and complex relationships with non-biological constructs, props, and aids''. It was not until relatively late in human evolution that mind began to become something different from the brain, first by the evolution of spoken

language, and then by art, sedentism and writing. I remain unsure whether the recent and future technological developments that Clark describes in his book mark a new phase of cognitive evolution or merely a more explicit manifestation of the cyborg-mind that began evolving in prehistory.

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Author's Response

By Andy Clark

Thought happens. Here I sit, sipping coffee, scribbling on paper, accessing files, reading and re-reading those four wonderful, challenging, yet immaculately constructive reviews. And somewhere, and to my eternal surprise, thought happens. But where, amidst the whirl of organisation, should we locate the cognitive process? One possibility is that everything worth counting as (all or part) of any genuinely cognitive process hereabouts is firmly located inside the head, safe behind the ancient fortress of skin and skull. All the rest, according to this surgically neat view, is scene setting: preparing and maintaining the pitch upon which the great thinking organ performs.

Richard Feynman may well have disagreed. Upset by an interlocutor's remark that his extensive notes and scribblings were merely the record of his work, he acidly replied:

> No, it's not a *record*, not really. It's *working*. You have to work on paper and this is the paper, ok?

Natural-Born Cyborgs was an extended meditation on this simple theme, leavened (as the title suggests) with a measured dose of techno-futurism. The human mind, I wanted to argue, is naturally designed so as to co-opt a mounting cascade of extra-neural elements as (quite literally) parts of extended and distributed cognitive processes. Moreover (and hence the techno-futurism) this ancient trick looks poised for some new and potent manifestations, fueled by innovative work on human-machine interfaces, swarm intelligence, and bio-technological union.

Let's start, then, by re-visiting that opening gambit. Thought happens. But how?

Skin and Out

Terry Dartnall, in his engaging and inquisitive commentary, seems attracted to the idea that the inner is in some way special and that I make it seem less so only by perpetrating a subtle (or not so subtle) state/content confusion. Were he to confront Richard Feynman on the matter of paper trails and thought, the conversation might go like this:

> Dartnall: Your appeal to a notion of *working* is systematically ambiguous between a claim about the externalisation of cognitive content (true but trite) and one about the externalization of cognitive states (interesting but false). The paper trail is a record of the *contents* of your cognitive states. But the *states themselves* never got outside your head.
>
> Feynman: I beg to differ. My thinking itself involved those markings with pen and paper. The loop into paper is far more than an ongoing record of the contents of my mental states. It forms part of the extended dynamic process that *is* the thinking.

OK, maybe Feynman would not have put it quite like that. But I do think the imaginary reply stays close to the spirit of his earlier remarks. More importantly, it is in any case the kind of view that *NBC* (*Natural-Born Cyborgs*) was meant to suggest. Dartnall's resistance to this view has (I think) two sources. One is his suspicion that the biological is in some way special, that it plays a functional role that external elements and media simply do not (currently) play. The other is his restrictive ontology of cognitive states and cognitive contents.

Ontology is a wonderful thing, in moderation. But sometimes, a neat ontology can hide the true complexity of the phenomenon we seek to understand. If I were forced (presumably at gunpoint) to own up to an ontology hereabouts, it would primarily be one of vehicles and contents, rather than of states and contents. Thus, a perceptual content, such as greenness detected at some location in visual space, might have as its vehicle a brief and complex flurry of neural activity, none of it green. The content is thus one thing, and the vehicle another. As Dennett (1991), Hurley (1998), and others rightly insist, we conflate them at our peril. Not all the contents capable of informing our behaviour need, however, be presently active. My long-term memory, for example, enables me (if asked)

to answer the question, "where is MOMA (the Museum of Modern Art)?". The vehicle/content distinction obtains here too. There is a content, "MOMA is on 53rd St", which has a vehicle even when I am not currently rehearsing that content. When I do mentally rehearse the content, I do so by in some way activating or calling upon some vehicle, much as in the case of perceived greenness. *NBC* is an extended argument for what Hurley has called 'vehicle externalism': the view that the vehicles of content need not be restricted to the inner biological realm. In fact, the view in *NBC* is broader even than this, since not just cognitive contents, but cognitive operations (such as the comparing and transforming of representations) can, I argue, be supported by both biological and non-biological structures and processes (vehicles).

Dartnall complains that although (using his ontology) cognitive *states* remain firmly within the head, cognitive *contents* may indeed (but uninterestingly) be external. Thus concerning Otto (the mildly Alzheimic diarist from Clark and Chalmers (1998)), he writes that "Otto's diary contains the content of his cognitive state, but the state itself (once he has read his diary) is in the head". The question that we meant to address, however, was not that of the locus of the occurrent state of believing that MOMA is on 53rd St, which (for both Otto and normal subjects) we allow to remain firmly in the head. Rather, our discussion concerned the dispositional state of believing: a state (not a content, a state) that we ordinarily ascribe even when an agent is not actively rehearsing what they know. It is in this dispositional sense that you may be said to believe that Madrid is in Spain, even when that snippet of world knowledge is not in use. And it is this dispositional belief that (we claimed) might be shared by two agents even if the long-term trace (the vehicle of the content proper to the dispositional state) is in one case internal and biological and in the other case external and non-biological.

There are thus two kinds of cases the argument in *NBC* is meant to cover. One class of cases concerns the cognitive role, in ongoing problem-solving, of active loops into non-biological media (the artist's use of a sketchpad, Feynman's frantic scribbling). The other concerns the cognitive role, as support for dispositional knowledge and belief, of non-biological forms of data-storage. The two work together in the extended cases, just as they do in the non-extended ones. Information, to be useful, needs to be both stored and

deployed. It is our Cyborg nature, I argue, to use non-biological props and aids to turbo-charge both storage and use.

In addition, although Dartnall says that cognitive states (in his sense) are inside the head, it is not really clear what this can mean. He is apparently not talking here about the vehicles of such cognitive states (which, we argue, may sometimes be in the head and sometimes out in the world) but the states themselves. Vehicles can certainly have spatial locations. But the actual states (of believing *X*, either occurrently or dispositionally) do not seem like good candidates for spatial localisation. Indeed, to think otherwise seems to verge on making a kind of category mistake, of the sort that Dartnall himself explicitly warns against.

Part of Dartnall's larger suspicion is that biological forms of storage are intrinsically active (integrative, reconstructive) and that this somehow undermines the claim that non-biological media can (currently at least) serve as the vehicles of dispositional belief. I discuss this worry (and the related worries of Adams and Aizawa, mentioned by Dartnall) at greater length in Clark (forthcoming). But for now, let me just offer one very brief argument. Imagine that it had turned out (as it surely might have) that certain islands of human memory were not reconstructive, and that, in these cases, what was retrieved was always what was originally laid down. Imagine, to be concrete, that our memory for faces (only) was like this, so that I never merged two faces or made errors of recall due to subsequent learning. This would be somewhat analogous to Otto, whose long-term notebook traces are indeed unusually static. In this counterfactual world, should we say that these passive aspects of memory cannot count as partial determinants of some of the agent's dispositional beliefs (e.g., about the name of the person who looks *like that*)? I see no reason to be so restrictive. But if an inner mechanism with this functionality would intuitively count as cognitive, then (skin-based prejudices aside) why not an external one?

Technology and Transparency

Adrian Mackenzie, in his trenchant and (dare I say it?) penetrating critique, worries that I may inadvertently over-domesticate the Cyborg vision, robbing it of much of its ideo-erotic boundary-crossing charm.

I am actually sympathetic to this worry. The use of the Cyborg figure in what Mackenzie calls "cultural and feminist studies of science and technology" is in many ways different from (though not, I think, inconsistent with) my own. For it is the very essence of the Cyborg meme, in these uses, to challenge our preconceptions and to make us uneasy in our skins and in our sexual and political identities. By contrast, the key function of the Cyborg meme in *NBC* is to make us aware of the remarkable extent to which familiar, intuitively non-boundary-crossing, human thought and action is bio-technologically constituted, and thus to accustom us to the idea that hybridisation and boundary-blindness is in fact our normal state. Here is my only defence: the two projects merge and coalesce insofar as this realisation (of the domesticity of the hybrid being) removes one barrier (but only one barrier) from the exploration of even the most radical wave of near-future options. This matters, for those 'radical near-future' options are perfectly real and pressing. As William Gibson is reputed to have said, "The future is with us, it's just unevenly distributed".

It took me a while to appreciate just how the various pieces of Mackenzie's review, each of them clearly important and appropriate in their own right, actually hang together as a single (and wonderfully from-the-heart) reaction. But I think they do, in the following way. My Cyborg image can seem disappointingly 'domestic' (as above). I lay great stress on the importance of 'transparent technologies', those that are effectively invisible in typical daily use (such as the pen, which barely intrudes on our conscious awareness as we write). I celebrate a more 'biological' relationship with our best tools, in which they are simply factored in as robustly available problem-solving backdrop. Notably, I do not discuss the tidal wave of hard biotechnology itself, as manifest in designer doping in sports, drug-based enhancement of mental capacities, and the kind of genetic modification that opens the door to the delicate bioengineering of future humans. This is another tidal wave of change that may indeed (as Mackenzie aptly suggests) augur a more technological relationship with our biology rather than the more biological relationship with our technology advocated in *NBC*. What *NBC* presents is indeed, as Mackenzie charges, the image of a rather domesticated Cyborg.

Domesticated but not, I think, toothless. And since domestication was exactly what I intended, I had better just embrace it! I accept, then, that there is a whole bunch of more radical and perhaps worrying stuff waiting in the wings. I accept that what is in this way

worrying can also be exciting and liberating. My goal was to show that hybridisation, *in and of itself*, is just business as usual for us humans. So if there is something especially exciting or worrying about these other developments, then let's try to find out what it is, since it isn't (if I am right) simply the fact of hybridisation, or the potential mixing of flesh and metal.

In pursuing the image of the non-domesticated Cyborg, Mackenzie rightly notes that transparency, the lynchpin of Cyborg domesticity, has its costs. But there is no disagreement here (see for example pp. 47–58 of *NBC*). Sometimes, we certainly do want to bring our tools and technologies into clear focus, so as to de-bug, re-vamp, and even enjoy! And, as Mackenzie rightly points out, just who gets to do this and when is often a socially and politically charged affair. What is transparent to me may be opaque to you, and what is transparent to me today may be rendered opaque tomorrow. As Mackenzie sums it up "transparency and opaqueness are not intrinsic to the technology". I agree. All I meant to argue was that some technologies are better suited to invisibility in use than others. This is consistent with all his important points about "social, cultural, political, personal, economic and sexual power relations".

In sum, I accept the charge of creating a cosified, domesticated Cyborg. But I deny recklessness in doing so. The domestic Cyborg is a device for showing us what we already are, and thus better preparing us for what we might yet become.

A *Homo* of Our Own?

I am always happy to own up to a gaping lacuna, and *NBC* has plenty to offer in this regard. There is the lacuna, nicely spotted by Mackenzie, of emotion and valence. I don't say anything about these, yet they are central to human life, and surely play some important role in the construction of the self (a topic that *NBC* does indeed address). That's one very big lacuna indeed. Steven Mithen's fascinating comments unearth another, and have forced me to think much, much harder about that glib little evolutionary scenario that was slipped under the doormat with the more substantive claims about modern-day *Homo sapiens*.

The scenario, I blush to recall, went pretty much like this. Once upon a time, there were beings whose minds were pretty much

locked inside their heads. Then some of them developed (never mind how) the beginnings of human-like language. Cultured in the sea of words, these beings gradually learnt to treat their own thoughts as objects for reflection and study. With the invention of text, this process of building better worlds to think in really took off. We modern humans sit unsteadily atop this careening giant snowball of runaway co-adaptation. Our naturally plastic brains are fired in the developmental furnace of nth generation designer environments for thinking and for learning, and our thoughts are the thoughts of hybrid beings strung out between biology and those transformative waves of culture, technology and learning.

The questions upon which Mithen so wonderfully insists are simply (and profoundly): When *exactly* did this snowball start to roll? And what *exactly* got it in motion? He wants to turn up the magnification on those critical points in human history (and pre-history), so as to identify the hidden wellsprings of cognitive change. I am guilty (like many others I suspect) of sometimes finessing these questions by bluntly insisting on the role of culture, technology and training in constituting the modern mind. But to concede this (as Mithen certainly does) in no way dissolves the tricky questions. In fact, it just makes answering them all the more important. Once we appreciate the true power and reach of material culture, the question of its historical and/or evolutionary origins becomes more pressing and important than ever before.

What I found most exciting about Mithen's speculations was the idea, that I have always found attractive but never dared articulate in public, that among all of these innovations, human speech might *not* have been the key development. Mithen astutely notes that, in several places in the text, I rather clumsily conjoin reference to speech and to the use of text, as when I say (p. 81) that "with speech, text, and the tradition of using them as critical tools under our belts, humankind entered the first phase of its Cyborg existence". Repeatedly, I allude to 'speech and text', and I do so (I now suspect) so as to pay lip-service to the idea that speech might be the key, while deep-down believing (on the basis of no real evidence either way) that something else, perhaps even the use of text, is what marked the real take-off point of our Cyborg existence.

Mithen notes that the development of spoken language some 500,000 years ago did not seem to start any cognitive snowball rolling. But nor does he identify the key moment as the oh-so-recent development, around 3000 BC, of writing. Instead, Mithen's exciting

suggestion is that the emergence of art, around 100,000 BC, marked the moment when we humans first began to actively extend, manipulate, and augment our own minds. The spoken word enabled co-ordination, sharing, and the cheap, extensive, non-genetic transmission of acquired knowledge and skill. But it was the practice of inscribing environmentally persisting marks, in the form of cave wall drawings and carving, that was the first scene in the cognitive drama of the modern hybrid mind. In these first artistic acts, Mithen sees the early signature of cognition-enhancing technology: a kind of augmented reality overlay that does indeed begin to blur the boundaries between physical and informational space. With the (much later) invention of text, he suggests, the solid boundary-blurring materiality of art and the informational fluidity of speech combined to yield a truly potent engine of extended cognition. I find this a truly compelling thought.

As an aside, it is interesting (to me at least) to juxtapose Mithen's excitement (at seeing the deep parallels between *NBC*'s "descriptions of modern technology" and the cultural explosion of the Upper Paleolithic revolution) with Mackenzie's worry that the notion of technology deployed in *NBC* is so broad as to risk vacuity. I agree that it is very broad, and deliberately so, so as to raise questions about the very idea of a tiny inner agent who is the user of the body, or the brain, or the tool. At the same time, I want to stress (and this is what Mithen picks up on) the way *certain* tools and technologies materialise, freeze and externalise biologically generated thoughts and ideas. By keeping this subset in view, we can see that the notion of *kinds of technologies*, at least, is still able to do useful explanatory work.

Mithen asks whether, perhaps, those humans who lived prior to the emergence of idea-materialising artistic practice had complex thoughts (about God, the supernatural etc) but simply lacked the tricks of freezing and offloading onto the stable material environment. I remain officially agnostic on this, though I do believe that highly abstract thought is a product of, much more than a precondition for, the use of iterative strategies of freezing thoughts and ideas in material media.

The commentary closes with the million-euro question: do near-future technological innovations mark another major jump in human cognitive evolution? Here's one way in which they might. First-wave Cyborg technologies froze thoughts and ideas in material media. New-wave Cyborg technologies allow increasingly for more and more dynamic forms of delegation and offloading. For example, we can

train personalised software agents to actively seek information, goods or services. This means that our non-biological props and tools are gaining some of the semi-autonomous character of dedicated neural circuitry. I do not think this signals a brand new watershed in human cognitive evolution. But it does suggest a new and exciting twist on the standard Cyborg theme.

WHO, ME?

If there is something radical lurking in the heart of my domesticated Cyborg vision, it is surely the account of the human self. That account is distributed patchily throughout the book, and consists, at root, in a kind of no-self (or nearly-no-self) theory, according to which (what we ordinarily think of as) the self is a hastily cobbled together coalition of biological and non-biological elements, whose membership shifts and alters over time and between contexts.

Alicia Juarrero, in her elegant and unerringly accurate commentary, perfectly captures the spirit of the proposal while raising some of the most fundamental and important questions that it leaves unresolved. Juarrero asks how the concept of 'responsible agency' is to be fleshed out once we allow that it is (as I put it) 'tools all the way down'. If I am just a shifting loose coalition of onboard and offboard devices, how can I (who?) be responsible for my actions? And (relatedly): What holds any such coalition together? What makes any given coalition, at any given moment, count as *me*?

Where Juarrero places a question mark, Terry Dartnall digs a hole and plants a flag. Clearly uncomfortable with the idea that a standard non-biological tool could ever count as a real part of the agent, he writes: ''If I dig a hole in my garden with a spade . . . my-spade-and-I do not get the prize for 'best hole in the garden'. I get the prize, even though I could not have done the digging without the spade''. For Dartnall, then, it is always an agent using a tool, not an extended agent.

It is worth pushing at this a little. Suppose we ask about the role of Terry's biological arm and hand in the digging. Is this just a tool too? Certainly, it was the burden of much of *NBC* (as ably rehearsed by Juarrero) to show that we can achieve phenomenologically direct control over non-biological prostheses, and that skilled tool-users are in precisely the same boat. As Wayne Christensen (forthcoming)

nicely notes, "from the perspective of the motor control system using a tool is not fundamentally different to controlling an arm".

There is no time to rehearse Christensen's detailed argument here. But the main thrust is that a brain that is able to make the most of its own *internal* plasticity, by generating new, context-specific mixes of semi-autonomous modules and integrated processing, is by the very same token a brain that is at least *poised* so as to be able to co-opt *external* structures and processes into the very heart of its problem-solving routines. External and internal resources, as far as the mechanisms of integrative control and learning are concerned, are pretty much on a par. As a simple example, Christensen cites the work of Berti and Frassinetti (2000) who note that "The brain makes a distinction between 'far space' (the space beyond reaching distance) and 'near space' (the space within reaching distance)" and that "simply holding a stick causes a remapping of far space to near space. In effect the brain, at least for some purposes, treats the stick as though it were a part of the body".

Spades and sticks are, of course, impermanent parts of our typical physical ensemble, and many of our commonsense judgments about what should count as a tool versus a bodily part are clearly influenced by this. As a tool becomes more robustly available as and when needed, even these first person intuitions shift (see my discussion of Stelarc and his occasional 'third hand' in *NBC* Chapter 5). Overall, then, I think we here confront a wide spectrum of possibilities, rather than any single sharp divide.

But perhaps the idea behind Dartnall's comments is that the whole body is itself really but a tool, and that the locus of the mind and self is smaller still, presumably somewhere in-the-head. It seems to me, however, that the common thrust of much recent work in situated cognition is precisely to reveal the body itself as a genuine player in the cognitive drama, and not just a passive tool that does the brain's bidding. Certainly, much of *NBC* (like *Being There* before it) aimed to counteract a vision of the brain as a kind of disembodied controller. In *NBC*, one such argument went like this. Go into the head in search of the (physical vehicles of the) self and you just risk cutting the cognitive cake ever thinner, until the self vanishes from your grasp. For there is no single circuit in there that makes the decisions, that does the knowing, or that is in any clear sense the seat of the self. At any given moment, lots of neural circuits (but not all) are in play. The mix varies across time and task, as does the mix between bodily

and neural activity and all those profoundly participant non-biological props and aids.

But what, Juarrero will rightly insist, holds it all together? I do not have a good answer, but I do have some suspicions. The first is that the commonsense ideas of persons, selves, agents and moral responsibility are all (deeply interanimated) *forensic* notions. That is to say, they are concepts whose application is more a matter of habit and of practical convenience than metaphysical necessity. One lesson of *NBC* was meant to be that near-future cases will in all likelihood alter those habits and practical balances in ways that increasingly blur the line between tools and bodily parts.

The second suspicion is that the processes of 'soft-assembly' that bind the heterogeneous and distributed elements into temporary, agent-like coalitions will turn out to be scientifically tractable. For example, one intriguing possibility hereabouts may be to extend the notion of a 'dynamic core' (originally developed by Tononi and Edelman, 1998, as part of an account of what enters conscious awareness). The dynamic core is a highly integrated functional cluster of neural circuits, defined in such a way that "the term . . . deliberately does not refer to a unique, invariant set of brain areas . . . and the core may change in composition over time" (Tononi and Edelman,1998).

The core is marked by extremely high integration, rigorously defined in terms of mutual influence, between the contributing parts. Perhaps, then, we may similarly display certain non-biological elements as (at times) suitably causally intertwined with biological ones so as to create profoundly (but temporarily) integrated systems of reasoning, action, and control. Both Mackenzie and Juarrero suggest, in different ways, that very fine details of timing and feedback loops may be part of the answer to the riddle of soft assembled unity.

An important question would remain, however, concerning the role of the more insulated, semi-autonomous sub-systems (both onboard and biological and offboard and technological) that also play a role in making us who and what we are. These semi-autonomous resources (beautifully captured by Terry Dartnall in his image of the Bioborg) are not densely integrated with the dynamic core (if they were, they'd be part of it), and correspond, in Tononi and Edelman's treatment, to non-conscious neural processing. One of the big unresolved puzzles of *NBC* is, I think, how best to display these elements as more than simple (internal and external) tools, while respecting their semi-autonomous nature. Wayne Christensen, in the paper I

mentioned earlier, notes (concerning the neural realm) that "a balance of modularity and integration is required in order to produce behaviour that is diverse but coherent". I believe that one key to understanding how nature makes cognitive agents is to understand the general principles of this balancing act. When we do so, I suspect we will find many of the same principles at work on larger-scale ensembles, enabling extended cognitive systems to find and occupy the sweet spots between full integration and unstable aggregation. That does not answer Juarrero's well-aimed question, alas. But it does show where I am inclined to look.

In closing, I would like to thank the reviewers for this treasure trove of exciting suggestions and important challenges. They cement my conviction that this is an exciting and transformative time for the sciences of the mind. As technological progress provides new tools and new puzzles at about an equal pace, the time is ripe to begin to put together the many pieces of the puzzle of mind. That means, I firmly believe, seeing our unique cultural and technological scaffoldings as not just aids for understanding the mind, but as key parts of the minds we seek to understand.

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REVIEW SYMPOSIUM

NEW SOUNDS, NEW SOCIETY

Trevor Pinch and Frank Trocco, *Analog Days: The Invention and Impact of the Moog Synthesizer*. Cambridge, MA: Harvard University Press, 2002. Pp. xv + 368. US$29.95 HB.

By Tia DeNora

It was springtime 1996 in a remote hamlet in upstate New York and 'The Man from Moog', Jon Weiss, had been tracked ('finally') to his lair. Synth-detectives Pinch and Trocco coaxed Weiss into speech with tales of their own involvement in the period. A six pack was produced, rapport built up:

> It was getting late. We moved down into the basement. There it stood in the corner – a shadowy presence, row upon row of knobs, patch wires dangling like spaghetti, the ghostly modules of a long-grounded spaceship. (p. 4)

And then the time machine was activated:

> [Jon] hit a switch. Lights flashed. Jon patched in wires and adjusted knobs. The sound of an oscillator grunted into life. He soon had the sequencer set up, and a repetitive pattern of sounds flashed by faster and faster, the tone color changing as the filter came into play, tantalizing like shimmering icicles in the higher frequencies, then cascading downward through the deep resonant tones – the famous fat squelch of the Moog filter. Onward he patched and patched. (p. 5)

As is evident in this passage, Pinch and Trocco have the rare knack of capturing on paper the feel and mood of sound. The odyssey they describe, nothing short of a boys' own adventure, is a quest to salvage the sonic texture of an era, captured on the one hand by the epigraph from Fran Lebowitz' *Metropolitian Life* (1978): "spray cheese, stretch denim and the Moog synthesizer" (p. xvi) and on the other by the organic, hippy and counter culture movements associated with San Francisco and the Bulcha Box. Readers who can recollect polyester when it was new (wrinkle free pantsuits!), the invention of CuppaSoup®, or psychedelics, are already halfway

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there and, very probably, they will enjoy this book. Those who can not recall (or who weren't born yet), should be beguiled anyway by this tale of new sounds, new forms of association, and the career of the synthesizer from the 1960s to the present. An earlier reviewer called this book 'dry' and suggested it would appeal most to those with an existing interest in ''the music and social and technological history of the 1960s and 1970s'' (Craske, 2003); from that description I feel I must have read a different book. (Incidentally, in the interval between that review and this, *Analog Days* has won the Foreword Prize silver medal – first place, a book about Bin Laden, third, a cookery book!)

Pinch and Trocco's methodological mandate is neither frivolous nor easy to realise in scholarly practice. Its code resonates with the utopian and co-operative spirit of the age it describes: to 'avoid hindsight', to 'recreate the enthusiasm and uncertainties of what it was like back then, before anyone knew what it would be like now' and to 'use the pioneers' own words to describe their visions, their excitement, and their disappointments'. The study begins with the font of the synthesizer movement in radio hobbyist culture (Bob Moog as technology geek in basement). It moves on to follow the Moog's adoption by New York experimental musicians and the simultaneous, but completely independent, development of the key-less Bulcha machine (Bulcha did not like the term 'synthesizer' and its connotations of imitation and 'man-made' materials) and its association with Cage and Tudor at the San Francisco Tape Center. It examines SCOT (Social Construction of Technology) tributaries such as different strategies of user-accommodation (Moog's and Bulcha's), users' rich and varying interpretations of a technology's meaning (I enjoyed reading about the feminine appropriation of an other-wise male technology – e.g. Suzanne Ciani's description of her Bulcha as 'feminine and warm'), the meaning of 'innovation' versus 'invention' (the former involving a thousand conversations and a thousand different design decisions) and the various offshoots and offspring of the synthesizer over four decades. It dwells with affectionate humour, for example, on how the ribbon controller (for, among other things, smoothly shifting pitch) was used by rock keyboardists as part of their performance regalia. Keith Emerson would lift the two-foot-long narrow rectangular box from his groin as the music climaxed, sometimes attaching toy rocket motors for additional pyrotechnic impact. As Pinch and Trocco observe, sympathetically, ''running around with a big electronic phallus in live

performance is not always easy'' (p. 63). How *could* this book be thought to be 'dry'?

Back home in upstate New York (Trumansburg), Moog set up his factory. When the Moog was new, it was very new; no one quite knew, initially, what it would do, but the locals nonetheless had their suspicions of this new instrument and its new-fangled sounds. They associated it, not unreasonably all things considered, with sex, drugs and rock'n'roll. Like the electric guitar (p. 74), the synthesizer was not an instrument that was taught in school, another point in favour of its counter-cultural credentials. It is at this point that we come to the heart of what is at stake in the subject of this book, the affinities between sounds, their creators and recipients, and social practice. To this end, Pinch and Trocco develop the theme of resonance.

When Bob Moog arrived for his PhD defence and rode the elevator to the top of Cornell's Clark Hall, it occurred to him that even an elevator had a natural frequency at which it would resonate. If that frequency were to be set off nearby, he reasoned, the elevator would vibrate in sympathy. Moog thus began to jump in the elevator, trying to find the right frequency. He was successful and the elevator bounded in time with his jumping until it halted, and left Moog stuck for several hours before help arrived. (He passed his viva later in the day.)

In symbolic, less physical ways, sounds can be understood to resonate with the frequencies of social experience. It is possible to speak of sonic strategies deployed in the making of life, whether collective or individual. It's on this point that *Analog Days* covers ground that intersects with music sociology, at least as practised by me, and, to turn to an STS exemplar, the French one-time engineer and music sociologist Antoine Hennion (see Hennion 2001). As a sub-field, music sociology has over the past decade increasingly turned toward the mutual production of music and its users, a process in which sound and the creation/appropriation of sound are treated symmetrically.

Pinch and Trocco are concerned throughout with the ways in which 'new' sounds were appropriated for (and helped to fashion) social practice. The book is replete with illustrative vignettes. The Trips Festival of 1966 was one of these. There, the 'new' sounds were part of the way in which new values, orientations and sensations were forged. The links between sound technology's social construction, its affordances and the articulation of action and

identity could have been further developed in this book, though they are always deftly sketched. Some of the most intriguing of these sketches involve cases where there is (or might be) 'gender trouble' (in Butler's sense of subverting the institutionalised sexual binary of male/female, see Butler 1990). The most tantalising of these centres on Wendy (originally Walter) Carlos, the creator of *Switched-On Bach:*

> The question arises as to whether Wendy's metamorphosis (Carlos was one of the first public figures to undergo a sex change operation), which occurred just around the time she was developing as a synthesist, had anything to do with the Moog, and with synthesis itself. Perhaps there was something about this most unusual instrument that resonated with the most unusual transformation its star performer was about to undertake. (p. 137)

Occasionally, and it can be seen in this Carlos example, Pinch and Trocco step beyond their code of using ''the pioneers' own words to describe their visions''. When they do, their music sociology of transformation ventures onto hypothetical terrain. The ''transformative power of the synthesizer'', they opine, ''may have allowed her (Carlos) not only to conjure up a new musical meaning but also helped her find herself as a newly gendered person''. The authors had no access to the (understandably) media-shy Carlos, however, and this issue remains unresolved.

The authors get closer to the social-transformative powers of sound when they deal with another woman in this predominantly male world, Linda Fisher, who, unlike Carlos, gave an interview. Fisher described how she visualised the synthesizer as able to 'subvert' the 'dominant outlook'. Shades of Adorno here, with the idea that new sounds call for new sense-making practices (think also of Pierre Boulez' famous comment that he intended to 'strip the accumulated dirt' from music). Some time ago I suggested that music provides a place for 'work' (in the ethnomethodological sense) and that unfamiliarity creates space – for new types of work and for the 'play' required for that work (see DeNora 1986). The synthesizer would appear to have provided Fisher with just such a resource or space for social innovation. And so too, it would appear to serve the authors themselves, who, just occasionally, are overtaken by their own enthusiasm for, 'what it was like back then' (do not forget that they were if not pioneers, certainly members of the synthesizer generation).

Comparing Morton Subotnick's *Silver Apples of the Moon* (1967) with Carlos' *Switched-On Bach* (1968), for example, Pinch and Trocco engage in the normal members' classificatory activity, in this case, hailing a 'breakthrough', which they note via a paradox:

> [I]t was Wendy's music, which could never be performed, that was the more expressive, the more alive, the more like a performance. Although Subotnick's record won critical acclaim and sold considerable numbers for that sort of experimental record, it was Wendy's record that achieved the breakthrough. (p. 144)

A last quibble, perhaps because of the wide sweep of history and technology and people covered in this book's one hundred thousand words or less, the reception history behind the story of the synthesizer is mostly glossed, as are other issues worthy of unpacking: the engineers at Trumansburg were 'a tight-knit bunch'; Suszanne Ciani's search for a poetry of sound was 'overwhelmed by the wider cultural forces she encountered', 'the Minimoog itself went from strength to strength'. These glosses are not naïve, however, and it is acknowledged on p. 313 that ''the identities we have assigned to the actors in Analog Days are in an important sense inadequate … actors refuse to comply with the labels that we analysts give them''. (I was interested that Pinch, who has experimented in the past with 'reflexive' narrative forms, decided on this occasion to play it straight narratively.) What is needed, they argue at the end of the book, are, ''new ways to designate not only the liminality of machines but also the liminality of the human roles and identities built around the machines'' (p. 314). They then allude to this process with the concept of boundary shifters, that is shifting in and out of frames, and I would like to have seen this theme developed systematically – perhaps through specialist articles and similar spin-offs. That said, Pinch and Trocco's celebration of the age of the Moog reminds us that sound – not just music – is a too-often neglected feature of social life. Its exploration here makes an exceptionally engaging and thought-provoking story and I hope readers will also use the excellent discography (pp. 325–329) because, as John Cage famously put it in *Silence,* we need far greater, 'attention to the activity of sounds'.

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By Fred Callopy

In its broadest terms the story that Trevor Pinch and Frank Trocco tell is an extraordinary one. Here is an instrument whose journey from invention to mass market took place in less than a generation. It is an invention that threatened and affected a whole category of labourers (studio musicians) and changed our very idea of how music should sound.

Some potential readers may wonder why they should care about the design of musical instruments at all. This question is addressed in the very first words of the book's foreword, by no less than Robert Moog himself. ''Musical instrument design is one of the most sophisticated and specialised technologies that we humans have developed (p. v).'' This is in part because in music making both musician and listener function at the limits of their perceptual and cognitive capabilities, the music-instrument system is full of feedback loops, and musical instrument designers are not driven simply by objective performance specifications. These features combine to make musical instrument design a domain of great potential interest to designers. And rarely have researchers had the opportunity to interview as many participants in the design and development of a new instrument as we see here.

Viewing the synthesizer as a good case study of musical instrument design, the authors examine many interesting questions: Why do synthesizers have tonal keyboards? Why are they used to make familiar sounds, often reproductions of sounds created by other instruments? What accounts for a widespread reliance on factory pre-sets among synth players? And when and how did these choices get made? Pinch and Trocco worry as much about social aspects of the instrument's development as they do about technical ones.

Analog Days is organised along historical lines, though it also reads like a journey narrative. And indeed it is something of a search – a search for ''the meaning of an instrument (p. 10).'' The authors contend at the outset that they see the development of the synthesizer as a social as well as a technical story. By the end of their journey they will have moved all around the US and through a variety of its artistic and commercial communities to argue that many people and social phenomena must be invoked in order to explain the synthesizer's success. That aside, though, this is also a hero story, and the hero is Bob Moog.

Moog is introduced as the inveterate tinkerer; a mildly obsessive sort who would coax all sorts of interesting sounds from electronics he found at local army surplus stores. As the story of the synthesizer unfolds, he remains at its centre, or perhaps at a spot just a little off centre – generally a bit bemused by the rock musicians, and even the marketers, business people, and other designers who ultimately transformed his instrument into a commodity.

A continent away was Don Buchla, the anti-hero. Just three years younger than Moog, he too was an experimenter. As Pinch and Trocco tell their stories, Buchla is the counterpoint to Bob Moog; his Buchla Box its antithesis. On one level their story is the classic one of parallel inventions, followed by a horse-race in the market-place. The two men began work on their synthesizers within months of each other. Moog's first prototype was finished in the summer of 1964, Buchla's in the fall of the next year. Both were modular, patchable, and voltage controlled. So another question to which the book attempts to provide an answer is – why did the Moog ultimately come to dominate the market?

The answer the authors propose is that Bob Moog listened to his customers, where Buchla functioned more as a member of the *avant garde*. Both inventions started out in support of experimental sound creation. But where Moog responded to musicians who demanded a keyboard, Buchla resisted, seeing in the synthesizer new possibilities that demanded a new interface. What Moog did, and Buchla failed to do was to "embed into his technology a piece of existing culture – the idea that music is about intervals" (p. 309).

Early synthesizer players had a problem, actually two problems. First, the ways in which circuits could be combined produced an almost infinite range of possible sounds. And second, because there was no easy way of naming the sounds, it was hard to refer to them. A *Doors* recording session illustrates.

> "That crystalline sound," Jim [Morrison] jumped in. "I liked that sound of broken glass falling from the void into creation." "Which sound was that?" said Paul Beaver. "A couple back from where you are now," Rothchild said. "It reminded me of the Kabbalah," said Jim. "Kether, the I AM, creating duality out of the one. All crystalline…and pure. You know that sound." "Did I make a sound like that?" "Sure," Jim said. "A couple back." "Just go back to where you were," said Rothchild. And Paul Beaver began to unplug and replug patch cords, and twist little knobs, and strike the keyboard, which emitted strange and arcane and utterly unearthly tones that sounded nothing like the Kabbalah or Kether, the crown of the

Sefiroth. None of the sounds he was creating sounded pure and crystalline. And then we realized . . . he couldn't get back. (p. 121)

It is this unpredictable quality that many of the early synthesists most associate with that period, and some of them look back on it longingly. As Wendy Carlos put it "That was when you really felt like you were working with an invention" (p.136). So at this stage the synthesizer was difficult to control. These were machines on which a new breed of musicians conducted searches – searches for new sounds. There was a sense of transience about those sounds. They could be there one moment and gone the next. There was an attitude that these things were organisms, not fully controllable. Each synthesist put them together in personal ways.

But with the Minimoog in particular, the synthesizer developed into a portable, keyboard-based, performance-oriented instrument. Patch chords were eliminated in favour of knobs, the number of potential sounds was reduced in favour of simplifying the interfaces, and the pitch wheel was added to provide a sense of touch.

Ironically, the forces that caused synthesizer design to move in this direction were largely the result of the success of Carlos' own mega-hit *Switched-On Bach*. According to Ciani, the public's attitude about the potential of the instrument was misdirected by that work. Her search for "a poetry of sound" was overwhelmed by wider cultural forces set in motion by Carlos' success. The synthesizer became associated with classical music and classic sounds.

Much that can be learned from this work relates to the role that constraints and quirkiness play in design. For example, early synthesizers had monophonic keyboards. Instrumentalists like Keith Emerson therefore thought of the Moog as like a saxophone or trumpet, playing it as a solo instrument. With the emergence later of polyphonic synthesizers, the keyboard reverted to the keyboard's more typical role in the background (p. 206). This is particularly interesting, given that polyphonic instruments can still be played monophonically.

When one of Moog's engineers, Jon Weiss, went to visit *Sun Ra*, his synthesizer was not working as it was supposed to but he was using it to produce the most fabulous sounds. For Jon, this was the mark of an instrument as opposed to a machine. The authors generalise "It is departures from theoretical models of instruments – the unexpected resonances and the like – that make an instrument particularly valued" (p. 223).

For the attentive designer, there are lots of other lessons in this book. For example, through their interviews, Pinch and Trocco found that one of the things that worked best about the control panel for the Minimoog was the way in which knobs were staggered. ''It turned out that having things not all in military formation made it a lot easier for someone to find a control'' (p.225). Further, tactile controls like rocker switches permitted players to find their way around using feel. And it is important in a musical instrument for the musician to be able to dynamically alter the sound in very small ways. ''For many musicians, it is the pitch wheel on the Minimoog that enables them to make the instrument come alive. By bending a pitch or adding vibrato, a note can be given that special personal touch that violinists and guitarists find so important'' (p. 228). These and similar lessons will, I think, have wide application in the design of instruments, even instruments that are used for purposes other than making music.

This is an easy book to recommend to anyone with an interest in instrument design. It reads like a novel, and its many insights will remain with me long after the details of its stories have faded from memory.

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By David Philip Miller

For me this book provides cause for nostalgia on almost every page. It delightfully entwines the professional and the personal. It will take many people of a certain age back to their musical youth. The late 1960s and early 1970s were a time of new, adventurous, even grandiose sounds in popular music. Increasingly these sounds were electronic. I fled into folk and acoustic blues in growing despair at the impossibilities (in terms of skill and available technology in a small Northern England town on a schoolboy budget) of parroting the sounds of Hendrix, or Pink Floyd, or Robert Fripp, or Emerson, Lake and Palmer. The alternative would have been to head to the Army surplus store, crack out the electronic hobbyist magazines, raid my father's well-equipped workshop and

start soldering the circuits, or, as Trevor Pinch did, to buy a 'Minimoog'.

Whilst *Analog Days* brings yet another area of life under the scrutiny of technology studies (and of SCOT, the social construction of technology, in particular), the book does not present itself as a heavy academic tome. Written in a breezy, accessible, journalistic, sometimes 'hip' style, it might well have been serialised in *Rolling Stone* magazine more or less as it is. The authors' language converges with that of the interviews with leading protagonists that form a documentary backbone of the work. The design of the book reflects a labour of love. The elaborate, folding dust-jacket offers not only pictures of key men and their musical machines, but also a circuit diagram of some of the oscillators of a synthesizer. Even the page numbers are neatly encapsulated into the filigree of a segment of circuit diagram. Yet the style aids, rather than substitutes for, the communication of substance. The book is clearly aimed at a wide audience, perhaps doing for ageing, and former, hippies around the world what the *Golem* books have done for a general audience. In fact the ideas about technology and technological change that are conveyed here are in many ways subtler than the *Golem* message. At a time when the popularisation of the history of science and technology (the 'Sobel Effect' literature, see my paper in *Metascience* 2002) raises some important professional concerns, we have here an exemplary piece of popular, in the sense of accessible, history of technology. This book is not the product of a quick market decision but the outcome of years of careful research, and well-tempered conceptual analysis. It is based on the kind of intimate, carefully thought-through, knowledge of a subject and a period – in short on the sort of true expertise – from which the best popularisation stems.

Explicit theorising intrudes little on the telling of the story of the development and use of the Moog synthesizer. Introductory remarks and the occasional didactic paragraph are the only clues in the main text, until the concluding chapter, that SCOT theory is guiding and shaping the study. Nevertheless, the key foci of SCOT are all there: the 'interpretive flexibility' of artefacts; the importance of relevant social groups, especially user groups, in the shaping of artefacts; the processes whereby artefacts are stabilised.

'Interpretive flexibility' is illustrated and explained in a number of ways. First, among the generic class of 'synthesizers' (itself a negotiable term) there was a wide variety of devices: the Moogs, developed in the Eastern United States; the Buchla Box, developed by Don

Buchla in California, the Electronic Music Studio machines produced by Peter Zinovieff in London, and so on. All these devices were, in one way or the other, the product of interaction between the designer, other engineers, musicians and composers. Indeed, as Pinch and Trocco frequently point out, in many cases these roles were not at all clearly demarcated. Even at the personal level these devices, in their ongoing varied manifestations, were different things to different people. Female composer/musicians such as Suzanne Ciani, for example, related to the same machine in different ways from their male counterparts (who were of course in a vast majority). Ciani declared herself 'in love' with her Buchla 200, and believes that women work differently with the technology of synthesizers.

Amid the plethora of interpretive differences, however, some emerged as systematic and structured ones. The Buchla Box remained steadfastly experimental and unpredictable. For Don Buchla himself, and for the musicians and composers with whom he interacted most closely, these were virtues. The device was designed to contribute to musical experimentation. Its sounds were consciously not only 'way out' but essentially irreproducible (except by capturing them on tape). The 'access' to the device was also steadfastly foreign to other musical devices. Buchla resisted the connection of the machine to a keyboard and, even more fundamentally, the calibration of the machine to twelve semitones in an octave. For him, and for the composers and musicians who valued this device the most, such moves would be a sell-out to tradition and to commerce and a betrayal of its truly alternative character. It was different with the Moog. Its originator, Bob Moog, was not really of the alternative generation. He had a stronger market orientation and was happy to cater to the desires of mainstream musicians. Walter/Wendy Carlos's phenomenally successful *Switched-On Bach* was very important in linking the new machine with tradition and popular success. The decision to provide access to the Moog via a keyboard and to adopt the twelve-note configuration, and thereby make the instrument give play to the acquired skills of the vast majority of musicians, decisively moved the instrument to the mainstream. In a smaller way, another mode of access to the machine, the 'ribbon controller' which varied sounds by moving a slide along a metallic ribbon, catered to existing practices with its guitar-like, phallic possibilities for histrionics on stage.

None of these divergences or convergences was inevitable. There were those who maintained, for example, that the keyboard was not a constraint on musical innovation, that it did not restrict the

synthesizer's possibilities. This was so in the case of Malcolm Cecil and Bob Margouleff, and their eclectic machine 'TONTO'. Nevertheless the objectives, and ultimately the cultures, of designers and users of these musical instruments made some of the divergences and convergences more probable than others. Above all the Minimoog brought a synthesizer within reach of the budgets of many more potential customers and met a widespread desire from middling and imitative musicians for standardization and an instrument that could be played in live performance with relative confidence and predicability. The elaborate and hard to reproduce 'patches' of the original machines were increasingly hard-wired. Even so, as Pinch and Trocco show, there was no ready-made market for the Minimoog. Markets, like instruments, have to be created. The authors devote a chapter to the marketing exploits of David Van Koevering who did so much to 'move' Minimoogs in the US and internationally. They show convincingly that being a great salesman involved much more than having a 'gift of the gab'. It involved literally inventing the synthesizer market.

In the longer term, as the world went digital, so the synthesizer became more sophisticated and was looked to increasingly to provide a set of standard sounds that could be varied within carefully controlled parameters. The experimentalism, the element of surprise, and, some argued, the creative possibilities for 'sculpturing' sound were lost. Yet, as Pinch and Trocco bring out in their conclusion, the urge to experiment remains strong. Among young musicians today there is a renewed fascination with what they regard as the authenticity and experimental possibilities of analog devices. The experimental aspects of rave music, and to a degree the drug-fuelled marathons that often accompany its performance, in many ways capture the spirit of the experimental music/LSD scene of which the Buchla Box was an integral part.

The largely unobtrusive use of SCOT conceptual frameworks to analyse all these features of synthesizer technology works extremely well in my view. In fact, the theoretical approaches drawn upon are edged forward somewhat from the standard SCOT repertoire. In particular, the interaction of the individual with the machine is explored much more fully here than in standard SCOT case studies. Are there any criticisms to be levelled? It is a fairly standard criticism of SCOT that the approach can and does lead to the analyst ignoring issues of large-scale structure and power. Is that true here? From certain perspectives it might be. For example, someone who still

yearns for the true experimentalism of electronic music might well argue that the whole process of developing the synthesizer, even the concept of the synthesizer itself, involved corruption and control by the commercial forces of popular music. From such a perspective, the Moog, far from being the success story depicted here, was the instrument of the devil. The Moog 'sold out' and its digital descendants now simply reproduce traditional sounds and musical forms. SCOT in this sense simply recapitulates the history of the victors. Indeed, Robert Moog is a hero of this story and, fittingly, he provides a foreword to the book. Both experimentalists and traditionalists, some of whom find the synthesizer as distasteful as synthetic cream, might well complain that Pinch and Trocco have missed the big picture.

SCOT has also been criticised for its tight focus upon the dynamics of technological innovation and its tendency to leave unstudied the transformations of personal experience and social relations that technologies can be involved in. *Analog Days* makes a conscious effort, I think, to supplement the traditional SCOT approach in this regard, especially in examining the individual human-machine interaction with some sensitivity. It is true, however, that the 'impact' of the Moog is less fully dealt with than is its invention/innovation. Once synthesized music took a more traditional turn it did represent at least a potential threat to jobbing musicians. An examination of the extent of this threat and how it was played out, or not, would have required a deeper and more systematic study of how major performing artists and studios incorporated synthesizers into their production methods.

In criticism of *Analog Days* one might also argue that, with the numerous important technological issues available for study, an account of the Moog is a rather indulgent choice. Giving people like me a nostalgia trip may not be the best use of resources and ingenuity. Pinch and Trocco do not address this issue specifically, though Robert Moog's foreword does argue for the particular interest of musical instrument design because they couple technological sophistication with a subjective, quirky, even 'irrational' character. In my view, the intensity and importance of musical experience and expression in peoples' lives, the sheer delight involved in witnessing or engaging in exquisite moments of musical insight (or even, dare I say it, resolution) give the lie to the charge of triviality. The technologies of sound have been too long sidelined in the history of technology. All big causes look to the transformative power of music.

For me, SCOT sounds a true note in *Analog Days*.

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Authors' Response

By Trevor Pinch and Fred Trocco

Responding to three such generous reviews is not easy. Writing a book that attempts to reach a popular audience was also not easy. Since one of our reviewers, David Miller, comments on what he calls the "Sobel Effect" and congratulates us on "an exemplary piece of popular, in the sense of accessible, history of technology", we thought it would be interesting to share with *Metascience* readers some of our own experiences and dilemmas in writing *Analog days*. The sub-text for the next part of our response is "Why we aren't rich and famous".

Our project began as a conventional piece of technology studies. Sponsored by the Lemelson Center for the Study of Innovation and Invention at the American History Museum of the Smithsonian Institute, we set out to record oral histories with the pioneering musicians, engineers and salespeople who developed and marketed the first synthesizers. Admittedly the project did have that 'boys, own' feel and was in part a voyage of discovery. Having been part of the 60s we wanted to discover what it was we had been part of. Our respondents had been there too and they had (unlike us) remembered! As we started to collect the stories we found that our interviewees were wonderful raconteurs. Soon we became friends with some of them – staging events on the history of the synthesizer with them and ringing them up for friendly chats and visits. The stories they told were so good that we realized that the only way we would really mess up this book was by letting our analytic voice drown out their voices.

We also could not but fail to notice that the book had some commercial potential. A neighbour who worked in the publishing business suggested we send a draft of our book proposal to her friend and former colleague who happened to be one of the best known

literary agents in New York city. Without giving it much thought we sent off our proposal. A few days later we got an excited phone call from the agent confidently predicting that he would be able to sell the proposal to a trade publisher. Tentatively we asked him what sort of advance he thought the book might command, "We were looking at around $200,000", he casually remarked. Nice work if you can get it, we thought.

We did have to agree to a shorter manuscript than we wanted and a faster delivery date. Also he suggested we shelve our plans for a CD to go with the book – better to negotiate a separate contract for that later – more money. Pinch's own long history of disappointment with publishers made us cautious. A few weeks later the agent got back in touch. He was having difficulty selling the book: "No-one can see what interest there might be in an old machine. What they want is a book about sex, drugs and rock and roll." It soon transpired that the agent – unlike our reviewers – had little clue about the book. Having failed in his task to sell it for a vast amount of money, he recommended we go with an academic publisher with trade press interests – and that's what we did.

Metascience readers may find it difficult to believe, but we were somewhat relieved by this turn of events. Hip capitalists we were not. No-one in the analog synthesizer industry has made any significant money and we would have felt uncomfortable in the extreme interacting with our new friends knowing that their stories were about to make us rich. Also there is no doubt the book would have had to be less academic than it ended up being. The cross-over trade and academic book which Harvard University Press asked us to produce suited our goals but raised a new set of dilemmas.

To get the book accepted by the *Harvard Press* board we found ourselves promising to revise the book to make it seem more academic and less readable! The Press also held us to a strict word limit. Cut down the quotes was one editor's advice: "people want to hear the voice of the authors in a popular book". But the quotes for us were the main part of the story. In the end we stayed with the quotes and had to excise most of our academic foot notes (a loss which one reviewer has lamented). The power of the quotes is evident from the copious use to which our three reviewers have put them. Our skill, as Miller astutely notes, was to fine-tune our text to the style of the people we talked with. This is not a trivial exercise, as we discovered. We have drafts of whole chapters that we canned simply because we could not hit that "breezy style". Serialization in *Rolling Stone* would

be a first for S&TS (although Langdon Winner as a former *Rolling Stone* journalist may have been there already). Our experience is that this will not happen, however well-written and interesting the book. The sad fact is that popular magazines and newspapers these days hardly ever review (never mind serialize) any book published under the imprimatur of an academic press (this is another down-side of the 'Sobel effect').

But precisely because the book had cross-over potential we experienced things we had not experienced with academic books before (and certainly not with the *Golem* series): a devoted and enthusiastic editor and book design team who came up with the clever design which Miller enthuses about and a superb copy editor who really knew how to work with our prose. The hardest dilemma in going for a popular audience was what to do about SCOT. It informed our analysis but if we put all the terminology "up front" we would risk alienating readers. In the end we drew out the general themes of our project in the introduction but saved the detailed academic language for the final cadences. We think of this, like Miller, as a "true note" for SCOT but others will, we are sure, find it an unacceptable compromise.

We are gratified that Fred Callopy likes the book's focus on how musical instruments get modified in interaction with users. We share his belief that the book will be of much interest to instrument designers. Our study is part of a rich vein of user studies of technology – technologists are starting to take note of social studies of technology and anthropology of technology precisely because of the detailed attention paid to the role of users and over-looked mediators such as sales people. By choosing to study a musical instrument and its history in this way – we hope to show how SCOT and like approaches have relevance for fields of musical instrument design and ethnomusicology and the history of music more generally.

Music sociology, as Tia DeNora notes, is one area where the book finds a resonance. She is right to point out that the links between the identity of users and producers and a technology's affordances and actions could have been developed more. It is precisely this resonance with work on gender and cultural studies which provides an entry for S&TS scholars. The academic papers she hopes we will go on to write in this area are indeed currently under construction. DeNora also has some quibbles, noting that our treatment of Carlos as an example of the synthesizer's transformative power is "hypothetical". She is quite

right. Because of Wendy's reluctance to be interviewed and the central role she plays in the history we felt that a few speculative measures were better than silence. That this was a risk can be gleaned from Carlos's own response to our book. On her web site – http://www.wendycarlos.com/ouch.html – she has an 'ouch' list of people who have grievously offended her. Currently we are at the head of that list!

DeNora also notes our lack of coverage of the 'reception history' of the synthesizer. We were aware when we wrote the book that we did not have much to say about how ordinary listeners experienced the synthesizer and its sounds. Part of the difficulty here was that we did not really have the option which Tia DeNora has imaginatively explored in her own research (DeNora, 2000), that of interviewing listeners about how they experience music. Maybe we are wrong, but we felt that it was too methodologically problematic to ask listeners how they first experienced sounds they heard nearly thirty years ago. Listening practices are so dependent on context. This is a case where you really have to be there: whether dancing at the Trips festival with its hallucinogenic light shows and collages of sounds; or tripping in the communes watching the sunrise that surprised civilised man; or lonely in the bed sit experiencing the strangeness of the electronic timbres of *Switched-On-Bach*. Listening practices can be teased out for specialist groups such as doctors using stethoscopes (see Jonathan Stern, 2003) or audiences for symphony concerts (see Emily Thompson, 2002), but it is much harder to recapture the history of ordinary listening practices.

Lastly, Miller and DeNora notice our own enthusiasm for the people we studied. Were we over-indulgent and did we swallow too many of the heroic stories we were told? Obviously this is the danger in studying something you know and love amongst people who were once your heroes. Such an enterprise gives you the personal authority, dedication and enthusiasm needed for carrying through such a long and arduous project as this one turned out to be (seven years of research and writing). But there are risks. We were well aware of the dangers and tried to point to areas where we knew there was more to say. But this is also again the dilemma of trying to write in a popular style. To reach a popular audience you have to embed into current narratives. Hopefully (and here unlike Sobel) in S&TS we do this ultimately to shake things up and subvert conventional narratives about technology. It is always a balancing act – what and where to subvert and where to 'go with the flow'. That culture can be

built into technology is a message worth getting across to the widest possible audience.

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ESSAY REVIEW

PAPER TOOLS IN TWO AND THREE DIMENSIONS

Ursula Klein, *Experiments, Models, Paper Tools: Cultures of Organic Chemistry in the Nineteenth Century*. Stanford, California: Stanford University Press, 2003. Pp. xi + 305. US$65.00 HB.

Peter J. Ramberg, *Chemical Structure, Spatial Arrangement: The Early History of Stereochemistry, 1874–1914*. Aldershot and Burlington: Ashgate, 2003. Pp. xxiv + 399. £57.50 HB.

By Mary Jo Nye

The history of nineteenth-century chemistry is greatly enriched by these two books, which are complementary to one another in their themes and in the historical periods under scrutiny. The complementary nature is in part the product of fruitful collaboration during Ramberg's residence in Klein's research group in the history and philosophy of chemistry at the Max Planck Institute in the History of Science in Berlin.

Klein's focus is on the years 1827–1840, and Ramberg's is on the period 1874–1914, although there is a good deal more of the nineteenth century in these books. Each of these authors is concerned with the cultural practices of European chemical communities in the nineteenth century, by which they mean the laboratory culture of experiments and the theoretical culture of symbols. The authors examine both stabilities and transformations over the course of the nineteenth century, using the trope of the "quiet revolution" (Alan J. Rocke, *The Quiet Revolution: Hermann Kolbe and the Science of Organic Chemistry*, 1993). The manipulation of chemical symbols and formulae as productive tools of prediction and discovery is the core theme of these detailed studies, as the authors immerse the reader in the everyday problems faced by nineteenth-century chemists and the modeling techniques that they used to solve them. Manipulation of paper tools, it turns out, was a crucial research methodology in the

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transformation of chemists' empirical natural-history tradition into an experimental physical-science tradition, as new layers of meaning converted formulaic representations of chemical composition into physical models of three-dimensional configurations of atoms and molecular structure.

Klein's *Experiments, Models, Paper Tools* is both historical and epistemological in its aims. She has developed the very original and useful notion of 'paper tools' in the practice of science following an approach that Bruno Latour has called "science in action" (Latour, *Science in Action: How to Follow Scientists and Engineers through Society* (Cambridge, Mass, 1987). Historians like Andrew Pickering, Andrew Warwick, Jeffrey Hughes, Peter Galison, and David Kaiser have analysed how mathematical symbols and visual graphics operated in theoretical physics as instruments and tools of the trade, and Klein applies this kind of analysis to the science of chemistry.

In doing so, Klein is by no means the first historian or philosopher to think about the theoretical functions of sign systems in chemistry. Prominent among those who have preceded her is the French philosopher François Dagognet (*Tableaux et languages de la chimie*, 1969, and *Ecriture et Iconographie*, 1973). In Klein's view, however, Dagognet erred in describing the kinds of formulae developed by the Swedish chemist Jons Jacob Berzelius in 1813–1814 as fundamentally different from the later structural and stereochemical formulae of the later nineteenth century. Dagognet suggested that these later formulae were graphical formulae that operated as true icons representing an object (the molecule) by the symbol's similarity to the object. In contrast, Klein successfully demonstrates the graphical quality of Berzelian formulae, their function as models, and their evolution into diagrams that are consistent with the later forms rather than incommensurable with them.

At the risk of succumbing to a presentist perspective, it is useful to define some terms. From Berzelius on, chemists came to think of their systems of chemical symbols as denoting composition, constitution and/or configuration. A composition formula is an empirical expression that gives the composition by percentage weight of the constituent chemical elements (chemical atoms) in a compound substance. A constitution formula expresses the way in which the atoms are combined or attached to each other within the chemical compound. A configuration formula shows not only attachments, but also the arrangement of atoms and of groups of atoms in

two-dimensional or three-dimensional space. These differences in formulae are fundamental to Klein's and Ramberg's histories.

Klein's analysis focuses largely, although by no means exclusively, on the work of Berzelius, the French chemist Jean Dumas, the French pharmacist Polydore Boullay, and the German chemist Justus Liebig. This history is not only one of a transformation in paper tools and in theoretical explanations of chemical reactions, but also one of a quiet revolution from plant and animal chemistry (which was rooted in natural history, medicine and pharmacy) into carbon chemistry, which no longer took its materials solely from living organisms but began to synthesize new and artificial materials that were indistinguishable from their natural organic counterparts except with respect to their origin. It is Klein's argument that the style and pace of the new carbon chemistry resulted in considerable measure from the productive tools of the new paper chemistry.

Berzelius' chemical work began firmly within the natural history tradition, in which the classification of chemicals depended on their origins rather than their composition. Starch extracted from lichens varied according to the species of lichen and tannin according to the species of tree. As late as the 1840s, Berzelius' textbook divided organic substances first of all on the basis of their natural or artificial origins, while also including classification based on composition and constitution.

In 1813 and 1814 Berzelius introduced the chemical notation using symbols from the alphabet and superscript/subscript numbers that chemists were using routinely by the 1830s and that would become a permanent part of modern chemistry. The formulae represented not only the elemental composition of compounds, but also what Berzelius defined as their binary constitution, that is, the internal combinations of elements within the chemical compounds. Thus, as Klein notes, copper sulphate was represented as $SO^3 + CuO$, rather than as Cu, S, and O separately. This approach derived from Berzelius' assumption of electrochemical attractions holding the internal parts together, an approach that worked well in mineral chemistry but was to prove problematic in organic chemistry.

In showing how the Berzelian formulae became paper tools and how they affected chemistry, Klein examines in detail the laboratory research of Dumas and Boullay, who in 1827 suggested classifying artificial and natural organic compounds together, rather than separately, by applying the categories of composition and binary constitution, which were being used in inorganic chemistry, to organic

compounds. It was in 1827 that Dumas and Boullay laid out their formulae to model the formation reaction of ordinary ether. In 1828 they published a new classification, constructed largely by analogy with the binary constitution of ammonium salts, which was based on recent work on alcohol, ether, alcohol derivatives, and fats. Klein presents a striking analysis of how Dumas and Boullay manipulated their Berzelian formulae in an algebraic manner, taking, for example, C_2 or C^2 to be the same as 2C, since they did not regard the symbols as corresponding to real physical atoms. We find Berzelius in 1833 using the process of division to manipulate formulae in such a way as to arrive at the expression C^2H^5 for one of the binary components of ordinary ether. The 'radical', or compound body that plays the role of an element in organic chemistry, was a hypothetical substance that chemists now sought experimentally, as in the case of the ethyl and the benzoyl radicals.

Algebraic manipulation changed in 1834, when Dumas introduced the concept of substitution to denote the equivalent replacement of one portion (or atom) of hydrogen by one portion of chlorine in the formation of chloral. Faced with the evidence provided by reactions like the formation of chloral, Dumas broke with electrochemical dualism and binary constitution as concepts for modelling organic compounds, adopting instead the notion of the chemical type. This type formed the basis for an entire class of carbon compounds with constitutions that remained fundamentally the same throughout substitution of new elements or radicals in the original body of the chemical substance. Many chemists adopted three such types – ammonia, water, and methane types – as the basis for all carbon compounds. The conceptualisation of the chemical type demonstrates the persistence in chemical thinking of the natural history category of the archetype or unitary type.

From the 1840s on, the study of model reactions – for example, substitution in key classes of substances such as alcohol, ether, and benzoic acid – became increasingly systematic and productive through the use of an expanding toolbox of paper formulae or model formulae. The model formulae suggested and encouraged specific tests for the existence of new substitution products that were first imagined on paper. Not everyone was content with the direction that organic chemistry was taking. As Klein notes, Liebig left the field of organic chemistry in 1840, while Charles Gerhardt and others worried that chemists were attaching too much meaning to "putting

down on paper, a bit more to the left or to the right, this or that symbol'' (quoted in Klein, p. 150).

Peter Ramberg's *Chemical Structure, Spatial Arrangement*, the first major study of stereochemistry since O. Bertrand Russell's *Stereochemistry* (1981), tells a good deal of the rest of the story about how the chemical community came to agreement over new forms for expressing chemical structure. They did this by extending the function of model formulae from the constitution of molecules to the configuration of molecules in space in order to provide explanations and make predictions. Ramberg argues that nineteenth-century chemists on the whole were pragmatic and cautiously optimistic in their use of formulae and that, in the course of the century, they transformed the purpose of their formulae from symbolic to iconic images. In his analysis, Ramberg focuses on the figures of J. H. Van't Hoff, Johannes Wislicenus, Victor Meyer, Emil Fischer, Arthur Hantzsch, and Alfred Werner, arguing that, apart from Hermann Kolbe's infamous ridiculing of Van't Hoff's and Wislicenus's promulgation of the three-dimensional tetrahedral carbon atom for explaining reactions in carbon chemistry, there were hardly any substantial controversies over stereochemistry and no alternative proposals. Kolbe's journal, the *Journal für praktische Chemie*, consistently served as an outlet for critics of new ideas in chemistry and became characterized as a ''salon for disgruntlement'' (quoted in Ramberg, p. 238), but to little effect. Ramberg emphasizes that there were three principal hypotheses in Van't Hoff's theory, which was published in Dutch, French, and German during the period 1874–1877. These were, first, the tetrahedral hypothesis, second, the 'free' rotation of an atom or radical about carbon-carbon single bonds, and third, the restricted rotation about carbon-carbon double bonds. Only the last two hypotheses excited much debate.

After explicitly using Klein's argument for the period from the 1820s to the 1840s, Ramberg discusses the introduction into chemistry of the concept and terminology of 'combining power,' 'atomicity,' or 'valence' following Edward Frankland's discovery of zinc methyl and zinc ethyl in 1849 and Alexander Williamson's synthesis of symmetrical and mixed ethers in 1851. In 1864, Alexander Crum Brown introduced a notation involving straight-lines radiating from a central atom, saying that this was to indicate the 'chemical' position, not any physical position, of an atom with respect to the central atom. However, Ramberg notes how Frankland's description in 1866 of the valence bond as a ''point of attachment' introduced an

unmistakably physical interpretation of the concept. Auguste Kekulé's 1865 benzene ring, or cyclohexatriene, aimed to represent the idea that the six hydrogen atoms in benzene were chemically equivalent and that any of the six could be substituted without preference, allowing Alexandr Butlerov to define Kekulé's structural formulae as models of chemical, not physical, position.

The nineteenth century witnessed a growing number of studies concerning the phenomenon of chemical compounds sharing the same composition, but exhibiting different physical or chemical properties, known as isomerism. Wislicenus was one of the leaders in this field in mid-century Germany, and his 1873 proposal that lactic acid behaves both as an acid and an alcohol because some of its molecules differ in the spatial arrangement of their constituent atoms came to be known as geometrical isomerism. This hypothesis inspired Van't Hoff's idea of the tetrahedral carbon atom as a specific spatial arrangement. This idea was probably also partly influenced by Van't Hoff's seeing Kekulé's hand-held molecular models with the brass rods attached to a representation of carbon pointing towards the four corners of a tetrahedron. This physical model allowed the representation of single, double, and triple bonds by engaging the ends of one, two, or three of the brass rods. While the tetrahedron hypothesis was frequently referred to as the Le Bel–Van't Hoff hypothesis, Ramberg argues that Le Bel's approach was significantly different and less influential, since he aimed more narrowly to explain the relationship between optical activity and asymmetry in molecular configuration, and did not offer diagrams, much less the sets of cardboard molecular models that Van't Hoff mailed out to colleagues and included in the 1877 German publication of his theory.

Drawing an analogy with Thomas Henry Huxley's relationship to Charles Darwin, Ramberg characterizes Wislicenus as Van't Hoff's 'Bulldog', since Wislicenus became a prolific and effective propagandist for the new 'chemistry in space' after Van't Hoff turned his attention to chemical kinetics. Wislicenus made systematic studies of the physical and chemical properties of molecules based on predictions from the geometry represented in paper formulae. Victor Meyer introduced the word 'stereochemistry' in 1889 and it caught on immediately, as did the word 'configuration' used by Aemilius Wunderlich in an 1886 pamphlet distinguishing space formulae from constitutional formulae. In 1890, Meyer and Arthur Hantzsch became embroiled in a debate over the best means to explain the properties of benzildioximes after Hantzch and his student Alfred

Werner introduced the idea that it was the stereochemistry of the carbon-nitrogen double bond, and therefore the stereochemistry of nitrogen itself, that was responsible for the different isomers of these compounds. However, as Ramberg notes, neither Meyer nor Hantzsch doubted that the ultimate, satisfactory explanation would somehow involve the three-dimensional geometry of the molecules.

Hantzsch was uncharacteristically innovative in organic chemistry in the 1890s in his incorporation of new methods of physical chemistry into the field. He published in the *Zeitschrift für physikalische Chemie* as well as in the *Berichte* of the German Chemical Society, which sometimes found him too speculative or hypothetical in his approach. Emil Fischer was more characteristic of the commitment to traditional chemical methods in organic chemistry while open-minded and innovative in his use of the new stereochemistry. Ramberg argues that it is incorrect to say that Fischer set out to establish the spatial configuration of carbohydrates, but rather that he should be seen as having recognised that the chemistry of carbohydrates suggested the applicability of Van't Hoff's theory, and as having predicted that four asymmetric carbons in mannose and glucose would allow up to 16 possible geometric isomers. Fischer then developed projection formulae by taking his simple linear formulae for the carbon backbone and showing configuration by literally squashing the flexible rubber carbon–carbon bonds of his models onto the plane of the paper formula.

With Werner, organic and inorganic chemistry became unified once again in model formulae. After suggesting a stereochemical model for nitrogen to Hantzsch in 1890, Werner continued to apply geometry to non-carbon atoms and developed a theory of the radiation of valence force from a central atom – such as a metal atom, for example – into an octahedral space in which the groups could arrange themselves. This was a model that enabled him to isolate optically active metal-ammines between 1911 and 1914. Ramberg concludes that there was a broad similarity between the thought experiments conducted by Werner in the 1890s and the manipulation of Berzelian formulae in the 1830s by means of which he aimed to determine which radical was shared by a given series of organic compounds. There was a similarity, too, between the use of sets of chemicals by the two chemists as exemplary models or standard systems for laboratory analysis. Just as geneticists would later concentrate on specially bred fruit flies or mice for their research, so chemists relied first on alcohol and ether, and later on lactic acid, unsaturated acids, and benzildioximes as their preferred experimental subjects. The parallel

with Robert Kohler's conception of the "model object" is hardly coincidental, since Kohler himself wrote in *Lords of the Fly: Drosophila Genetics and the Experimental Life* (1994), that the "'Standard' drosophilas were, like chemical reagents or physical instruments, constructed artifacts of laboratory life" (see Klein, p. 6; and quoted in Ramberg, p. 341).

Here, then, are two very fine, groundbreaking books in the history and philosophy of chemistry that persuasively present the impact of paper tools on the practice and evolution of organic chemistry, and of chemistry more broadly. They offer a convincing interpretation of these paper tools as symbol-systems that move between layers of graphical representation and iconic meaning, showing how what Kekulé himself called '*Modellformeln*' were used for predicting reactions and producing new chemical objects. Klein and Ramberg are thoroughly reliable in the technical presentations of this chemical work, as well as imaginatively analytic in their description and interpretation. They also demonstrate inconsistencies and changes of mind in these nineteenth-century chemists as they tried to sort out the epistemological meanings of what Berzelius called empirical and rational formulae. Most significantly, these authors show us in detail how chemists' theoretical practice guided their experimental practice.

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THE BIOLOGICAL BASIS OF BEING GOOD

Donald M. Broom, *The Evolution of Morality and Religion*.
Cambridge: Cambridge University Press, 2003.
Pp. xii + 256. £50 HB.

By Alex Rosenberg

This is a slim volume in every sense. In 229 pages of sparse text Donald Broom scratches the now familiar surface of evolutionary accounts of morality and religion. Many but by no means all of the usual suspects are trotted across the stage: a bit about the brain, a note about the relative uniformity of moral codes across cultures, and pretty soon we are into the standard story of the selective emergence of reciprocal altruism. Not much game theory, public goods or prisoner's dilemma, so this part of the book is not suitable for introducing more advanced work like that of Skyrms (*The Evolution of the Social Contract*). There is a page or so on Wilson and Sober (*Unto Others*) and group selection, but little connection to the vast issues in evolutionary biology and the philosophical controversies about sociobiology and evolutionary psychology that have raged over the last two decades There is a chapter devoted to a subject on which the author, a professor of veterinary medicine at Cambridge, should know a good deal: ''Biological capabilities needed for altruism and morality''. This is a subject that must surely be close to *a priori*: what is required is a capacity to recognise and remember past interactors, a theory of other minds, an ability to recognise opportunities for strategic interaction, and both the right emotions and enough control over them to make them useful in solving commitment problems. What we lack is a characterisation of how far down the phylogenetic scale such traits are to be found, what neurological apparatus subserves them, and to what extent differences among the degree to which organisms realise these traits can explain differences in their cooperative behaviours. Alas, either these are subjects on which there has been as yet little informative research and a good deal of glaringly


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obvious and largely *a priori* discussion, or else the author has not been able to locate it.

The discussion of moral theory – deontology, utilitarianism, etc. – is puerile, but then the work is not intended for philosophers. However, even for non-philosophers, the real defect is the author's tendency to move blithely from the metaethical to the ethical and back again without any qualm or even recognition of the difference. Here is an example.

> In many societies, various aspects of sexual behaviour are included in moral codes. Indeed, for many people the ideas elicited when reference is made to moral or immoral behaviour largely concern sexual behaviour. The argument presented here is that most sexual behaviour is not a moral issue at all and that, although some sexually motivated actions are immoral, in general the importance of sexual behaviour in morality has been greatly overstated. There are reasons why many people may include in codes for living some condemnation of some sexual behaviours which are not related, or are only indirectly related, to morality. Much of the sexual morality which is regarded as transgression of a code is in the code because it is of importance to guarding of the mate by males…
>
> Mate-guarding is in itself not morally wrong but some of the methods used can be questioned on moral grounds. (pp. 140, 144)

There is in these passages and surrounding them an easy shift from explaining mores – whether sexual or non-sexual – as conferring evolutionary fitness to assessing mores as morally wrong. But if sexual mores' claims to moral status are explained away by showing that they are merely fitness conferring, then why not draw the same inference about moral norms? The author never seems to notice that the evolutionary explanation of a norm is tantamount to evolutionarily explaining it away. So, he blithely carries on throughout the work endorsing various norms – for which he has pretty much already accepted undermining explanations – as valid, enforceable principles of normative value.

Actually the persistent movement back and forth across the metaethical/normative ethics divide should be unsurprising. Though he mentions G.E. Moore only to confuse the former's naturalistic fallacy with David Hume's injunction about not inferring normative conclusions about what ought to be the case from factual premises about what is the case, Broom does not notice the significance of this matter for his argument. This becomes particularly apparent when he turns to the evolution of

religion, where he endorses the institution on the grounds of its usefulness for enforcing morality.

Surmounting problems which William James could not, and which led Wittgenstein to invent the notion of a 'family resemblance concept', Broom gives us a necessary and sufficient condition definition of 'religion' (p. 165). And he argues that all religions embody a moral code which is central to their functioning. This, he holds., is because the evolutionary adaptation argument for a binding morality is too complicated for the plain man and "more easily comprehended principles are needed for any religion" (p. 169). To begin with, evolutionary adaptation does not justify morality even to smart people. It merely explains why people might believe it. But secondly, the claim reveals either a logical confusion or careless writing. After all, religion is said by Broom to arise because it fosters the community required for the reciprocal altruism that is evolutionarily selected for. So, it's not that religion needs morality, but rather it's morality that needs religion (as a cement for society). Most of the chapter is a mixture of *a priori* and pop sociology of religion. But the chapter comes complete with the author's advice to religions about how to avoid becoming extinct: (1) emphasise their moral aspects; (2) point out their moral commonality; (3) eliminate those of their teachings harmful to individuals within and outside the religion; (4) eliminate practices encouraging sectarianism; (5) encourage interest in other religions; (6) suppress those components incompatible with the teachings of science; (7) eliminate the supernatural features of the deity that conflict with science; (8) make components of the religion which support its moral code easier to understand among skeptics; (9) decrease dogmatic force of prescriptions; (10) exclude no one interested but unwilling to accept core doctrines (pp. 192–193). His holiness, Pope John Paul II will, I am sure, be glad of this advice. So far as I can see, it is a prescription for the evolutionary disappearance of an institution that both relies upon and breeds the sort of ingroup/outgroup distinctions required by optimal inbreeding theory. Accordingly, it is almost wholly incompatible with Broom's analysis of the evolutionary origins of religion.

In the last chapter Broom outlines and embraces a relatively enlightened Fabian view of the importance of social welfare and the likelihood that it will be fostered by generally egalitarian policies. That he should suppose for one minute that an account of the evolution of morality and the institutions that subserve it, even one as superficial and unoriginal as his, should underwrite these sensible

normative claims, makes it apparent that philosophers interested in metaethics or normative ethics can safely disregard this work.

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FROM DADA TO DEWEY

Robert P. Farrell, *Feyerabend and Scientific Values: Tightrope-Walking Rationality*. Dordrecht: Kluwer Academic Publishers, 2003. Pp. 247. EUR 90.00, US$86.00, £56.00 HB.

By John Preston

Robert Farrell's book is an explication of Paul Feyerabend's philosophy and defends it against many of the criticisms made both by sympathisers (like myself) and outright critics. The first thing to say about it is that it generally supplies careful readings of Feyerabend which should, in theory, give the lie to the way most of his critics represent his views. It does a good job of crediting Feyerabend with views that are plausible and worth defending, giving him a sensible alternative to the sort of 'Rationalism' that insists on trying to make sense of science in terms of context-independent methodological rules.

Farrell argues that the 'epistemological anarchism' of *Against Method* (1975) is not one of Feyerabend's views, but *merely* part of his *reductio ad absurdum* of 'Rationalism'. He works hard to give Feyerabend a single consistent *positive* position from the late 1960s to the late 1990s, although, as he is aware, this rather conflicts with Feyerabend's own claims to have been intellectually restless. In general, Feyerabend is seen here as wanting to *balance* Rationalism with 'anarchism', rather than as rejecting Rationalism *tout court*: "Feyerabend is not an irrationalist: Feyerabend can be seen as arguing against a particular conception of rationality, not against the very idea of rationality itself" (p. 71).

Two minor worries are that Farrell does not find enough in the texts themselves that might constitute Feyerabend's alternative theory of rationality (p. 43), and that Feyerabend was more attracted to, and more attached to, epistemological anarchism than Farrell allows. (Far more attention is devoted here to Feyerabend's remarks on anarchism than to his – equally important – remarks on 'dadaism'). It

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is much to be regretted that in the thirty-year period Farrell is concerned with, Feyerabend did not manage to set out his own conception of rationality in any concise way. Why did Feyerabend spend so much time explaining a view he did not hold, taking care to distinguish it from other views (such as scepticism and political anarchism), and why did he do so *little* to make his own positive view (values-based rationality) clear and explicit? Farrell admits that although we can "bring forth" (p. 161) from Feyerabend's writings the ideas of context-dependent and values-based rationality, Feyerabend never systematically explored their implications. I am less sure than Farrell that there's a single completable edifice in Feyerabend, since I can't see him as enamoured of any philosophical system. In these respects, therefore, I think Farrell goes further beyond Feyerabend's texts than other commentators (including myself). In response to such worries, Farrell admits that his interpretation is at some distance from the texts, but argues (rightly, I think), that it is legitimate to try to find a consistent position across different works if that position is also plausible and interesting. Even though I have my reservations about the position he outlines, I agree with him that it does indeed go some considerable way towards meeting these desiderata.

On this more important question of the plausibility of the views Farrell endorses here, Farrell's book has some important things to offer. I thought Chapter 7, on Kuhn's views on paradigms, rules and values, was the most successful chapter, and a genuine contribution to the explication and (more importantly) the development of Kuhnian views. It establishes certain parallels between Feyerabend and Kuhn, interpreting Kuhn as proffering a values-based alternative to 'Rationalism'. For Kuhn and Feyerabend, criteria of theory-choice function "not as rules (which determine choice) but as values (which guide it)" (p. 172). Four such values (comprehensiveness, empirical accuracy, fruitfulness, and testability) comprise a shifting (and probably incommensurable) plurality against which *all* inquiry is assessed (p. 203). These values allow us to recognise *both* that there are universal and atemporal factors in rationality *and* that we cannot understand the rationality of specific episodes without in-depth study via 'the anthropological method'. Whether or not this fits everything Feyerabend says, I entirely agree that it's a perspective worth exploring. Farrell's explanation of why this conception of rationality will favour empiricism over the other extreme, abstract Platonism, is one of the best parts of the book, giving Feyerabend a position which

is an improvement on the one I attributed to him in certain published work.

Farrell takes my own interpretation of Feyerabend's work to task several times, and some of his objections to it are well-taken. He suggests, for example, that Feyerabend's later works endorse a 'metanarrative' associated with general human rationality (against my reading, which interpreted them as more 'post-modernist'). I would be happy to be shown wrong here, and I think Farrell makes quite a lot of progress towards doing so. He also wants to show that Feyerabend's argument for pluralism follows from Popper's conception of empirical content, thus rebutting the objections of Larry Laudan and John Worrall, and obviating certain moves I made. Again, I really hope he has succeeded here, although I did not find his account of how rival theories increase empirical content clear enough to decide.

On the issue of Feyerabend's treatment of reality I thought Farrell less successful. He and I agree that in his last papers Feyerabend was trying to grapple with this issue without falling into the extremes of subjective idealism and metaphysical realism. Farrell, though, creditably thinks that Feyerabend was more successful in this than I do. He argues that although Feyerabend never explicitly defended such a view, a Deweyan version of non-reductive naturalism can be seen to constitute "an implicit structure in Feyerabend's philosophy" (p. 224), and that his ideas can profitably be placed within such a setting.

Some of the important views attributed to Feyerabend as a result (such as that science is only one approach to reality, and that science has objectionable reductionist tendencies) are certainly Feyerabend's, and absolutely fine by me. Others, though, such as the view that no area of experience and no science are epistemically privileged with respect to any subject-matter, need interpretation. The naturalism in question is supposed to imply Feyerabend's view that we can legitimately reason 'backwards' from ethical, political and social considerations to the nature of the entities of microphysics. This implication remains, in my opinion, wholly implausible. One does not, *pace* Farrell, have to be *any* kind of reductionist to insist that physics is our only way of finding out about (and hence the *authority* on) the microphysical aspects of things, and can legitimately overrule other suggestions deriving from how we would *like* the world to be, microphysically-speaking.

In conclusion, Farrell's book is one that anyone with an interest in Feyerabend should certainly read. It genuinely opens up a new way of reading Feyerabend's work that should constitute an important

position in the debate on 'the rationality of science'. I am depressed to report, then, that the standards of copy editing on display are lamentable. The publishers ought to be ashamed that they evidently could not supply a copy editor capable of excising misplaced apostrophes, or inserting missing ones, recognising that 'criteria' and 'phenomena' are not singular nouns, and correcting a wealth of grammatical errors such as failures of agreement in number.

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BIG COUNTRIES ASPIRING TO POWER AND INFLUENCE—VIA THE INTERNET

Marcus Franda, *China and India Online: Information Technology Politics and Diplomacy in the World's Two Largest Nations*. Lanham, MD: Rowman & Littlefield, 2002. Pp. xvii + 251. US$27.95 PB.

By Cong Cao

China and India are the most populous countries in the world. Both have rich histories and cultures, and both aspire to superpower status. Moreover, both Chinese and Indian elites are well-educated and hard-working, and they both excel in science and engineering. In recent years, the two countries have been extremely active in the pursuits of information technology (IT). A comparison of their strategies, developments, and aspirations in this area is bound to be interesting. With a combined total of 100 million 'netizens' – the number is less impressive than it looks when compared to the combined total population of 2.3 billion – China and India have the potential to become Internet powerhouses, which means that they will gain greater influence not only on the world IT market, but also in international relations and the global military order. Franda's book is a welcome effort to examine this important topic.

The book consists of 6 chapters (excluding the conclusion). The introductory chapter considers the role of the Internet in the aspirations of a modern nation. A second chapter provides an overview of the potentials of the two Asian IT giants. The rest of the book is divided into 4 chapters, with 2 of them devoted to each country: one background chapter portraying the Internet growth in that country, and the other analysing the country's ambition to become a superpower, in the Internet and elsewhere.

China and India have embraced the IT revolution in different ways. Oriented toward hardware, China is now the world's number one country in total telephone users and mobile users, and it has over the

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past few years witnessed significantly increased numbers of PC users and PCs connected to the Internet. Chinese firms are keen to make complete software products, either 'sinicising' foreign packages for domestic use or creating indigenous versions. India, in contrast, has benefited directly from the large-scale diaspora of its educated elite. The professionals of Indian origin in the Silicon Valley have created a computer software outsourcing boom in certain Indian cities, such as Bangalore (pp. 27–34). More recently, India has similarly taken advantage of the language ability of its elite workforce and actively participated in the 'business process outsourcing' market.

However, Franda's assertion that China has lagged behind India in software development is problematic. It is hard to say which model – China's or India's – is better, for each suits the particular situation of the country. The recent takeoff of China's Internet portals has derived strength from its domestic software vendors. For example, the Internet portal Sina.com is connected to RichWin, a software vendor, and Netease.com started out as a Chinese–English dual language e-mail system. The difficulty for Chinese to use their language in software programming has encouraged Chinese entrepreneurs to seek and eventually take a different path to the IT revolution. Not only is China ahead of India in building up an indigenous software development capability, it also has a more coherent strategy and better potential to achieve big results. In addition, compared with that of India, China's software development is better integrated with other sectors of high-tech industry, such as consumer electronics, PCs and telecommunications products.

Nevertheless, language and culture have always been an issue in the development of the Internet. Because of this, the politicians and technical elites of China and India have pursued and promoted the usage of their native languages in information technology. The two countries' aspirations to superpower status by way of the Internet also show in their efforts to participate in defining the agenda for its future development. The book examines China's active involvement in setting Internet-related standards and protocols (pp. 58–60). China's TD-SCDMA has become one of the three third-generation mobile communications standards approved by the International Telecommunications Union, and, as such, it may impact on the future of the Internet. Along a similar line, China has been campaigning for a new Internet protocol, IPV6, and a new wireless Internet security standard, the Wired Authentication and Privacy Infrastructure (WAPI). China's ambition is also reflected in its effort

to challenge the Wintel hegemony and to search for alternatives, such as locally-made chips and Linux-based software.

The book discusses at length the challenges that the Internet has posed to the governments of the two countries. As one of the few surviving Communist regimes, the Chinese Government cannot afford to be left behind in the worldwide information revolution. Its political leadership regards the Internet, along with other high technology, as the last chance to catch up in the new industrial revolution and knowledge-based economy, as well as a strategy to win the information war in the future. But, in the meantime, the Chinese Government hopes to maintain political control of its citizens. In recent years, the Internet has become a channel through which collective actions were organized (e.g., the 1999 Falungong protest in front of the Chinese Communist Party compound). For this reason, the Internet is under constant surveillance. Certain types of websites are prohibited from access, and the users of Internet cafes must surrender their identifications on visit, even though most of them are just interested in playing online games or looking for one-night stands. To a lesser degree, India has also put the Internet under a controlled environment.

The popularisation of the Internet has to do not only with *per capita* GDP and foreign direct investment, but also with such social factors as illiteracy rate, infant mortality, life expectancy, teledensity, and even television ownership. The different levels of social development in the two countries help explain why, despite India's advantage in language and software industry, it may be lagging behind China in the Internet industry. Domestically, both China and India are faced with the problem of unequal economic development. Each has a number of modern metropolitan cities, but the vast rural areas remain poor and isolated. Unfortunately, the book does not discuss this contrast of information 'haves' and 'have-nots'.

Given the considerable interest in the two Asian giants' development paths, Franda's comparative study of their ambitions in information technology is a timely contribution. The Internet provides a window to the national aspirations of China and India. And it may indeed be an avenue for these two countries to achieve superpower status.

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DEEP THOUGHTS ABOUT DIAGRAMS

Mark Greaves, *The Philosophical Status of Diagrams*. Stanford: CSLI Publications, 2002. Pp. 214. US$22 PB.

By George Gale

Mark Greaves' wonderful new book "is concerned with trying to explain a remarkable fact: for a particular set of theories which encode some of our highest standards for correct reasoning – the theories of expression and proof which are operative in geometry and technical logic – the representations which are currently sanctioned are uniformly sentential" (p. 2). Thus, although diagrams have figured centrally in the history of both geometry and logic, today diagrams have no status at all, or, at least, no positive status. How is this plunge in status to be explained? In the end, Greaves finds that:

> *The possibility of diagrammatic methods in formal proofs in logic and geometry has always been primarily dependent on the characteristics of the metaphysical and ontological theories under which they are carried out*. (p. 4, emphasis in original)

While this conclusion is perhaps unremarkable, it *is* remarkable that no one has ever tried to show it before. Greaves does, and well.

The book is divided into two parts: the first provides a compact and clear history of geometry and its underlying and evolving metaphysical systems, beginning with the ancients, especially Euclid, through Descartes, and ending with the sudden decline of diagrams in the late Nineteenth Century. Kantianism, the major foundation for geometry during that century, provided *a priori* sanction for diagrams as the working out in imagination of our intuition of space. As well as the discovery of non-Euclidean geometries, other problems, especially problems with intuitive notions of a curve raised by Wantzel and Weierstrass, challenged Kantian notions. Indeed, "by the 1880s, it was clear that reliance on geometric intuition as a trustworthy source of knowledge in mathematics had resulted in problems in analysis as well as in geometry" (p. 61). Finally, the concomitant need for an

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abstract philosophy of mathematics suited well the ''general emphasis in later nineteenth-century mathematics on revisiting foundational questions'' (p. 61). Pure geometry as it emerged from Frege, Pasch and Hilbert, provided a diagram-free, purely sentential notion of proof, which has lasted unto our own day.

In the second, and greater, part of the book, Greaves traces out the interrelations between philosophy, most especially metaphysics, and logic. Again, he begins with the ancients. To my mind, Greaves' very concise presentation of the connections, both structural and developmental, among Aristotle's logic, metaphysics, and science is the most elegant version of the task I have ever seen. That alone is well worth the book's price.

A major reason that diagrams did not figure until relatively late in the career of syllogistic logic lies in the fact that ''Aristotle developed the syllogistic in support of a specific conception about the scope and uses to which logic would be put, and that this conception naturally entails that sentences of a certain limited grammatical form be the principal targets of the theory'' (p. 88). That is, given Aristotle's metaphysics – especially the view of the relation between predication and properties from the *Categories* – and his view of what a science is, syllogistic can have only the form it does have, namely, categorical–sentential.

Because of these Aristotelian intentions, aside from some early oddities such as the efforts of Lull and Bruno, serious use of diagrams for reasoning (rather than just depiction) did not occur until Euler and the rise of extensional logic. As Greaves presents the case, it was Euler's decision to let his circles represent extension rather than the traditional intension of the terms that would ''make possible an entirely new sort of diagram type for logic, characterized by the use of regions to represent concepts'' (p. 122).

Neither Euler's diagrams (which had only limited power) nor his extensional interpretation made much of a dent in logic at the time. However, ''by the time of Boole and Venn, it is clear that a significant, although comparatively small, group of logicians had embraced an extensional, class-based interpretation of the component concepts in a categorical proposition'' (p. 127).

Greaves' discussion of developments in logic during the nineteenth century is delightfully economical. He covers, in turn, the development of Boole's symbolic logic, emphasising the expansions that it made in ''Aristotle's vision of the scope of logic along two main dimensions'', namely the expansion of types of propositional forms

allowable, and, secondly, the provision of "unified methods for reasoning with all of his targeted types of propositions" (p. 143). Venn subsequently developed his system of diagrams explicitly to provide a "technically undemanding introduction" (p. 147) to the major aspect of Boole's logic.

Yet it was the failure of Venn's system to represent "many types of non-universal Booliean claims" that led Peirce to produce the last great effort to construct a complete diagrammatic logic. Greaves' account of Peirce's system is a marvel of efficiency and clarity; since so few now know Peirce's system except via gestures, reading this section of Greaves' book should be a required bit of homage to a great thinker.

At the same time, the work of Frege and Hilbert in the foundations of mathematics and logic led inexorably toward the rejection of interpreted representational schemes for logic. According to Hilbert, this, at least in part, "had the effect of foreclosing the possibility of diagrammatic proof systems in logic whose representational techniques went beyond the relatively simple devices used in the *Begriffsschrift*" (p. 199).

It is no accident that diagrams lost their status in *both* geometry *and* logic in the late nineteenth century. Although, "for most of their histories, the stories of logic and geometry were separate and non-overlapping", the "general concern with rigor and the development of the foundations of analysis and modern algebra" (p. 203) brought the two stories together.

Greaves concludes with a point some might find surprising: "What we have shown is that the current attitudes about the legitimate representational formalisms in modern axiomatic systems result from a confluence of intellectual and historical currents in the late nineteenth century" (p. 204). In other words, it could have been otherwise. Are diagrammatic schemes still plausible in either geometry or logic? If Greaves is right, if our metaphysics changes, diagrams might very well come back into play.

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KNOWLEDGE IN A BOX

Anke te Heesen, *The World in a Box: The Story of an Eighteenth-Century Picture Encyclopedia*. Translated by Ann M. Hentschel, Chicago: University of Chicago Press, 2002. Pp. xii + 237. US$20 PB.

By Alan Rauch

Encyclopaedias immediately assume significance in our reckoning of knowledge, if only because of the amount of shelf space that they consume (at least before the advent of electronic encyclopaedias). The multi-volume encyclopaedia, a fixture of libraries and studies since late in the eighteenth century, has assumed an imperious quality that, for better or worse, exudes an aura of authority and reliability. But despite the impressive heft of works such as *l'Encyclopédie*, the *Britannica*, or Sir David Brewster's *Edinburgh Encyclopaedia*, the encyclopaedia, as a literary phenomenon, has not been examined with the same degree of scrutiny that, say, even conduct books have received. To be sure, the work of Robert Darnton and more recently Richard Yeo (in his *Encyclopaedic Visions*, 2001) has initiated an interest in the ideological and cultural significance of encyclopaedic literature; and this is a very rich area that deserves considerable attention.

Anke te Heesen's book, originally published as *Der Weltkasten* (1997), is a welcome addition to this literature. The 'World in a Box' was an unbound collection of 468 images produced for pedagogical purposes by Johann Sigmund Stoy (1745–1808). Stoy, a theologian turned publisher, located in the outskirts of Nurenburg, launched this project, 'The Picture Academy for the Young', after he moved into the city itself. We can be forgiven for not knowing about Stoy or his work, given "that neither the Academy nor Stoy himself had any appreciable effect on either the educational doctrines or the forms of artistic representation current in the late eighteenth and early nineteenth centuries" (p. 6).


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Why then does Stoy's Picture Academy warrant a book-length treatment? Bluntly put, the Picture Academy, te Heesen argues, "is an epitome of the Enlightenment" (p. 6). And while one might want to fine-tune some of te Heesen's broader claims about this, it is very difficult to resist her compelling argument for seeing the Enlightenment through the thoughts, actions, and ambitions of Johann Stoy. There were other compilations of knowledge for children in Germany, most notably Johann Bernhard Basedow's (1723–1790) *Elementarwerk*, which was also well illustrated. Unlike Stoy's 'box', Basedow's work was a conventionally bound four-volume set. Stoy's conflation of image, text, cabinet, and plaything in the Picture Academy distinguishes it from other works and it is through this unique synthesis of seemingly disparate elements that te Heesen finds an argument for the conceptual practices of knowledge in the Enlightenment.

Te Heesen's work is also critical for anyone interested in the codification of knowledge, whether during the Enlightenment or even in contemporary culture. By examining this 'picture box' in minute detail, te Heesen provides subtle but persuasive arguments that address the significance of images and, more important, the logic(s) underlying assemblages of knowledge.

Te Heesen foregrounds two areas that are critical to our understanding of the modes of dissemination of knowledge in culture. The first of these two areas is the role of children who, as the targets of moral and intellectual instruction, retain early habits and opinions into adulthood. The second area is the function of the image in the diffusion of knowledge. Many historians typically bypass images or dismiss them as merely descriptive at best, and ornamental at worst. Yet images provide an important entrée into encyclopaedic literature for a wide variety of individuals not generally considered when "readership" is addressed; these include children, the illiterate, and the incidental browser.

Stoy's 'Picture Academy' was built around fifty-two cards, each of which was composed of nine copperplate engravings; viewed weekly, the cards were meant to cover the span of a year. A central image, drawn from both the Old and New Testaments, dominated each 'tableau' (as Stoy called the cards) and was surrounded by eight smaller images intended to complement the scriptural scene by representing 'ordinary life', secular history (in two scenes), nature, trades, Aesop, mythology, and morality. The overall intention was to provide a work that was not only entertaining, but also moral

and instructive. A child could realise, through the 'Picture Academy', not merely his (the Academy was intended for boys) moral future, but also through another image, a possible future in trade or commerce. Stoy provided a text that explained the 'connections' between the illustrations but also encouraged users of the Academy to interleave blank pages on which children could write information or append illustrations of their own. In this sense, the Picture Academy, with its multiple images and Stoy's suggestions for insertions, emulated what we would now call a 'hypertext', that is a growing set of connections or links to information that can be non-sequential.

The diffuse quality of the Picture Academy, depending on one's perspective, was exacerbated or enhanced by the fact that it remained an unbound collection in a box. Certainly the serendipitous nature of the juxtaposition of ideas and images must have appealed to some educators and children. One of the great charms of encyclopaedic works – notwithstanding Coleridge's loathing of the method of arranging ideas and objects in encyclopaedias on the basis of the 'accident of orthography' – *is* the fact that completely unrelated concepts can be found side by side. This ''loose stream of knowledge,'' as Edmund Gosse observed, ''linking 'such subjects as Parrots, Parthians, Passion-flowers, Passover and Pastry, without any invidious preferences'', could also be a delight to the curious reader.

Alas, for Johann Stoy, any delight produced by his Picture Academy was overshadowed, according to te Heesen, by the perception that his ''compendium of knowledge (could) scarcely provide a serious basis for thorough elementary education''. Ultimately, Stoy's project, which he published privately beginning in 1782, was only a modest success. The Picture Academy was certainly a curiosity, but it was not used, te Heesen writes, ''in the manner that Stoy has intended''. Faced with financial reverses, Stoy shut down his press several years before he died, destitute, in 1808.

The strength of te Heesen's *World in a Box*, however, does not depend on the narrow history of Johann Stoy. The book extrapolates from Stoy's rich concept of 'A Picture Academy' to issues of Enlightenment understanding. Te Heesen situates Stoy's project in the larger scheme of other forms of collecting, sorting, and arranging knowledge, a move that complements the work of Lorraine Daston and Katherine Park in *Wonder and the Orders of Nature* as well as that of Marjorie Swann in *Curiosities and Texts*. The late Renaissance tradition of *Wunderkammern* emerges again during the Enlightenment

in remarkable collections of artefacts, including those of Hans Sloane and Ashton Lever, assembled for private and, occasionally, public consumption. The Picture Academy thus becomes a metaphor for what te Heesen calls a 'spirit of order', and a desire for descriptive and thorough taxonomies of the kind proposed by Linnaeus, the central icon of eighteenth-century collecting and taxonomy. Moreover, the organisational structure of cabinet-based learning was successful, te Heesen suggests (here drawing on Frances Yates's *The Art of Memory*), because it trained the memory and offered a mnemonic structure for recalling the knowledge that one had acquired.

Te Heesen also argues persuasively, though not extensively, for the mnemonic aspect of the Picture Academy. The organisation of materials in Stoy's clever arrangement of images was intended, she writes, "to erect a catalog inside a child's mind by which to organise collected scraps of knowledge" (p. 179). Whether images are absolutely necessary to this process is not absolutely clear, but the process itself is engaging. Turning to Gosse again, he reflected that encyclopaedic facts "clung to odd cells at the back of my brain", which, he wrote, "I can only trace back to the *Penny Cyclopaedia* of my infancy". Thus, the very absence of logical order in terms of the arrangement of 'facts' seems to serve readers well, if somewhat haphazardly, with respect to the retention of certain bits of knowledge. Many readers, in fact, considered the absence of order a critical element in the textual energy and the appeal of encyclopaedias. Indeed, whether from a literary or a cognitive perspective, the function of 'organisation' in encyclopaedias (or the lack thereof), deserves more serious consideration by future scholars of encyclopaedic knowledge.

On occasion, te Heesen attempts arguments that are beyond the scope of her study. She writes, for example, that the conceptual backdrop – illustrated by Stoy's Picture Academy – "explains the pedagogical approach of encouraging pupils to collect and of employing the box as collection's essential material basis" (p. 185). The literalising of the 'box' in this speculative aside is not convincing. Nor does te Heesen make it clear how this pedagogical method came about, to what extent it was employed, and in what contexts it was actually used.

Still, this is a minor point, and te Heesen does remarkably well in finding resonance in Stoy's finally not-so-eccentric version of the encyclopaedia. By looking at this unusual but fascinating example of eighteenth-century encyclopaedism, te Heesen is able to suggest a

wide range of meanings for the way that knowledge has been, and continues to be, codified, organised, preserved, and imagined. What is particularly provocative about te Heesen's work, particularly for readers old enough to conceive of encyclopaedias as hefty tomes on the shelf, is the notion that, in this electronic age, codifications of knowledge strongly privilege, once again, images over text. Moreover, computers offer a kind of portability of knowledge that might well have pleased the innovative mind of Johann Stoy. Computers may not figure at all in this engaging work, but for those who are interested in the impact and conceptual implications of having the world in an 'electronic box', I can think of no better place to start than te Heesen's book.

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SCIENCE AND THE SELF IN VICTORIAN SOCIETY

George Levine, *Dying to Know: Scientific Epistemology and Narrative in Victorian England*. Chicago and London: University of Chicago Press, 2002. Pp. xi + 326. US$45, £31.50 HB.

By Rosaleen Love

The phrase 'I'm dying to know' is usually understood as a paradoxical verbal conceit. I might say I am dying to know the latest gossip, but at the same time I know that I wouldn't really be prepared to hazard my life for vulgar curiosity. Besides, I am fairly sure I cannot know anything after I am dead. My twenty-first century sensibility interprets the phrase 'dying to know' as dead metaphor.

What if I lived in nineteenth-century England, and was of a more sombre intellectual and moral bent? George Levine takes the phrase 'dying to know' as a key to understanding the notion of objectivity in Victorian science and literature. What if the call for detachment from desire, prejudice, and interests should promote an ideal of self-abnegation and self-sacrifice, even unto (metaphorical) death? In the search for knowledge of nature, society, and self, the interpreting self is always present, a self which should be suppressed to let nature best speak for itself, according to the Victorians in Levine's study. Such acts of self-repression, Levine suggests, are like a death, ''if not dying literally in the body, dying in the sense that all those contingent qualities that constitute life for everyone are displaced'' (p. 15).

'Dying to know'. The reader is at first sceptical of the apparent 'one phrase fits all' approach, suspecting it to be a somewhat unnatural, indeed forced literary organising device. Levine, however, is a persuasive enthusiast for his utopian epistemological pursuit. His range of sources is impressively cross-disciplinary. Chapters examine the following themes: 'the narrative of scientific epistemology'; 'the epistemology of science and art'; 'autobiography as epistemology'; 'women and scientific autobiography'; and 'objectivity and altruism'.

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Authors subject to close reading of their texts include scientists Charles Darwin and John Tyndall, novelists George Eliot and Thomas Hardy, statistician (and novelist) Karl Pearson, art critic William Pater, and more. The irony that these were men and women of considerable egotistical drive is not lost on Levine. To be aware one is practising self-effacement is to be aware that one is aware of one's loss of self, and just a little self-congratulatory with it.

For Victorians, science was often portrayed as revealing harsh and brutal truths, showing nature to be coldly indifferent to human aspirations. The trope 'dying to know' is one way Victorians interpreted the progressive decentring of the human. Levine's index marks some fifteen shades of meaning of the negation of self, from self-alienation to self-surrender. Fourteen of these terms have negative connotations of loss. Only one term, self-surrender, hints at the sense of joy that is to be found in intellectual work, the sense of going with the flow of discovery, and surrendering into mental processes.

Levine illustrates how many epistemological narratives take the shape of the quest story, where the seeker undertakes arduous tasks that require great self-sacrifice, in order to attain the goal of truth. John Tyndall wrote: "the sacrifice of self, this upright determination to accept the truth, no matter how it may present itself – even at the hands of a scientific foe, if necessary – carries with it its own reward . . . when a man consent to lay aside his vanity and become Nature's organ – his elevation is the instant consequence of his humility" (p. 15). Levine analyses this and similar examples of the narrative of self-effacement in his chosen authors, with the intention of showing the stern Victorian ideal remains crucial to human experience. Moving beyond the limits of the self is a condition, not only of scientific knowledge, but also of altruism and community responsibility.

Levine may have begun his personal epistemological and moral quest in puzzlement at the paradox of 'dying to know', with a strong sense of something missing from 'an imagination of knowledge that requires death for its acquisition'. At the end of his book, Levine is convinced that the 'dying to know' narrative still shapes the narrative of modern science, but with a new gloss. What matters, he concludes, is "what avails for life": "In the end, there is no escape for it: we are all of us dying to know, but now dying to know how not to deny; how not to resign ourselves to the inhuman in the quest for the knowledge that might humanise; how to be objective and altruistic at the same time" (p. 283).

Dying to Know is a welcome addition to a broad-based cultural studies that, as a matter of course, embraces science in its definition of culture, and examines science, along with literature, as morally serious activities.

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THE OPEN (CRAFT) SOCIETY

Pamela O. Long, *Openness, Secrecy, Authorship: Technical Arts and the Culture of Knowledge from Antiquity to the Renaissance*. Baltimore and London: Johns Hopkins University Press, 2001. Pp. xiii + 364. US$57.00 HB.

By Margaret J. Osler

Pamela Long has written an extraordinarily good book on the development of ideas about intellectual property from ancient times to the Renaissance. The great strengths of this book lie in the wide range of sources Long uses and her thoughtful engagement with earlier studies. Emphasising actors' categories and assumptions within local contexts, Long shows how these categories and assumptions were taken up and used by later authors in different contexts. Because of its long chronological and broad geographical scope, the book reveals changes in disciplinary categories and changes in the social status of these disciplines that are more difficult to discern in studies focusing on shorter temporal periods or narrower geographies. The book contributes to our understanding of the themes she addresses as well as to a deeply nuanced understanding of the interactions among science, technology, and modes of literary production – all treated within changing social contexts. Her evidence leads to revisions of a number of common opinions.

Contradicting received wisdom, Long argues that craftsmen were not particularly concerned with guarding the secrets of their trades. In a detailed consideration of openness and secrecy in classical antiquity, she notes that there is little evidence of secrecy regarding technological developments. In Periclean Athens, there was a general openness about technical matters. Later, among the Romans, technical treatises were displayed at court. Their authors dedicated them to men of high social status, in hopes of gaining their patronage. This lack of secrecy is evident even in discussions of the technology of weaponry as well as civilian technologies.

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Social changes in late antiquity led to an increasing emphasis on private life and the emergence of mystery religions. One aspect of these developments was the proliferation of esoteric and magical writings such as found in the Hermetic corpus. The authors of these treatises, as well as the Neo-Platonic philosophers, created a distinction between the initiated and the uninitiated, stating that deep wisdom and magical practices should be revealed only to initiates. Throughout her book, Long argues that it was these esoteric traditions, rather than the crafts, that emphasised secrecy.

In the earlier Middle Ages, craft procedures and ingredients were handed down in books of craft recipes that were copied in *scriptoria* and circulated widely. With the rise of towns in the later Middle Ages, engineering, architecture, and painting became important to the display of power by the new urban elites. Engineers, architects, sculptors, and artists wrote books describing their trades partly in the pursuit of patronage. By the fifteenth century, the number of treatises increased, and the range of topics expanded. Authors of these treatises were a varied group, including artisan–practitioners, physicians, and learned humanists. Their motivations for writing books were varied. On the one hand, they wanted to display their own cleverness; on the other, they sought patronage. Typically they dedicated their books to potential or actual patrons. One far-reaching consequence was that certain arts – especially painting, sculpture, and architecture – experienced a rise in status, coming to be considered liberal arts and in some cases fine arts. Similarly, some crafts came to be considered forms of knowledge that could be understood by rational or even mathematical principles.

These developments are reminiscent of Edgar Zilsel's sociological thesis, published in the 1940s, according to which the roots of the Scientific Revolution lay in the encounters between scholars and craftsmen in the shops of printers. In contrast to Zilsel's rather superficial and reductionist account, Long's contextualised consideration of these practices provides a far more nuanced and complex understanding of the relationships among the many groups involved in the production of varieties of knowledge.

In a fascinating chapter on the esoteric traditions of the Renaissance, Long demonstrates the tension between secrecy and openness among alchemists, Hermetic philosophers, Paracelsians, and practitioners of natural magic. Alchemy, with its obscure symbolism and frequent use of pseudonymous authorship, continued to value secrecy in an effort to confine the mysteries of its practices to the adept.

Although the Hermetic tradition drew heavily on ancient Neo-Platonism, publication of Hermetic texts by Marsilio Ficino and their further elaboration by Giovanni Pico della Mirandola, Cornelius Agrippa, and Giordano Bruno led to the wide diffusion of these ideas. Paracelsus' application of Hermetic and alchemical ideas to medicine, emphasised the utility of magical practices, an important motive for openness in what had been traditionally secretive traditions.

Long concludes her book with two lengthy chapters on openness and authorship in mining, metallurgy, the military arts, painting, architecture, and other arts. In an epilogue, she briefly brings the story into the seventeenth century with a consideration of attitudes within the Royal Society and the French *Académie des Sciences*.

Openness, Secrecy, and Authorship is a masterful study that demonstrates the value of careful interdisciplinary scholarship. Avoiding the positivist distinction between science and technology as well as ideological and reductionist assumptions about the relationship between knowledge and the material foundations of society, Long demonstrates how craft traditions contributed to the history of human thought. She provides insight into the shifting boundaries between craft and learned cultures, the relationships between scholars and craftsmen, and the important role of patronage and courtly culture in these developments. Because she reads the products of learned scholars as well as the archival records of unlettered craftsmen, her book contains an unusual depth of knowledge and sophistication of insight.

This book is a model of a new approach, successfully fusing serious consideration of social context with a thoughtful understanding of the intellectual developments involved. It should be required reading for scholars and graduate students in the history of science, technology, and intellectual history.

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BENEATH THE SURFACE OF RUSSIA'S SUBMARINE TECHNOLOGIES

Robert Moore, *A Time to Die: The Kursk Disaster*. London: Doubleday, 2002. Pp. 271. £10.99 PB.

By Wendy Varney

I should warn from the outset that *A Time to Die* is not written from a history and philosophy of technology viewpoint. Nevertheless, there is plenty in this book to whet the appetite of anyone interested in the mix of technology, politics and culture. Science, a failing economy, espionage, diplomacy, deception, and national pride all played a part in the *Kursk* disaster.

Written in a journalistic style, the saga that unravels is clear and informative, with enough detail of the technological aspects to throw light both on why the *Kursk* was the pride of the Russian fleet and how technological advancements that are not matched by attention to social aspects have calamitous consequences.

At the root of the disaster, in which 118 submariners died, was the concentrated form of hydrogen peroxide known as high-test peroxide (HTP) used as propellant in the *Kursk*'s torpedos. In contemporary society we hear much of technology transfer, its benefits, sometimes its ineffectiveness and sometimes its poor consequences as technologies get superimposed lock, stock and barrel on societies with vastly varying environments and cultures. Such concerns are warranted. Less often discussed are the cases where knowledge of technological advancements should have been shared but were not, often for reasons of military or corporate secrecy and sometimes for reasons of national arrogance. It is hard to know why the Russian Navy had not recognised the dangers of using HTP as a propellant, long known by the British Navy which ceased its use following HTP's crucial contribution to an explosion killing 12 British sailors in 1955, but certainly the *Kursk* submariners were victims of a lack of knowledge or lack of its application.

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In many other aspects the *Kursk* was state of the art and was described by its designers as unsinkable, just as, ironically, the *Titanic* had been described. It was also among the largest attack submarines ever built. Most notable among its celebrated innovative features was its double hull. The inner hull was made of thick, high-alloy steel, providing strength, structural stability and a high likelihood of surviving a collision or torpedo attack. Little did its designers expect the explosions which would doom it to come from inside, not outside, the submarine. The outer hull was much thinner, was hydrodynamic and designed to resist corrosion as well as having a lower magnetic signature. This, along with its clean shape, reducing turbulence, made detection difficult, an advantage in war games and espionage but a significant drawback when the submarine eventually went missing.

Similarly, the rubber tiles surrounding the outer hull for the purpose of giving the *Kursk* a low acoustic profile, proved a hindrance to detection and therefore rescue. As Moore notes, the accident, in which the nuclear reactors and nuclear weapons remained unscathed, was simultaneously the *Kursk*'s designers' "worst nightmare and most remarkable achievement" (p. 37). It is sobering to think how much worse this disaster might have been.

It seems that all safety precautions and mechanisms were confined to the submarine and were mostly damaged from the two explosions. The force of the first explosion overwhelmed the automatic safety systems, most importantly the sensors and release mechanisms of the emergency buoy which, had it been launched, would have made location of the submarine a quicker and easier task.

Despite a number of clear signals that something had gone wrong, everybody in the vicinity ignored these. The crew of the spying *USS Memphis* felt the violent explosions but did not want to confirm their uninvited presence to the Russians. The Russian submarine *Karelia* was also rocked by the explosions but its crew, not privy to aspects of the naval exercises being conducted, assumed there was an explanation. When the *Kursk's* planned missile firings did not take place, once again naval officers aboard the *Peter the Great* and its support ships did not raise the alarm. Moore claims it is part of the Russian culture not to probe or point to problems. "Even in tsarist times men were deterred from raising troublesome issues" (p. 45). Long periods of Stalinism and/or secrecy had not mitigated this.

The problems did not end with the wasted time and lost opportunities to locate the *Kursk* quickly and easily. The search and rescue infrastructure, a less glamorous part of the navy's image, had suffered

from lack of funds and was completely inadequate to the task. Political paranoia, secrecy, fear of losing face and an onerous bureaucratisation combined to ensure that there would be no survivors when an international team finally reached the *Kursk* eight days after it had plunged to the bottom of the Barants Sea. Even then the system kept bumbling along on its perverted path of damage control. Few of those who saw the instant sedation of a grieving mother in the midst of her complaint about the half-hearted efforts to save her son on the *Kursk*, will forget that scene.

The layers of culture, technologies and techniques and the level to which they are intertwined are one of the most interesting aspects of the *Kursk* story. Another is the comparison between the high-tech nature of the submarine and the archaic nature of the back-up systems, which were in a shambles. On occasions the crew did not even get paid, though that is not an uncommon occurrence for workers in contemporary Russia. One lucky member of the *Kursk* had been left ashore by the captain specifically to collect the crew's salaries on pay day before the Navy could divert the money elsewhere (p. 15).

Another notable aspect is how problems become exacerbated in the face of cover-ups. A feature of this is the lag time between disasters in Russia and the administration's admission that there is a problem. This was true not only with regard to the *Kursk* but with other disasters such as, for instance, the Chernobyl nuclear accident where local residents suffered much greater exposure due to the delayed response by authorities who were more concerned with playing down the situation than with dealing with it.

Politics has a way of embedding itself into technologies and so it has been with Russia's pursuit of submarines which symbolise strength, might and advancement but are backed by little in the way of broad emergency systems and whose crews appear to matter little to administrations who seek primarily to cover themselves. Perhaps the comfort that the dead cannot criticise works subconsciously among some leaders.

The book has some flaws. A few more insights into the risks involved in having nuclear powered submarines carrying nuclear weaponry around the world's oceans and strategic seas and playing war games at the expense of the world would have set this in context. Also, while Moore is critical of the Russians, and rightly so, there seems to be an underlying assumption that the actions of the US spy ship were beyond reproach. Spying is much murkier than that.

Although the ends are not always tied up explicitly, those interested in social aspects of technology will be intrigued by the story of the *Kursk*. The book makes a good supplement to Loren Graham's *The Ghost of the Executed Engineer* (Cambridge Mass, 1993) which lays out many of the political and cultural priorities that were still in evidence in this more recent disaster.

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THE POWER OF ENLIGHTENMENT?

Giuliano Pancaldi, *Volta: Science and Culture in the Age of Enlightenment*. Princeton and Oxford: Princeton University Press, 2003. Pp. xv + 381. US$35.00 HB.

By David Philip Miller

This is a fascinating treatment of the scientific work, life and enduring significance of Alessandro Volta. It provides a detailed understanding of the relationship between Volta's electrical researches and the local, national and international contexts within which he worked. Developing that understanding is a major focus of the book. There is, however, another preoccupation: finding a methodological *via media* between what Pancaldi regards as the unviable extremes of realism on the one hand and of constructivism on the other. Interesting as this preoccupation is, it does mean that those hoping to find a full account of Volta's life, or even of the full range of his scientific activity, will be disappointed.

Pancaldi begins by sketching Volta's early life, his education, career, and emotional history, linking all these to his investigative style as a natural philosopher. We see the models of the man of knowledge that Volta was exposed to through his family (part of the minor aristocracy) and his education and how he reacted to these exemplars. Pancaldi traces Volta's awakening to the wider possibilities of national and international networks beyond Como in Lombardy where he was born. Volta's trajectory into local public service and then to a professorship at the University of Pavia is also examined.

Pancaldi sees tensions everywhere, between tradition and innovation in the roles that Volta took on, and between the role of the theoretical natural philosopher that he initially aspired to and that of inventor of electrical machines that he was more readily granted by the network of European philosophers. The book then moves to consider the broader context of the Italian scientific community of which Volta was a part. Pancaldi deploys a prosopography of those publishing in academic proceedings and scientific journals in Italy

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and a survey of visitors' perceptions of institutions to produce an overall characterisation of Volta's 'local' context and the nature of his relations with it. It appears that in the Italian states natural philosophy was pursued almost entirely within state educational and bureaucratic institutions. Pancaldi argues that lack of economic dynamism and neglect of popular education in the Italian states narrowed the possibilities. Many institutions were perceived by visitors as still dominated by courtly show and as remote from the incipient but weak sources of demand for useful knowledge. This was the context in which Volta's career was shaped and which stimulated also his search for wider contacts and reputation. Two chapters are then devoted to Volta's early electrical researches, his development of the electrophorus and his wider attempts to construct a science of electricity. We then learn of the cosmopolitan network of which Volta was becoming a part as his European-wide reputation began to grow. The jewel in the crown of that reputation was the invention of the battery which is analysed in detail in the next chapter, with a further chapter dealing with its appropriation or reception.

The final two chapters widen the focus considerably. The first looks at the creation of Volta as a scientist—hero, his depiction in works of art, and the celebration of the hundredth anniversary of his death in Fascist Italy in 1927 by a large international conference of physicists. A rather unusual final chapter, almost Ben-Davidian in character, uses the case of Volta as a thin pretext for an argument about the importance of diversity and pluralism to scientific and technical innovation.

The centrepiece of *Volta* are those chapters dealing with Volta's development of the electrophorus and the battery, or voltaic pile, and tracing the 'reception' of those devices by the wider natural philosophical community. The fact that Pancaldi chooses to treat the development and the reception of instruments separately, at least analytically, indicates that he does not want to buy into the constructivist notion that these are in fact inseparable processes. Pancaldi does employ what looks like the idea of interpretive flexibility, beloved of constructivists. For example, he discusses a variety of constructions of Volta's battery by Volta himself and by other electrical practitioners. Nevertheless, he insists at the same time that we can, and should, recognise what he calls the 'basic identity' of the 'instrument itself' (p. 254). Pancaldi recognises that manipulations of instruments, and accounts of what is going on in the experiments that use them, interpenetrate to a remarkable degree: he has Volta moving

back and forth between "theory and the instrument-displaying-theory-at-work" (p. 108). However, he maintains that at a practical common-sense level it was possible for people to fairly readily replicate apparatus and experiments, to construct the *same* instrument to work with.

In Pancaldi's view, Harry Collins makes the process of replication unnecessarily problematic. The crux of this issue is the same as the problem of residual realism. Constructivists of a Barnesian stamp do not deny the existence of a real world that is the object of scientific inquiry, or of a real instrument. Yet the answer to the question of what difference this residual realism, or this residually real instrument, makes to the examination of scientific meaning is 'not much'. In my view it is Pancaldi's exaggeration of the extreme localism implied by constructivism that drives him to an assertion of residual realism. The latter is not needed to support his otherwise excellent account of Volta's work and his interactions with other natural philosophers across Europe. Pancaldi's apparent belief that such interaction is in principle impossible within a constructivist interpretation of knowledge keeps alive a debate as futile as that over Kuhnian 'incommensurability'. Theoretical debates about instruments and experimental practice, whether based in Bachelardian style 'hardware-discourse couples', or in Pickering's 'mangle of practice', have moved on considerably from the way the problem is set out here.

To a significant extent, however, we can appreciate the value of Pancaldi's historical achievements in this book, independently of these contentious methodological and theoretical issues. His account of the range of roles potentially available to Volta and the way in which he navigated between them is excellent. Volta's early attempts at producing the high theory of the natural philosopher did not meet with much approval and Volta adjusted, in his public utterances at least, towards a more modest role as developer of instruments and associated experimental repertoires. This is skilfully and subtly handled. A second key historical achievement lies in Pancaldi's account of Volta's development of the battery. He shows how, very late in the day, the theoretical, conceptual and experimental framework within which Volta was working was traversed by William Nicholson's model of the electric fish. Volta saw the latter in a novel way and this was the proximate cause of his invention of the battery. Pancaldi is at his best when dealing with the complex shaping of Volta's experimental activity.

As the sub-title of the book suggests, Volta's activities were framed by Enlightenment ideals and participation in the 'Republic of Letters'. These were not, as some of us would have it, elaborate rationales for activities best explained in terms of a calculus of interests. Rather, the ideals were, according to Pancaldi, themselves a key impetus to activity, stimulating what Pancaldi calls the "imitation–competition game". Although the conduct of this game is not as fully articulated as it might be, this historical approach is presumably linked to the central idea of the concluding chapter – that scientific and technical creativity flourishes in circumstances that encourage imitation and competition. Pancaldi argues that Volta's creativity was made possible by the diversity of sources of support for it and by the pluralist competition of European states in the field of natural philosophy. The latter condition kept under control the Napoleonic forces' attempted annexation of the symbolic capital represented by Volta's discoveries. In more recent times it also presumably constrained the attempts of the Fascist regime to link the celebration of Volta's anniversary in 1927 with their own agenda. Pancaldi's point appears to be that whilst Volta could be annexed at a general symbolic level, his actual scientific accomplishments remained untouched in this process.

Pancaldi characterises his work as steering not only between realism and constructivism but also between pro- and anti-Enlightenment attitudes in modern scholarship. Those not persuaded of the necessity for, or value of, such *via media* will have problems with his larger claims. *Volta*, nevertheless, proves to be a very stimulating work. In its central chapters in particular it lays a claim to the careful attention of all future researchers into the achievements of this remarkable natural philosopher.

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A MIRROR FOR MEDICINE: THE PAST REFLECTING THE PRESENT?

Margaret Pelling, *Medical Conflicts: Patronage, Physicians and Irregular Practitioners, 1550–1640*. Oxford: Oxford University Press, 2003. Pp. xvi + 410. £65 HB.

By Susan Hardy

The heading of this review was originally going to be ''Back to Basics'', a title indicative of my immediate reaction upon receiving (not even opening!) Margaret Pelling's new monograph. Somehow, just by sitting on my desk, the book exuded competence and trustworthiness. In a period when the ''minute and marginal'' matters of medical history continue to expand themselves exponentially into book length studies it would, I felt, be a pleasure to immerse oneself in the solid results of such erudite archival archaeology.

However, as so many generations of students have found to their dismay, the contents of books do not somehow infuse themselves into one's consciousness by a process of osmosis: some careful reading of the lines is required before contemplating the process of reading between them; but, as, carefully, I read, a feeling of recognition crept over me and I found myself pondering, for example, the amazing coincidence of finding the ''active, health-seeking, communicative patient'' (p. 338) alive and well and living in sixteenth-century London. Now of course this example of finding the present in the past might be seen as a welcome move away from the patronising school of historiography which depicted strange, simple people in quaintly unfamiliar situations; moreover there is plenty of the 'past is a different country' evident in this work. Nevertheless the thought did at least flash across my mind that finding versions of those of whom we approve in the past continues to be a very satisfactory historical pursuit.

Having said that, it should be noted that this book is not for the faint-hearted; nor indeed is it for speed or skim readers, in whose

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ranks I can usually be found; nor is it for those who may be looking for a little enlightening bedtime reading. It is one of those books in which the footnotes frequently take up more than half the page; but, of course, this evidence of scholarly infrastructure alerts readers to the fact that they are in the presence of a serious source: one that can, with confidence, be used to construct a pattern of the past in the 'bottom up' manner which we medical historians continue to prefer.

Refreshingly, and even unusually, the title of the book tells the reader exactly what it is about: healers of one sort or another and their patrons and patients in early modern London. Those who are familiar with this topic and period will be aware that there are many still unanswered or even unaddressed questions concerning who went to see which practitioner and why. To this end Margaret Pelling has used the records of the College of Physicians of London to examine the attempts at control of medical practitioners within the city, although, as many readers are no doubt aware, previous studies have already claimed to do just this. Pelling herself notes that ''the College has not been neglected by historians'' (p. 2) and indeed one of its contemporary chroniclers, Harold Cook, introduced the initially illuminating but now somewhat overused phrase 'medical market place'. Rather, Pelling has an agenda other than that of producing an institutional history or a series of biographical studies of 'eminent' doctors, most of whom, she seems to feel, have had exposure unrelated to their importance. Her task, as she sees it, has been to examine the actual functioning of the College while attempting to avoid the two-tiered historiographical trap of taking the physicians ''at their own valuation'' (p. 2) and of ''reading back'' notions of professional status from the point of view of the present, although, as I have noted above, these traps are more easily spotted than avoided even by the most experienced historians.

The method of this study is to use evidence gleaned from the College's ''exclusionary and punitive'' approach (p. 1) to highlight the supposedly illicit trades and crafts of healing that were being pursued concurrently in the city. In other words, disaffected diatribes from the literate and licensed are being used, not to reaffirm their status but rather to indicate the nature and extent of the 'opposition': if physicians were complaining there must have been something or someone to complain about. Of course, as Pelling herself points out, despite this fairly sophisticated historiographical approach, there is always a gap between ''intention and actual events'' (p. 5) and the gap threatens to become a chasm when the deeper motive of the study is

revealed as an attempt to categorise the ubiquitous horde of 'irregulars' in a way which does not define the medicine and practitioners of this period simply "by a process of exclusion, defining what it was not" (p. 333). Moreover Pelling's claim is that all of the previously understood divisions of regular and irregular medical practice (the term she prefers is 'medical occupations') still did not include all of the 'middle people' between the definitely regular and the blatantly quackish.

While not wishing to criticise a work for failing to be that which it never claimed to be (a situation regrettably familiar to many authors) it would seem that there might be easier ways to 'get at' what was actually going on in the medical market place (a useful phrase after all) than spending years in what initially appears to be a type of historical second guessing. Pelling herself makes the point that many of the so-called irregulars were by no means illiterate ignoramuses and therefore must have made their presences and practices known and available to the public by some means or other. She points out that medicine "was above all a portable skill in universal demand" (p. 333) while the 'Biographical Index of Medical Practitioners, 1500–1640' (Appendix A) which was "fundamental to the present study" (p. 344) has obviously made use of many and varied primary and secondary sources, all of which would lead one to expect rather more evidence of this kind in the book itself.

Enough of academic quibbling: this is, as one would expect of a title in the *Oxford Studies in Social History* series, a scholarly, thoroughly researched work with an admirably thorough bibliography and an excellent index; essential reading I would suggest for anyone with any research interests in this period.

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SENSIBILITY AND SCIENCE

Jessica Riskin, *Science in the Age of Sensibility: The Sentimental Empiricists of the French Enlightenment*. Chicago: University of Chicago Press, 2002. Pp. ix + 338. US$25.00 PB.

By Jeff Horn

Jessica Riskin's book can be compared to one of Shakespeare's lesser-known comedies put on at Stratford-on-Avon by the Royal Company. It is well conceived, clever in parts, and nicely illustrated with relevant images, but most of the audience will go home somewhat disquieted, thinking they should have enjoyed the experience more. In the end, they will decide they are disappointed, wishing they had gotten tickets to a play with a better plot. Although some will find Riskin's book a major new interpretation of the period, most will find it fundamentally dissatisfying.

A dense, over-written style, combined with an approach and methodology all of which strongly echo those of Ken Alder (see *The Measure of All Things: The Seven-Year Odyssey and Hidden Error that Transformed the World*; New York: Free Press, 2003) implies that Riskin's book was intended to be a major statement about the pervasive role of natural science in the French Enlightenment, but Riskin's material and approach work best as a series of little arguments. Riskin's definition of sentimental empiricism is somewhat murky; her own account of it is more functional or contextual than descriptive (pp. 4, 5, 15, 16). The lengths that Riskin goes to in order to portray Enlightenment empiricism as basically sentimental in inspiration with "a corresponding intimacy between the natural sciences and the emerging moral sciences" (p. 153) often gets in the way of her evidence and more limited but more convincing arguments. Having to constantly hammer at the point that "[s]entimentalism characterized the methods of what are now considered the hardest sciences, physics and chemistry" (p. 6) is a heavy burden for any book to bear (see p. 73 and p. 121 for examples), especially one that is

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essentially a series of case-studies unified solely, and often loosely, by her focus on the omnipresence of sentimental empiricism in the world of science.

The book is divided into eight chapters. Chapter 1, ''Sensibility and Enlightenment Science'', sets out the contours of the concept of sensibility and how it evolved over time before concluding with the plan of the work and some of the historiographical implications of her central arguments. The first substantive chapter, ''The Blind and the Mathematically Inclined'', focuses on the experimental study of blindness in the aftermath of John Locke's philosophical assertions. Riskin wishes ''to recover, within the various meanings of ''sensibility,'' their unifying logic, in order to show that this logic functioned as a medium of interaction among science, culture, and politics'' (p. 21). Chapter 3, 'Poor Richard's Leyden Jar', establishes a dichotomy between 'rational, mechanist systems of physics' that attempted to 'internalize the natural world by reducing it to a mechanical model' and the sentimental empiricists (such as Denis Diderot) who saw the natural world as 'imbued with agency' modeled on ''the social world'' (pp. 69, 70). As the title implies, this chapter focuses on Benjamin Franklin whose studies of electricity were the sentimental empiricists' ''favorite example of the good sort of physics'' (p. 71). In the chapter, ''From Electricity to Economy'', Riskin focuses on the naturalism of the Physiocrats rather than on their attitudes about material or markets. She asserts that '[t]he Physiocrats understood their injunction that nature be allowed to determine taxation as the crux of an empiricist economic philosophy', a shift of focus away from questions of productivity or technical knowledge that thoroughly misrepresents Physiocratic views on agriculture, industry and commerce (pp. 109–111). 'The Lawyer and the Lightning Rod' explores ''the proper relations of scientific to legal authority'' (p. 141) in which Maximilian Robespierre made a name for himself by defending the installation of a lightning rod in his native Arras. Riskin depicts empiricism in law and its links to natural science, but here the sentimental aspect seems to have dropped out (p. 142). Her attempt to return to her central argument (p. 148) is striking in its ignorance of the intricacies of the debate about the nature of the ''general will'' in the French Revolution and the institutional and social impact of Revolutionary politics on the practice of science. ''The Mesmerism Investigation and the Crisis of Sensibilist Science'' portrays the royal investigation of mesmerism in 1784 as ''an application of the credo of sentimental empiricism outside the

ranks of established natural science'' (pp. 189, 190). Imagination, and its role in science and society, takes centre stage in this chapter. ''Languages of Science and Revolution'' explores how language could ''dangerously loosen the senses' grip on the mind'' (p. 227). Riskin is particularly interested in the innovative chemical nomenclature that emerged on eve of the Revolution. She ends with a few examples of views on civic education. The conclusion traces the legacy of the sentimental empiricists.

There are some worthwhile aspects to Riskin's book. She is to be lauded for using sensibility as an analytical framework designed to complicate our understandings of the workings of eighteenth-century French culture. Her case studies make the importance of sensibility manifest in a number of unexpected contexts and illustrate multiple means of understanding a number of interesting cultural and political issues having to do with scientific matters. Intellectual history in Riskin's hands also successfully contributes to the externalist/internalist debate which is so fundamental to the study of science in the age of Enlightenment, by showing that ''ideas are not separable from scientific practice or social organization and cannot be treated separately'' (p. 16). It is, however, the repeated attempts to magnify the significance of these findings for scientific practice that ultimately pall on the reader.

Riskin's overemphasis on sensibility often has little to do with standard scientific concerns. Her depiction of sensibility as an important scientific 'style' clearly demonstrates to the reader the profoundly negative impact that sentimental empiricists had on scientific practice and the pace of discovery that augurs poorly for the 'heroic' role she has cast for them. Sentimental empiricism undermined the experimental approach that has become known as the scientific method. By facilitating, justifying and excusing non-productive forms of scientific practice, even in Riskin's account, sentimental empiricism appears to have slowed down the pace of innovation and distorted scientific knowledge (pp. 86, 87, 91, 148, 209, 222, 223, 243, 246, 264, 278). Although she claims that she is interested in the social, political and cultural linkages of the ideas – scientific and otherwise – that she explores, Riskin's project must be understood as a politely-worded, but basic rejection of the documentary and institutional mastery inherent in Charles Gillispie's account of the period (p. 270; see Charles C. Gillispie, *Science and Polity in France and the End of the Old Regime*. Princeton, NJ: Princeton University Press, 1980). Riskin revisits many of the themes

inherent in *Science and Polity* and comes up with a very different and troubled account of the relationship between scientists and the state, as well as the role of public opinion in scientific matters. She makes good use of the techniques made prominent by the linguistic turn in the historiography of the last three decades, but she lets them run amok (pp. 229, 261) in her quest to make a 'big' statement about the appropriate methodology for the history of science in the new millennium. The informed reader will frequently wonder if her approach is best suited to providing the interpretation she wishes to advance. A number of glaring mistakes about the names, attributes or powers of institutions do not inspire confidence in her reinterpretation of the questions explored so fruitfully by Gillispie. (pp. 149, 158, 187, 191)

In its theoretical foundations and interest in perceptions of eighteenth-century issues grounded thoroughly in present concerns, some will find Riskin's approach to the history of science to be a model for the twenty-first century. Her book demonstrates yet again that culture must be factored into considerations of the development of the natural sciences in the eighteenth century. Riskin's reliance on an idealistic and linguistic conception of culture to explore questions of scientific thought and practice, however, fails to convince readers familiar with the archival evidence that a Gillispie-like approach to questions of science is outmoded. As a general statement about science during the French Enlightenment, this book is an example of the linguistic turn spun rather far out of control.

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THE PUZZLE OF BIOLOGICAL DESIGN

Michael Ruse, *Darwin and Design: Does Evolution Have a Purpose?* Cambridge, MA: Harvard University Press, 2003. Pp. 371. US$29.95 HB.

By Paul Sheldon Davies

Michael Ruse, historian and philosopher of science, attempts in this book to resolve a puzzle hovering above the contemporary intellectual landscape. Educated people tend to believe that the theory of evolution by natural selection as formulated by Charles Darwin, and as more fully formulated half a century ago during the modern synthesis, defeats the argument from design. The fact of evolution and the mechanisms of selection, we tend to think, explain the apparent design of living organisms better than any form of theology. At the same time, however, educated people, including some of our best biologists, talk without blushing about the functions or purposes of components of living organisms. Appeals to biological design, it appears, are alive and well in biology departments everywhere. The very thing Darwin defeated – biological design – appears to be flourishing among the very theorists who ought to know better.

So, one is tempted to ask, which is it – Darwin, *or* design? Ruse's answer to this question is nicely reflected in his title. As he sees it, Darwin did indeed kill design. Evolutionary theory explains the diversity and adaptedness of living organisms far better than theism or deism; attributions of literal design are unequivocally false. Ruse further claims, however, that when contemporary biologists attribute functions or purposes to the components of living organisms, their attributions are (1) metaphorical, not literal, and (2) necessary for the science of biology. The correct view, then, is conciliatory: both Darwin *and* design are rightly here to stay.

Consider Ruse's reasons for (1). Metaphors are concepts borrowed form one domain of discourse and applied to another.

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When extending a concept in this way, we usually know that our extended application is literally false, so truth is not the point. We extend our concepts that we may see things anew, ask questions we would otherwise miss, and so on. And sometimes we extend our concepts for more pressing reasons; sometimes the refusal to think metaphorically can cripple an entire inquiry. Precisely this, according to Ruse, is how it is with talk of biological functions. It is, he claims, fruitful to conceptualise the components of living systems in the same way we conceptualise the artifacts of our own making. But it is more than fruitful: it is practically necessary. The claim is that, if we fail to extend our artifact-based concepts to the components of living organisms, the biological sciences will grind to a halt.

The relevant artifact-based concepts refer to *values* that artifacts possess. The components of living things, according to Ruse, when viewed through strictly Darwinian lenses, are seen entirely in terms of causal mechanisms. Values do not enter the picture. By contrast, we view artifacts not merely in terms of mechanisms but also in terms of the desires that cause us to employ them. Ruse's meat thermometer, he tells us, contributes to the preparation of delicious yet bacteria-free meat, something he and his family desire. Artifacts acquire their values by way of such desires. Ruse's reasoning for (1), then, is that we extend the concept of values from the components of human-made things to the components of living things, not for the truth of the extended applications – they are not true and we know it – but for the weighty reason that, were we to fail, the science of biology would go extinct.

Why think that, without metaphorical design, biology would go the way of the trilobite or the stegosaurus? This brings us to property (2), the practical necessity of metaphorical design. Ruse's reasoning comprises three rough steps. First, evolution by natural selection produces adaptations, some of which are so intricately organised as to appear designed; they appear as if only an intelligent designer with considerable powers of forethought could have thought them up. Evolution produces, in Ruse's words, *organised complexity*. Second, when we conceptualise an organised, complex component in the same way we conceptualise an artifact – when we apply the relevant value concepts – we are caused to ask questions about purposes or functions we otherwise would fail to consider:

> Precisely because one thinks (metaphorically) of organisms as if they were designed, one has many, many questions thrown up to be answered. Why are there plates on the back of stegosaurus? Why is the trilobite lens such a strange shape? Why does one butterfly mimic another?. . .Without the metaphor, the science would grind to a halt, if indeed it even got started. The plates are there; the lens is there; the mimics are there … Brute statements, which probably we would not be led to make in the first place because there would be no reason even to notice. Who would care about the trilobite lens? Why bother to describe it rather than something else? (p. 285)

Third, the failure to ask such questions would be theoretical suicide. Biologists would fail to notice, let alone theorise about, the most striking causal effects – the astonishing extent and degree of mimicry among butterflies, for example. Ruse cites a handful of studies in which biologists, sometimes by their own avowals, would not have arrived at the theories they offer without first having raised questions about functions.

On Ruse's view, then, we should accept evolutionary theory and all its consequences, including the death of literal design. At the same time, however, we should not permit ourselves to see the world in the full light of this fact. We should allow ourselves to 'see' the biological world through the same eyes as our theological ancestors. Why? Because seeing the world in the light of the facts would undermine the science of biology. On pain of annihilating most of biology, and evolutionary theory in particular, we must refrain from fully embracing the implications of evolutionary theory.

This strains credulity in two ways. First, is it really possible to 'see' the components of living things in terms of design even while *knowing* that no such design exists? Is it possible for me to 'see' you as someone who loves me even while *knowing* that you hate me and long for my destruction? Metaphors may play an important role in scientific inquiry, as Ruse says more than once, but are they to be used even when they conflict with or conceal what we already know? Second, as Ruse well knows, Dobzhansky famously said that nothing in biology makes sense except in the light of evolutionary theory. Ruse's solution to the puzzle of design, however, seems to suggest otherwise. He recommends that we retain the ability to conceptualize living things in terms of design – in terms of something which, according to evolutionary theory, does not exist – on pain of destroying virtually all of biology. It seems that, for Ruse, nothing in biology makes sense except in the light of the denial, or

at least the refusal to fully embrace, one rather powerful consequence of evolutionary theory.

Perhaps the gravest doubt concerns the studies Ruse cites. He offers them as evidence for thinking that, without the attribution of metaphorical design, the science would cease. The case studies do indeed involve the attribution of functions, but that by itself is no reason to agree with Ruse. There are, after all, alternative theories of functions, some of which eschew any implication of design, literal as well as metaphorical. Ruse is aware of these theories and, while he mentions one, he does not, in my judgment, give it its due. On my formulation, the *theory of systemic functions* applies to any type of system and any systemic capacity, including populations of organisms and the capacity of populations to change or to remain in equilibrium. The theory, formulated in this way, can account for the organised complexity of biological adaptations – a point that Ruse mistakenly denies – in which case it is false that metaphorical design is required to make sense of organised and complex adaptations. More can be said – more *has* been said – in defence of this alternative (see my *Norms of Nature: Naturalism and the Nature of Functions*. Cambridge, MA: MIT Press, 2001, reviewed in *Metascience* 12 pp. 312–321). At any rate, if I am right that the theory of systemic functions handles the studies Ruse cites, then we are in fact faced with the choice he tries so hard to avoid: Which is it – Darwin, *or* design?

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KUHN AND HAGIOGRAPHY

Wes Sharrock and Rupert Read, *Kuhn: Philosopher of Scientific Revolution*. Cambridge: Polity, 2002. Pp. 248. £50 HB.

By Joan Leach

If many books just at the moment have a post-millennial feel about them, this provides another for the stockpile. We are in a time where most major presses cannot publish summative works of the great thinkers and the great ideas of the past century, nay millennium, fast enough. Thomas Kuhn, here in a hagiographic series entitled 'Key Contemporary Thinkers' published by Polity, gets his turn. It is a truism to say that Kuhn has been influential. Measuring that influence in quality is a bit trickier and Sharrock and Read weigh in to help us assess the kind of significance Kuhn has had and is likely to continue to have.

The general scheme of the book takes an immense literature and attempts, somewhat successfully, to make it manageable. In the first third of the book, the authors set out to give an exposition of Kuhn's philosophical contributions. In the remainder, we get more time with his 'seminal' contributions treated under three headings: normal science and two versions of incommensurability. The exposition is built around the central claim that to understand Kuhn, one must read him as a philosopher and not an historian, or as the authors have it, ''Hardly anyone, even authors giving lengthy accounts of Kuhn's work, read his historical studies in conjunction with *SSR* (*Structure of Scientific Revolutions*), and this is, we think, one reason why the understanding of Kuhn is characteristically so poor'' (p. 23). The authors use the historical case studies of Planck and Copernicus to shed light on claims in *SSR* about 'normal science' and 'paradigms' and to defend Kuhn from what the authors see as a rampant contemporary criticism, namely that Kuhn is not 'nitty gritty' enough in his case studies. They launch a number of interesting arguments in relation to this claim. Notably among them, the authors successfully

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push the notion that, for Kuhn, novelty is not the motivation for innovation in science and they remind us of places in Kuhn's case studies where he makes this amply clear through fine-grained analysis. The exposition ends, however, with a curious argument defending Kuhn from the accusation that he is enamoured with characters like Copernicus and Planck because, at heart, he is a Whig historian. The authors defend Kuhn by pointing out that Copernicus and Planck were not really 'revolutionary figures' because they were motivated by a somewhat "geekish nature" (p. 95). The mind boggles at defending 'great men' in a series on 'Key Contemporary Thinkers' by pointing out that they were actually geeks.

Of the 'critical issues' that the authors tackle, they are especially compelling on the role of 'paradigm' in Kuhn's system and the conceptual mess that this term has caused since. The authors are pointed: "let us be quite clear about this: the wish of Kuhn's cruder fans or appliers to ape natural science by means of 'getting' a paradigm, finding their own Newton, is deeply ironic" (p. 137). It is this strange desire, manifest mostly in the social sciences, that demonstrates a current trend to do lip-service to a kind of relativism but really holding out for a paradigm, that the authors see as damaging to a fuller interpretation of Kuhn.

The remaining section tackles incommensurability as it is usually discussed by friends and foes of Kuhn. First, the authors rehash critiques of incommensurability as a form of conceptual relativism (Davidson and the like). Second, they examine incommensurability in relation to Kuhn's idea of 'world changes'. On the one hand, they see Kuhn using 'different worlds' as an idiomatic expression that is essentially harmless. On the other, the authors give a reading of Kuhn's Kantianism that suggests he did have a philosophical theory about the difference between perception and observation. This discussion is useful, especially in the light of the authors' contention that Kuhn himself was troubled by this and felt he never got it clear. This is a productive area to probe in thinking about Kuhn and his philosophical cohort and the authors suggestively leave it that it may be this issue that characterises differences in approach in contemporary philosophy of science.

'Open questions' is either one of the strengths or serious flaws of the book. If the audience envisioned here is those of us interested in Kuhn as students of history and philosophy of science, the defence of Kuhn and seeing the trees in the forest works. The only problem is that the authors view Kuhn in such a way that the forest never

appears. So, during the exposition, we are treated to Hacking positioning Popper, which the authors use to see Kuhn in contrast to Carnap. Students might need a chart. But leaving 'open questions' is not a good enough end for those of us engaged in the arguments presented in these pages. So, while there is a good gloss of a sweep of views and some suggestive arguments, the reader is left wishing the authors had done one or the other; given the exposition or gone full-bore on their interpretation of Kuhn's philosophical contributions. The authors perhaps are stuck by the hagiographic genre into a summary here and a rousing defence there and a smidgin of criticism in between. The 'key contemporary thinkers' genre falls somewhere between textbook and academic monograph, but there are pearls here. . .and geeks.

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GENETICS AND POLITICS IN CHINA

Laurence Schneider, *Biology and Revolution in Twentieth-Century China*. Lanham: Rowman & Littlefield, 2003. Pp. 320. US$75.00 HB.

By Fa-ti Fan

In October 1948, Tan Jiazhen, a geneticist visiting the United States from China, met with an administrator at the Rockefeller Foundation in New York. Tan had just attended an international congress of genetics in Sweden, where he first heard about the storm of Lysenkoism in the Soviet Union. Knowing that China was soon to be taken over by the Communists, Tan worried about his safety if he went back to China. With a Ph.D. from Caltech under Theodosius Dobzhansky, he would hardly fair well in the eyes of Lysenkoites. His American sponsors, however, urged him to return, maintaining that Lysenkoism would not take hold of China. They couldn't have been more wrong (pp. 103, 104).

In 1952, Lysenkoism (called Michurinism in China, as in the Soviet Union) became an official doctrine of the Chinese Communist Party. For the next four years, the 'Morgan–Mendelism' was denounced as idealist, bourgeois, elitist, and simply wrong. Geneticists suffered public humiliation and political retaliation. Many careers were ruined. Not until late 1956 did the grip of Lysenkoism loosen, owing mainly to the decline of Lysenkoism in the Soviet Union, the increasing political tension between China and the Soviet Union, and a change of political policy by the Chinese Communist Party. Gradually, Chinese geneticists resumed their research on a modest scale – only to suffer another, even more devastating, blow during the Cultural Revolution (1966–1976), which wiped out the whole of academe and higher education. Intellectuals, including scientists, were sent down by the millions to the country to toil in the fields and learn from the peasants.

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Genetics had started out in China on a promising note. In the 1920s–1940s, scores of bright Chinese students studied genetics at the best American and European universities through the fellowship programs of the Rockefeller Foundation and other scholarly agencies. They received cutting-edge training in the laboratories of T. H. Morgan, Dobzhansky, and the like. In addition, many went to Cornell University, which excelled in plant genetics, to study agriculture and plant breeding. After returning to China, these scientists continued genetic research and frequently collaborated with their colleagues in the United States. They published research results in international science journals as well as in the recently founded Chinese journals in biology. Despite the difficult material conditions and the endless wars, Chinese geneticists remained productive throughout the 1930s and 1940s. It is a historical tragedy that they would not enjoy the same degree of research autonomy and international contact until the post-Mao era, a full thirty years later.

Schneider's remarkable book chronicles the development of genetics in China through the twentieth century. Schneider, an intellectual historian of China, has worked on the project for twenty years. He has done extensive research into many Chinese and American archives, and has interviewed two dozen Chinese scientists whose careers were fatefully linked to the political history of genetics in China. Much of the interview material, collected in the 1980s, will always be uniquely valuable, as most of the first generation of Chinese biologists have died. This book is important for anther reason: it is the first comprehensive survey in English of any aspect of biology in modern China. It sets a benchmark for any future attempts in this unjustly neglected area of research.

The book is organised into three parts. The first part narrates the institutionalisation of genetics in Republican China, roughly from the 1920s to the 1940s. It emphasises the American connections of Chinese science during this period. The second part, which is the centrepiece of the book, details the fate of genetics and the scientific community within the political context of Mao's China, with a focus on the rise and decline of Lysenkoism. The last part reports the rapid expansion of genetics and biotechnology in post-Mao China. It highlights the aggressive state policy for developing biotechnology as well as the ambition of Chinese scientists in 'getting ahead' in the global biotechnology market.

The main themes that run through the book surround the issues of "control" – specifically "the control of nature by science, the control

of science by state and nation, and the control of scientists by state and party'' (p. 3). The first theme concerns the attempts of Chinese scientists and Government to employ science (e.g., agricultural genetics, Lysenkoist science) in the pursuit of national production and profit. The second theme characterises both the concern of China over its dependence on other nations for modern science and the nation's desire to achieve independence in this area. The final, and best-developed, theme describes the efforts by the Chinese State to control scientists and their research; the quintessential example is, of course, Lysenkoism.

This simple framework, though somewhat schematic, serves Schneider's overall purpose well. His primary interest is in the relationship of the scientific community and the State, and he relies mostly on an 'externalist' approach that deals mainly with macro social and political events. Nevertheless, Schneider does not shun the technical literature produced by the Chinese scientists. When necessary, he summarises the goals and contents of their research projects. Unfortunately, restricted by his approach, he does not go further and explore the problems of knowledge translation, laboratory and knowledge production, and other subjects that have been the foci of much scholarly interest. And the brief descriptions of scientific research do not add up to a coherent interpretation of science as cultural practice.

Given the title of the book, the reader may be forgiven for expecting to find more on other research areas in biology than genetics. In fact, taxonomy, biogeography, and other subdisciplines in the tradition of natural history formed the staples of biological research in China through most of the twentieth century. It would be illuminating to compare the development of genetics and those of other fields in biology. After all, other biologists were not exempt from national politics. From this standpoint, Schneider's circumscribed narrative runs the risk of looking through tunnel vision. These caveats aside, the book is a major contribution to both the history of Chinese science and the comparative history of modern biology.

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PRACTICAL MEN AND PAPYROPHOBIA: REVISIONIST STUDIES OF THE EARLY DEVELOPMENT OF BRITISH GEOLOGY

Hugh Torrens, *The Practice of British Geology, 1750–1850*. Aldershot (Hampshire) and Burlington (Vermont): Ashgate, 2002. Variorum Collected Studies Series CS736. Pp. xiv + 356. £59.50, US$111.95 HB.

By Kenneth L. Taylor

Introducing this collection of essays, Hugh Torrens makes the point that although his training was in geology, not history, the two disciplines share similar problems and require comparable skills. His 1960s doctoral research in geology prepared him to appreciate micro-historical investigation: to distinguish faunas within the particular zone of Jurassic rocks he examined for his thesis, it was essential to collect fossils "to the nearest inch". The habits he developed in becoming a "myopic stratigrapher" were applied in turn to the historical work he began to undertake in the 1970s; so he became, he quips, "a mole-like historian" (p. ix).

Torrens's self-description, however tongue-in-cheek, is not without warrant. Much of his historical writing does indeed show close and detailed examination of that which is specifically localised, as one sees in the thirteen selections reproduced in this volume. But it would be a mistake to suppose this means that the problems and themes of Torrens's historical research are confined to small and local matters. They are not – nor would I presume this to be any more true of his 'myopic' stratigraphic work. Almost every one of these papers serves to advance and amplify an underlying thesis that should be of interest to historians of geology, and by extension to historians of science generally. Torrens maintains that geology, in its origins and early development, owes much more than is usually acknowledged to the motivations and labour

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of practical people. His examination of early geology challenges the traditional emphasis placed on the rationalising contributions of 'great men' such as Hutton, Werner, and Lyell. In championing the historical roles of prospectors, surveyors, miners, engineers, and mineral dealers, he raises important questions about the proper balance between theory and practice in our historical perspective.

Most Anglophone historical accounts of geology celebrate the achievement of the surveyor William Smith (1769–1839), often touted as the 'father' of English stratigraphy or even of English geology as a whole. But Smith has sometimes been presented as if he were the single exceptional instance in the category Torrens has been working to expand. Most of Torrens's articles enlarge on other examples of such practical men of geology (all discussed in this volume are male), 'forgotten' or to some substantial degree neglected in historical literature. A good representative is John Farey (1766–1826), although he is certainly far from the least known, and was evidently a figure of unusual capability. In addition to being the immediate subject of one essay (the introductory piece co-authored by Torrens with Trevor Ford, for the reprint of *General View of the Agriculture and Minerals of Derbyshire*, vol. 1, 1811), Farey plays large roles in a few others and makes appearances altogether in no fewer than ten of this volume's selections. Among Farey's many interests and employments, the ones that matter most here were his work as land steward for the fifth Duke of Bedford and, following his dismissal from that post upon the Duke's death in 1802, his career as a land and mineral surveyor. Farey came to know William Smith no later than 1801, and soon became one of the staunchest advocates of the importance of Smith's stratigraphic methods and discoveries. He also worked under commission for two well-placed patrons, Joseph Banks (whose Derbyshire estates encompassed noteworthy mineral assets), and John Sinclair of the Board of Agriculture. The consequences for geology include Farey's production, between 1807 and 1812, of a remarkable trio of stratigraphic sections across different parts of England; his three-volume mineralogical and agricultural report on Derbyshire for the Board of Agriculture; and, along the way, a number of unsigned articles on geological subjects for Abraham Rees's *Cyclopaedia*, from 1806 to 1811.

Details of Farey's sometimes unhappy relations with the gentlemen-geologists of the Geological Society in London (of which Farey was never a member) as they emerge here in Torrens's accounts – particularly in the nuanced essay on Banks and the earth sciences – reflect the metropolitan scientists' prejudices against Farey's tiresome interest in

minute detail and his corresponding detachment from Wernerian geognostic theory. Farey was infuriated at being thwarted in his efforts to have his Derbyshire mineral survey published fully in the Geological Society's new *Transactions*; Society officers proposed to reduce Farey's paper to one-quarter its original length. In due course Farey took a measure of revenge on his tormentors, inveighing in public against "the very improper and interested views of *a Party*, alike hostile to the cause of real Geological Science, as to those Persons engaged in Practical and un-theoretical investigations of the facts of the British Stratification, *by refusing to publish*, and yet claiming the right to detain my Manuscripts!" (*General View of the Agriculture and Minerals of Derbyshire*, vol. 3, 1817, p. viii). Torrens duly notes that Farey's difficulties were compounded by his misfortune, as someone closely identified with Banks, in being caught in political crossfire between the Geological Society and the Royal Society, involving George Bellas Greenough and Banks in particular. His main point remains that historians fail adequately to take account of the obstacles that faced mineral surveyors in their attempts to publish, and that the "debt of later members of the Geological Society to the mineral surveyors has simply not been acknowledged" (p. V, 68).

Two other instructive cases are those of the Shropshire naturalist and chemical lecturer Arthur Aikin (1773–1854), and the Irish-born surveyor–engineer James Ryan (ca 1770–1847). Torrens's discussion of Aikin is mainly oriented around the *Proposals for a Mineralogical Survey of the County of Salop, and of some Adjacent Districts*, an undated pamphlet accompanied by an engraved mineralogical map and section. Through some typically thorough sleuthing, Torrens establishes the publication date of this prospectus at 1810, and shows how Aikin, after working at his survey for two decades, had finally to abandon the project in 1816 for lack of subscribers. Following an appreciative examination of Aikin's geological interpretation of the part of Shropshire he succeeded in surveying, Torrens notes how inadequately Aikin's pioneering work was acknowledged by its most famous beneficiary, Roderick Murchison, in *The Silurian System* (1839). (It is heartening to know that the Geological Society archive preserves Adam Sedgwick's privately but candidly expressed indignation at Murchison's miserly under-allocation of credit to others, on his 1836 reading of a draft introduction to *Silurian System*.) In Torrens's account, Aikin's unfulfilled survey project is an object lesson about conditions tending to leave such scientific workers in historical obscurity.

Where Aikin's comparative historical invisibility is found to result from his inability to command financial support for his work, and from the lingering force of historical distortions promoted by triumphal publications like *The Silurian System*, Ryan's is judged by Torrens to derive in large part from a misguidedly cheap view of cost-effectiveness on the part of contemporary mining owners and management, as well as a dose of anti-Irish prejudice. While employed with Irish and Welsh mining concerns, Ryan in 1804 invented (and in 1805 patented) an unprecedentedly effective boring device, described as acting like a surgical trepan, capable of recovering rock cores in reliably preserved sequence and orientation. Despite his success in persuading scientific men (including Farey, Banks, and Greenough, as well as James Sowerby) of his invention's virtues, not only for mineral exploration but also for mine ventilation, the device was only sporadically adopted in his time. The invention was expensive to use. It also came upon the scene at a time when mine safety was the subject of a politically-charged debate pitting Humphry Davy's safety lamp against improved ventilation measures, a controversy in which factional alignments set scientists against practical men. Torrens sees Ryan as "the victim of polarised attitudes and geographies" (p. viii, 79).

A term applied by Torrens to Ryan's activities – and to those of three or four other figures treated in these essays, and less explicitly to several more as well – is *papyrophobic*. A state of papyrophobia, or the condition of leaving disproportionately little written residue for the use of historians, is the chronic condition of most of Torrens's subjects. To write little that ends up being preserved, in some cases possibly nothing at all, is the lot of such practical men. Torrens sees historians of science as tending toward papyro*philia*, and not coincidentally as sometimes prejudiced in favour of high culture and against serious consideration of historical actors who by occupation or underprivileged economic condition wrote comparatively little, and published even less. In my opinion, Torrens goes a little too far when he characterises historians as "only used to assessing publications" (p. vi, 20), and I think it is not difficult to find examples of historians of geology who consult manuscript resources to historical advantage. Still, any reader can judge by the contents of this volume that Torrens sets a high standard for the relentless pursuit and historical use of unpublished resources. Among these essays are accounts of some of his remarkable manuscript finds in American as well as British libraries and archives. It should also be said that he frequently turns up valuable information by digging indefatigably

among printed materials that are obscure by virtue of being local or ephemeral. Also important is Torrens's recommendation to weigh non-written historical resources such as specimens preserved in fossil and mineral collections, as well as instruments.

Much of what Hugh Torrens includes in this volume appears to be motivated by a desire to redress historical imbalances arising out of his subjects' papyrophobic circumstances. I venture to add that one senses, in at least some of these essays, his interest in historical adjudication to rectify injustice, conceived not just intellectually but also economically and socially. When he refers to "that most destructive of English barriers, Class" (p. v, 68 – the context has to do with Farey's relations with Banks and the gentlemen of the Geological Society), we take Torrens to be commenting on a past continuous with the present. One does not need to know Hugh personally (as I do) to discover that his sympathies lie with those who work for a wage, who find themselves obliged to go abroad to employ their technical trade, who pay a price for flouting prevailing conventions, who strive for enlightened reform against the resistance of entrenched authority, or who are victimised by the exertion of privilege of wealth or position. To one degree or another, each of these sympathies is expressed historically in the essays collected here.

I do not mention this element of personal engagement to find fault with it; actually, I think it energises and in the end adds to the value of Torrens's historical work. With this in mind, it occurs to me to call attention to one of Torrens's essays that perhaps ought to have been included in this collection (I wonder why it was not) since I think it splendidly combines practically all of the themes I have discussed in this review. I mean his 1995 Presidential Address for the British Society for the History of Science, "Mary Anning (1799–1847) of Lyme: 'the greatest fossilist the world ever knew'" (*BJHS*, 1995, 28: 257–284).

Finally, I should take note of what I judge to be Torrens's view that a full and proper historical reckoning of geology's place in the British Industrial Revolution has yet to be accomplished. The work represented in this volume makes a strong case for this question's further examination.

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A HISTORY OF IGNEOUS PETROLOGY – AT LAST!

Davis A. Young, *Mind over Magma: The Story of Igneous Petrology*. Princeton: Princeton University Press, 2003. Pp. xxii + 686. US$69.95 HB.

By David Oldroyd

Professor Davis Young is the first person to have written a comprehensive history of igneous petrology, from antiquity to the end of the twentieth century. It is, in a way, analogous to von Zittel's classic *History of Geology and Palaeontology in the Nineteenth Century* (English edn, 1901).That is, it is accurate, broadly comprehensive, and written from an internalist perspective.

It may seem surprising that this major topic has remained neglected for so long. But the field is so vast and complex that it cannot be undertaken by anyone who has not spent a lifetime in the field. There have been scholars such as the Australian petrologist Thomas Vallance or the American experimental petrologist Hatten Yoder who could have tackled the task. But Vallance's talents were diverted into the history of Australian geology and he died relatively young. Yoder, too, has now passed away, and though he knew all about Young's topic he did not get further than writing his brief 'Timetable of Petrology' (1993) and a relatively brief account of the early history of work at the Geophysical Laboratory, Washington, where so much experimental work was undertaken, particularly by Norman Bowen, for whom Young has previously written a biography (1998).

Young's task is greatly magnified by the problem of languages. Much petrological work was written in German, French, and Russian, and to some extent in Japanese. A historian of the field must grapple with at least French and German (and Russian would be an advantage). In fact, there's an old Russian text on the history of petrology by F.Y. Loewinson-Lessing (English translation, 1954), but it was written from a scientific perspective that was even then rather dated; so the book did not serve as a successful precursor to putative

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histories of petrology by anglophone historians. Young has battled with the French and German, but that still leaves much relevant literature only accessible through secondary sources. However, he has surveyed a vast amount of secondary literature in the form of small-scale studies (probably *all* the relevant material available in English) and provides an account that successfully covers the broad sweep of the enormous field.

Young starts at the 'beginning': with ideas about volcanoes in the ancient world. From there he proceeds through the historiographically well charted territory of the Vulcanist/Neptunist controversy: Hutton/Werner and such. Thereafter, however, the going gets increasingly difficult for the scientists concerned, the historian, and the readers of Young's *magnum opus*. The scientific problems were essentially twofold. There's a vast number of different igneous rock-types, differing in chemical composition and texture. What is the *cause* of this diversity, and *how* are they to be classified (the latter problem forming the basis of the science of petrography)?

Igneous *diversity* is, then, one of the main foci of Young's attention, both in *Mind over Magma*, and in his earlier publications. Petrographic classification provides problems quite different from those in biology, ethnography, linguistics, etc. For the most part, igneous rocks are not related to one another genetically or evolutionarily. Thus they are not amenable to cladistic analysis. One can, moreover, find rocks of similar appearance with differing chemical composition; or rocks that have different appearances but similar chemical compositions. There is also the problem of customary usage. Old names such as basalt, granite, gabbro, etc., may need to be retained though it may be difficult to fit them into more fine-grained taxonomic systems. For such reasons, philosophers discoursing on problems of classification (wisely) steer clear of igneous petrology. I do not recall any text that chooses this science to illustrate taxonomic conundrums. The leading text on the philosophy of geoscience (Engelhardt and Zimmerman, *Theory of Earth Science*, 1988) prudently discussed *sedimentary* petrology when considering taxonomic issues.

Matters were made worse by the fact that different authors developed quite different taxonomic systems, so that students of different teachers or schools learnt different systems. For myself, I was brought up in the 1950s on Alfred Harker's texts, blissfully unaware that there was almost as much diversity among igneous rock classifications as there is among igneous rocks themselves. I do not

know how I would have survived if I had tried to proceed to advanced work in this field. Had I tried to do so, a text such as Young's would have been a wonderful crutch. But none such existed in those days.

So taxonomy is one of the main issues in *Mind over Magma*, which explains how things were eventually sorted out (more or less) in the 1970s by the International Union of Geological Science's Sub-Commission on the Systematics of Igneous Rocks. Young discusses its work and explains how it was preceded by the efforts of Albert Streckeison, who devised his own system, and in publishing it posed 15 questions, to which he gave his own preferred answers; and invited comments. He received about 80 responses, and in this way the petrological community began to put its house in order, achieving a reasonable taxonomic consensus. It was a *social* process. Igneous rock classifications are not 'out there' waiting to be discovered: they are *constructed*.

But long before all this happened there was the old problem of the origin of granite, which entailed a lengthy dispute between 'neo-Vulcanists' and 'neo-Neptunists'. Linked to this was the nineteenth-century Continental geologists' liking for classifications based on the ages of rocks, along with texture and chemical composition: it was supposed that igneous activity generated different rock-types at different epochs. Thus (supposedly) there could be a kind of stratigraphy based on igneous rock types. This fallacy took a long time to die; and Young ably describes its long and painful demise.

Many people have supposed that geologists were thinking strangely in believing that granite (made of quartz, feldspar, and mica) could crystallise in some way from aqueous solutions, given that the rock is insoluble in water. The Huttonians had to be right! But, Young shows, things were not so simple. Quartz has a higher melting/freezing point than feldspars. So if granite were formed from a cooling melt "well-formed crystals of quartz should have imposed their faces on the form of subsequent[ly crystallising] feldspar rather than the other way round" (p. 82). This textural argument was difficult to counter, and it was advocated by a number of investigators, notably Theodor Scheerer (1847).

The old arguments were rehearsed in the twentieth century, the principal protagonists being Norman Bowen of the Geophysical Laboratory, Washington, and Herbert Read at Imperial College, London. Bowen and his co-workers had begun to determine experimentally what was going on in melts as they cooled and crystallised.

The compositions of hot mixtures at different temperatures (and later at pressures) could be ascertained by the *sudden* cooling of samples and the chemical and mineralogical analysis of the 'quenched' samples. Hence the phase-equilibria of systems could be studied empirically. This provided the road to Bowen's famous 'reaction series', according to which, on cooling, even basic ('basaltic') melts might – after the settling out of various crystal types – yield material of granitic (acidic) composition. (The initially crystallising material could react again with the remaining fluid as cooling proceeded.)

Against Bowen *et al*. were Read and his London supporters, various Scandinavian geologists, and to some extent Arthur Holmes in Durham and Edinburgh, and, more particularly, his wife Doris Reynolds. They were all impressed by what they could *see in the rocks*, especially those exposed on the glacially polished Scandinavian coasts, the appearance of which suggested a 'granitisation' process occurring by the penetration of hot, water-bearing fluids, between the layers of previously deposited rocks – the process known as 'migmatisation' or 'metasomatism'. It was, then, in the contest between the 'magmatists' and 'migmatists' that Read uttered his famous aphorism: 'the best geologist is the one who has seen the most rocks'. The controversy had become one between the empiricist field geologists and the empiricist laboratory experimentalists.

The petrological 'core-set' eventually deemed that, for granite at least, the battle was won by the magmatists in a famous memoir by Tuttle and Bowen (1958) on their study of the albite–orthoclase–quartz–water system. Given the importance of this publication, I think Young could have explained its arguments in greater detail. (But he did this in his Bowen biography.)

Bowen's ideas can, in a sense, be traced back to Darwin's idea that the crystals first forming from a cooling melt will tend to fall to the lower part of the molten magma, in a process of 'gravity settling', so that when solidification is complete the compositions of the upper and lower parts of the igneous body differ. This principle was developed in the second half of the twentieth century, especially by Laurence Wager and his co-workers at Oxford. Wager's investigations at the Skaergaard Intrusion in Greenland (which became a frigid Mecca for igneous petrologists) were developed into a general account of *layered* igneous rocks, and Young shows how studies of recent years, using fluid dynamics, have provided models for the sequential deposition of crops of crystals in a magma chamber, arising from convection currents in the cooling melt.

Here I have picked only a few of the themes in this heroic volume. I might also have discussed such topics as igneous provinces, the forms of igneous intrusions, the 'space problem' for the accommodation of intruded magmas, the relationship of plate tectonic theory to igneous petrology, the study of lunar rocks, the contributions made by international collaborations, experimental work with the polarising microscope, modern instruments for chemical analysis such as the electron micro-probe, . . . The list could be extended almost indefinitely.

I would, however, take issue with some of Young's choices of topics. Metamorphism and mineralogy *per se* are necessarily excluded from anything more than passing mention. But some account of high, or ultra-high, pressure studies would have been worthwhile. To make room, I think that moon rock studies could have been left aside, and likewise the 'tokenistic' page or two on women's contributions. Ultra-high pressure work has revealed the existence of a form of silica (coesite), which only exists at immense pressures, and thus only forms naturally at great depths. Yet it is found near the earth's surface contained *within* other crystals, which have allowed the maintenance of the high-pressure form in a low-pressure environment. Diamond is similar, though it does not quickly revert to the form of carbon that is stable at atmospheric pressure (graphite). So diamonds and coesite have presumably been brought to the surface rather rapidly. How so? And how is it that some of the carbon of diamonds appears (on isotopic evidence) to be of organic origin and to have been buried deep by subduction processes.

Such matters take us into the field of geodynamics, problems of the constitution of the earth's interior, and activities at the boundary between the earth's fluid core and its surrounding 'solid' mantle. So what about controversial 'plume theory' as the explanation of the transmission of magma from the core to the crust? Young tends to treat the formation of magma as lying outside his remit. That is, to be sure, a defensible position for a petrologist, but I'm reluctant to accept magma as a *deus ex machina*. I think these problems would have been more relevant to Young's account than moon rocks.

This said, I must reiterate my admiration for the author's accomplishment. A collection of essays on the history of igneous petrology would *inevitably* have been disjointed, incomplete, and ultimately unsatisfactory. Young's effort allows the picture to be seen as a whole. The book is essentially written for petrologists. It assumes knowledge of basic chemical, mineralogical, and petrological

information. It could not be otherwise, given the topic and the task. But the figure captions should have been more informative. Also the subject index is anorexic and does not do justice to the book.

Such comments notwithstanding, I think the book stands as a *major* contribution to geoscience historiography. We now have a map of the terrain, accurately delineated. It remains for others to add details, and fill in the contours. Larger-scale maps of parts of the whole are now invited.

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BOOK NOTICES

Paul Berg and Maxine Singer, *George Beadle: An Uncommon Farmer*. Cold Spring Harbor, NY: Cold Spring Harbor Laboratory Press. Pp. ix + 383. US$35.00 HB.

In this lovingly crafted biography of George Beadle, the Nobel-winning geneticist credited with discovering that genes make proteins, two eminent biologists retrace the internal history of genetics through the twentieth century as they tell the life of their subject. The book is meticulously researched with regard to official records concerning Beadle, his correspondence, interviews with co-workers, etc., and is sensitive to the subject himself, for whom the authors obviously feel great respect and fondness. Beadle's character is communicated very nicely, and the reconstruction of his intellectual interests and personal life over time most convincing. Despite the tendency to hagiography (particularly intellectually, as Beadle's views always appears closer to today's truth than any one of his contemporaries), it is unlikely that a better biography will ever be written. As a history of genetics, however, it adds little to the secondary literature beyond the exposition in accessible language of some of Beadle's experimental work not previously treated by historians. Unfortunately, the sincere efforts of the authors to place Beadle in intellectual context suffer from the typical flaws of scientists' histories: whiggishness in taking views too close to today's and attributing them to Beadle and his friends anachronistically, excess concern with retrospective distribution of credit for discoveries we now regard as true, and short shrift to contemporary investigations of gene action conducted by scientists as influential as Beadle at the time – e.g., Goldschmidt, Kuhn, Moewus (before he was declared a fraud) and especially Waddington. The end result gives the impression that Beadle was the first to do serious experiments investigating the physiology of gene action, effectively inventing biochemical and developmental genetics, when actually, during the 1940s, he was only the most prominent American in an already existing field. Nevertheless, it is a worthy biography and historians of twentieth-century biology concerned with Beadle certainly should read it. (NR)

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Brian Regal, *Henry Fairfield Osborn: Race and The Search for the Origins of Man*. Aldershot: Ashgate, 2002. Pp. xix + 219. £40.00 HB.

This intellectual biography of Henry Fairfield Osborn (1857–1935), Director of the American Museum of Natural History, is focused on Osborn's theory of human origins and his ideas of race. The book is, therefore, a welcome complement to Ronald Rainger's *An Agenda for Antiquity* (1991), which deals primarily with Osborn's administrative role with the American Museum of Natural History. Regal's book follows the intellectual development of Osborn closely, situating his ideas and opinions in their immediate intellectual and political contexts. The main chapters describe his religious upbringing, his early career in science, his idiosyncratic view of evolutionary theory, and his interest in the eugenics and anti-immigration movements. There is also a chapter on the Museum's Central Asiatic Expedition, whose announced goal was to find the remains of the earliest humans. Overall, the book is well-written and presents an insightful portrait of a major scientist in twentieth-century America. (FF)

Guangqiu Xu, *War Wings: The United States and Chinese Military Aviation, 1920-1949*. Westport, Conn.: Greenwood Press, 2001. Pp. 250. US$ 67.95 HB.

The development of aviation technology in a modernising nation can be a fascinating topic in the history of technology. It provides opportunities for research into the problems of technology transfer, technology and modernity, technological nationalism, and so on. This is certainly the case with Republican China, which placed military and commercial aviation high on its agenda for national defense and modernisation. Unfortunately, Xu's book offers only glimpses into these issues, as it focuses faithfully on the military and diplomatic aspects of American involvement in the establishment of Chinese military aviation. For what it sets out to do, however, the book is a solid achievement. It is the most comprehensive study in English of military aviation in Republican China.

Xu has worked in many archives in Britain, China, and the United States. Scholars interested in the subjects of aviation technology in China and the comparative history of aviation may find the book useful. (FF)