

## BOOK REVIEW

### The sapient paradox: can cognitive neuroscience solve it?

What makes the human mind unique? One answer would be our particular kind of culture, which might be called 'mindsharing' culture. Human beings are not only able to detect the existence of other minds, and to understand that those minds have beliefs, but are also able to form networks of trust built around shared intentions and beliefs. No other species does anything like this.

Much current research in neuroscience is aimed at understanding the processes that contribute to our construction of culture. Recognizing the importance of integrating this work into their research, and of drawing neuroscientists into more collaboration, the McDonald Institute for Archaeological Research at the University of Cambridge initiated a conference in September 2007, devoted to the theme 'Archaeology meets neuroscience'. A special issue of the *Philosophical Transactions of the Royal Society* is now devoted to the proceedings of that pioneering meeting. Although understandably selective, this volume contains a smorgasbord of current ideas and research from philosophy, psychology, anthropology and archaeology. The selection of papers is diverse and stimulating. This relatively new marriage of disciplines still lacks a unifying framework, but one must start somewhere, and no time like the present.

A major link between archaeology and neuroscience is provided by cognitive science, which has a foot in both camps. Some aspects of cognition, such as literacy, mathematics and music are obviously cultural in origin. Others, such as attention, perception and action stem directly from the design of the central nervous system. These two influences, brain and culture, work together in forming human cognition, and cognitive scientists find themselves in the position of having to explain many of the higher cognitive capabilities of human beings in terms of hybrid brain-culture mechanisms. Evolutionary models are one important way of ordering the evidence on hybrid mechanisms, and epigenetic factors may prove to be paramount in this process. Human infants are socially oriented learners, and cultural influence is crucial in shaping the development of higher cognitive functions in the brain. It has become clear that no theory of human cognitive evolution—including symbolic thought and language—can succeed without accounting for the role played by culture in shaping

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the adult mind and brain. And no account of human cognitive and cultural evolution can attain widespread acceptance unless it is compatible with what we know of brain function.

Colin Renfrew's keynote article in this volume focuses on what he calls the 'sapient paradox', a puzzle that has been a thorn in the side of prehistory researchers for some time. There seems to have been a long—in fact, inordinately long—delay between the emergence of anatomically modern humans and our later cultural flowering. Both genetic and archaeological evidence converge on the conclusion that the 'speciation' phase of sapient humans occurred in Africa at least 70 000–100 000 years BP, and possibly earlier, and all modern humans are descended from those original populations.

Renfrew labels a later period, extending from 10 000 years ago to the present, as the 'tectonic' phase. This has been a period of greatly accelerated change, stepping relatively quickly through several different levels of social and material culture, including the domestication of plants and animals, sedentary societies, cities and advanced metallurgy. It has culminated in many recent changes, giving us dramatic innovations, such as skyscrapers, atomic energy and the internet. The paradox is that there

was a gap of well over 50 000 years between the speciation and tectonic phases. The acceleration of recent cultural change is especially puzzling when viewed in the light of the hundreds of thousands of years it took our ancestors to master fire, stone tool making and coordinated seasonal hunting.

If human beings were biologically modern 70 000 years ago, why the long delay before this cultural potential was realized? One might be tempted to invoke climate as an excuse. The most recent Ice Age effectively prevented the development of agriculture until about 12 000 years ago. But once nature gave us a break, so to speak, we quickly domesticated plants and animals, and moved into larger, more stable communities in regions of the planet favourable to this development. Thus, one solution to Renfrew's paradox may be that there is no paradox. Another might be that there was no delay, and that he has unfairly discounted the tremendous innovations our ancestors made during the so-called delay period, which was marked by numerous innovations in toolmaking, boat building, painting, sculpture and navigation, among other things. Whilst our ancestors did not reach the stability and prosperity of later societies, their achievements were nevertheless impressive, when contrasted to the lifestyles of previous hominids. So why place the tectonic phase so late? Why not move it back to the time when the first boats were invented, or the first caves painted? At this point, the run-up to modernity begins to look more gradual, driven by an accumulation of culturally invented cognitive tools that eventually reached a critical tipping point.

Despite these valid questions, the central point of Renfrew's observation remains true. There was a dramatic acceleration in the rate of human cultural and technological change that began about 10 000 years ago, and it is important to understand how our species suddenly became so innovative. There is no evidence for a major change in the brain after speciation that might explain this. There may well have been many minor changes, but present evidence shows that the cognitive basics of human existence—imitation, language and symbolic thought—are shared by all living humans and that the basic elements of higher cognition are present in all known cultures.

Renfrew thinks that the interaction between material culture and the brain accounts for a great deal about the acceleration of the cognitive-cultural evolution of humans, a position that I strongly support. Of course, the biological potential had to be there, in the form of increased brain plasticity and a capacity for learning and communication. But humanity could not have reached its present levels of cultural change without first advancing the technology of symbolic communication. Renfrew raises many questions that should concern neuroscientists. How does culture influence brain development? What are the key neural components of the human brain-culture interface, and of our social mind-set? What detailed causal chains enable the cognitive-cultural chemistry of sapient society to work? More urgently, perhaps: how might they be optimized?

The rest of the papers in this volume address these questions in various ways. They can be clustered around two broad themes: material culture and distributed cognition; and underlying neural and cognitive adaptations.

## Material culture and distributed cognition

In his thought-provoking paper, Ed Hutchins proposes that the study of human cognition should be extended beyond the brain, into distributed networks of cultural practice, which include the cognitive use of material artefacts woven into webs of social interaction and ritual. He argues that such networks can amplify the apparent intelligence of the individual, and can make apes (and by extension, humans) seem symbolically more competent than they are. Hutchins's claim (bound to be controversial among researchers who work on primate behaviour) is that the apparent symbolic competence of enculturated apes, such as the bonobo Kanzi, might be illusory, and stems from the apes' role as components in a distributed system of cultural practice into which they have been placed by their human keepers. This is because the framework of cultural practice, which ultimately orchestrates the apes' performance, does not demand a fully symbolic response. The same might be said of many apparently symbolic operations carried out by humans, such as rote addition and subtraction.

Hutchins implicitly defines 'true' symbolic processing as an internal cognitive process of 'seeing as', which is unique to humans. According to Hutchins, Kanzi's performances are merely chains of non-symbolic responses to specific stimuli, linked by chains of cultural practice. Of course, it might be that these apes are symbolically competent, but the research paradigm used may not enable us to make an informed choice between these interpretations. How can we choose between them, if all we have to go on is the apes' behaviour? Hutchins's choice is based mostly on a 10-year old behavioural study, which compared 'enculturated' with 'non-enculturated' chimpanzees on a series of symbolic tasks. Both groups were trained to perceive symbols as representations of abstract relationships (such as larger or smaller, lighter or darker) and to generalize their knowledge to new tasks. The results showed that the apes' successful use of symbols did not depend on their having been enculturated into a human-like social environment. Apes raised in a laboratory performed the tasks equally well.

Hutchins points out, correctly, that the lab-raised animals used in the behavioural study were also reared in a human–ape artificial culture, with a special set of cultural practices. It may have been a different set of cultural practices from the ones used by the Rumbaugh's with Kanzi, but it was nevertheless qualitatively similar. Their environments had crucial cultural practices in common, which systematically led the apes to notice and respond to certain stimulus features in the testing situation. This involved, especially, conditioning their attention; they had to learn where and when to look, and what to attend to. This enabled them to use human-made symbols to communicate, without altering their basic cognitive abilities. Culture led the way. It was a specific chain of cultural practice that led their attention from one key aspect of the world to another (this seems to be how human children learn language).

Even if we concede the point, we might complain that this article still begs the question of whether apes are 'real' symbol users. Maybe symbol recognition is one thing, and symbolic

invention another, in both apes and humans. Maybe apes, and early hominids, were able to perceive truly symbolic relationships in their natural environment because they responded to various cues in a passively representational manner. Maybe all that symbolic recognition normally demands of us is that we attend selectively and knowledgeable to the specific relationships the symbol is supposed to elicit. The use of symbols in this relatively passive manner does not require the perceiver to invent a representational action, and it is well established that apes are poor at this. I have postulated that symbolic invention is something else entirely, and evolved independently, along its own trajectory, in humans (Donald, 1991).

Animals raised in conventional laboratories do not have anything like Kanzi's language capabilities, and the simple discrimination paradigms cited by Hutchins were not as complex as those used by Savage-Rumbaugh and her colleagues. Kanzi's abilities appear to be qualitatively more complex and abstract. Why should we refuse to call his behaviour truly symbolic? Hutchins is right in pointing out that a distributed cultural-cognitive system might account for a lot of competent symbol use without requiring any deep capacity for symbolic invention. However, our primate ancestors may well have been 'symbol-ready' long before we evolved the capacity to invent symbolic representations and build systems of practice based on them. They lacked the crucial ability to invent symbols themselves. Hence, they were unable to invent the mind tools needed for sharing cultural representations.

This raises the question of the neural interface with symbols and tools. In their paper, Dietrich Stout and his colleagues describe several PET studies on the neural sources of stone toolmaking skills in three expert subjects (archaeologists and anthropologists experienced in making stone tools). They compared three stone-knapping conditions: a control condition, in which subjects were not trying to manufacture a tool; a second condition, where they made a primitive Oldowan stone tool; and a third, where they made a more sophisticated Acheulian handaxe. PET results were also compared with those of a previous study on untrained novices. The results showed activation in roughly the same brain areas that are normally engaged in primate praxis, with some additional involvement of human language areas. Their data showed a significant familiarity effect (as would be expected in a learned skill), and a laterality bias that favours the hypothesis that language was somehow involved in the toolmaking expertise of these subjects (not surprising, considering that they were academics). The authors infer from this that language and toolmaking must have co-evolved, but this leap of logic escapes me. In modern subjects, we might expect an esoteric skill to be associated with language. But this does not constitute evidence for language 2.6 million years ago, when the first Oldowan tools were made, or even 1.8 million years ago, when the first Acheulian tools appeared. In short, the cerebral sources of toolmaking skill in modern humans look just like those of any other visual-manual skill. This finding is interesting, but leaves the evolutionary question wide open.

Scott Frey reviews several clinical cases of patients who lost skills following brain injury, and presents recent MRI evidence on human praxis, for which he used an imaginative rehearsal paradigm. Like Stout *et al.*, he argues for a common origin for stone toolmaking and language, and concludes that gesture and

toolmaking might have common origins. This is a sensible position (with which I agree to some extent) but, again, it is not clear how these studies add to the evolutionary argument. Unfortunately, but inevitably, given the methods used, these studies can only be carried out on modern human subjects with fully modern brains. One could not conclude from this evidence, for example, that ancient hominids must have had language. That simply does not follow. As their data suggest, the key to understanding toolmaking will be to expand our knowledge about motor learning, and especially about how it interacts with the amodal imaginative rehearsal process that enables human beings alone to refine their skills, a modelling process that I have called 'mimesis'.

At the other end of the spectrum, Scott Jordan approaches the issue of human cognition from the perspective of 'wild systems theory', which appears to be systems theory modified slightly to account for the minds of wild agents, such as human beings in their environments, to include material culture. In this sense, archaeological artefacts become components in a wider theory of cognition. This approach is in general agreement with my own emphasis on external memory, exograms and the importance of the interface between individual minds and the external memory environment. It also agrees with Hutchins's invocation of networks of cultural practice. I would like to see more detail in Jordan's proposals, on both the neural and cultural levels. He refers obliquely to efference copy at one point; this is a century-old idea that was picked up by the early cyberneticists, and more recently by motor theorists. But although efference copy may speak to the mechanism of basic voluntary movement, it explains nothing about such uniquely human social capacities as theory of mind, fantasy play or reciprocal mimetic games. What specific methods or theoretical ideas does wild systems theory add to our armamentarium for studying cognitive evolution? At this point, it is hard to judge.

Chris Gosden writes on 'social ontology', a term he proposes to capture the complex process by which human capabilities are brought about through interaction with the material world. He applies this concept to one specific item of ancient material culture: the sword in Iron Age Britain, and one particularly beautiful example, the Kirkburn sword, which dates back to 250 BC. The manufacture of this sword involved the collaboration of many highly developed crafts, including mining and metallurgy, aesthetics and, above all, swordsmanship itself which dictated the way in which it was designed to fit the needs of warriors. This example is a good illustration of the way in which a material item can be woven into a web of cultural practice, and is a nice expansion of Hutchins's theme.

Andreas Roepstorff summarizes many recent trends in the use of recent brain imaging data to argue that the distributed cognitive systems of culture may have evolved slowly through the mediation of material symbols. As external representations became more complex, internal representations in the brain became interwoven with them in new ways. The result was 'extended mind' and distributed cognitive-cultural systems. In general, this article yields some stimulating ideas, but I would have welcomed more discussion of the massive amount of brain-imaging work being done on symbol processing, especially in reading and mathematics. Investigating these areas is crucial, if we are to

understand how the brain-symbol interface works, and how material culture 'extends' the mind. In conclusion, Roepstorff's paper is a useful example of an important direction of theory and research, and harbingers an exciting future for neuroscientific research on the brain-symbol interface, and on the interconnections between the human brain and the distributed cultural-cognitive networks within which it operates.

## The underlying neurocognitive adaptations supporting human culture

Another important theme related to the human paradox is the nature of the adaptation that makes us so paradoxical: the unique social intelligence of human beings. I wish to note, and regret, the absence of certain major players in this section, most notably from the Max Planck Institute for Evolutionary Anthropology, which has produced so many important findings on human and ape social cognition. This is not to downplay the very fine selection of contributions to be found in this volume, only to regret their absence from such an important meeting.

Humans are characterized by their tendency to carry out coordinated action in groups. This requires a basket of capabilities, not simply one novel feature. Knobloch and Sebanz's paper is an ambitious attempt to synthesize several experimental approaches to this topic by combining a Leipzig-like approach to shared intentionality with Jamesian ideomotor theory (updated with recent additions from mirror neuron theory), and various hypotheses about the human capacity for having a theory of mind. They argue that coordinated action is the key to human culture, and that a theory of joint action must combine various elements to explain this capacity. This is a useful synthesis, since it addresses a variety of issues stemming from certain widely used paradigms. However, on balance, I find the authors' approach a bit disappointing, perhaps because this kind of work leads to rather spare black-box theories that do not address basic brain mechanisms, or say much about culture or human prehistory in any detail. In a volume focused on neuroscience and archaeology, more discussion of these topics would have been desirable.

Bloch adds a new distinction into the study of social cognition, one that is not commonly found among comparative studies of ape and human cultures, but which may help researchers isolate one of the special features that makes human social cognition unique. He distinguishes between 'transactional' and 'transcendental' social cognition. In humans, transactional social groups are not qualitatively different from those found in other social mammals while transcendental social groups are special to humans. It is certainly striking that enculturated apes are unable to form transcendental social groups. Bloch's use of the word 'transcendental' is idiosyncratic, and has nothing to do with Kantian metaphysics. It refers to the ability to envision imaginary societies, and to re-classify people as members of such (invisible) societies. A nation or a religious community would constitute a transcendental social group, as opposed to, say, a family. Transcendental groups align their ideas by sharing a purely imaginary worldview, which comes to define membership in the group. This is of great practical import, since it

determines the lines of effective interaction and trust within a society, including the lines of economic activity.

A transcendental social group is usually too large to comprehend in terms of direct personal experience. In contrast, in a family group, or even an extended kinship group, everyone has some experience with most people in the group. This is a very useful distinction, and raises the issue of whether cognitive neuroscientists might help to differentiate Bloch's concept of social imagination from other kinds of imagination. Further, one might ask: is transcendental social cognition mediated by the same mechanisms as Hutchins's proposal for a uniquely human imaginative capacity of 'seeing as?'

Chris Frith carries this question further. He focuses on human imagination, imitation and group learning and distinguishes between learning from instruction and learning by observation, arguing that human uniqueness may lie in our ability to engage in the former which requires metacognitive oversight. This reiterates the idea that pedagogical interactions are a unique feature of human cognition, and adds the notion that this might involve an evolutionarily novel kind of learning. He postulates a major role for mimesis based on observation in social transmission, and cites recent experimental work showing that children can learn a variety of emotional responses and behavioural strategies simply by watching other children play. This empathic, embodied and imitative social process is ubiquitous, extending even to the assimilation of very deep conditioned emotional responses. However, none of this requires metacognitive oversight, whereas pedagogy does, and this indicates that metacognition might be one of the key drivers underlying the construction of culture. This is a very helpful observation, and is compatible with available neuroscientific evidence on the expansion of the executive and supervisory regions of the cortex in hominids.

So-called 'mirror' neurons also figure large in Frith's proposal. Like many others in this volume, Frith implies that a direct link has been proven between mirror neurons and imitation. Unfortunately, it has not. In fact, monkeys and apes, who have large numbers of mirror neurons, are very poor imitators, and even poorer at understanding or generating gestures. Moreover, there is no good evidence yet that mirror neurons have anything to do with the impressive social cognitive skills of primates. A great deal of work remains to be done in this field, and some of the grand claims that have been made on behalf of this category of wildly over-promoted premotor neurons may have to be quietly forgotten, once the dust clears.

Lambros Malafouris proposes the concept of 'extended self', a variant of extended mind, and a new term: 'tectonoetic' awareness, which plays on Endel Tulving's notion of 'autonoetic' awareness, an evolutionary elaboration of his first theory of episodic memory. Tulving believes that autonoesis, sometimes called mental time-travel, is special to human beings. Malafouris's idea is that our progression to autonoetic awareness develops largely in terms of our historic interactions with things—that is, with material culture. Tectonoetic awareness is thus a product of our engagement with material culture, a scaffolding process without which a person could not achieve full autonoetic consciousness. The implication seems to be that many humans would not have full autonoetic awareness if their immersion in material culture did not

provide them with the appropriate tools. This is debatable and I think he misreads Tulving on this point. On the other hand, I agree with the main thrust of Malafouris's argument that selfhood is profoundly transformed through its immersion in material culture, and that the crucial role of artefacts should be acknowledged in cognitive science.

Fiona Coward and Clive Gamble also emphasize the notion of embodied cognition and argue that humans run 'embodied simulations of other's actions', rather than abstract representations of those actions. This claim is very close to my own argument for an archaic 'mimetic' adaptation in hominid ancestors over 2 million years ago. They also cite Gallese and the mirror neuron literature. However, although the existence of mirror neurons seems, on the face of it, to strengthen the feasibility of the notion of mimesis, I would again ask for caution, and much more evidence. There has been too much speculation, based on indirect evidence, mostly from human fMRI studies, about mirror neurons (never directly recorded in human subjects), and about their implications for the supposed localization of certain social-cognitive functions in the brain. This is sometimes backed up, again indirectly, by clinical case studies. However, even when we can demonstrate that a specific brain region is involved in a certain function—a good example might be the fusiform cortex's proven involvement in word and object recognition—this does not explain in any detail how the neurons in that area can achieve such a result. And it does not allow us to conclude that, because two paradigms might activate the same brain areas, they must employ similar cognitive mechanisms (the same area might be engaged in numerous different, but overlapping, functions).

Dwight Read and Sander van der Leeuw trace artefact technology in human evolution, and match it against a proposed timetable for the expansion of working memory. Their theory places much of the cognitive burden of human evolution on this expansion, and there is no doubt that working memory and executive function play a crucial role in human intelligence.

Memory management and the control of attention are central factors in mimesis, spoken language and tool use, and it is well established that expanded executive functions were a major factor in the evolution of our peculiar form of human intelligence. The authors' major contribution here is their attempt at a systematic breakdown of the steps involved in stone toolmaking, in terms of the cognitive load imposed by this complex task. This is exactly the kind of analysis needed to design finer experimental paradigms for neuroscientific experiments on toolmaking, which factors so strongly in our speculations about human cognitive evolution.

Together, these papers demonstrate the healthy diversity of opinion that exists in this very active and important field of investigation. They also show how far we have travelled in the past 20 years, and remind us how much further we have to travel before our neuroscientific theories of human cognitive and cultural evolution can be filled out with the kind of detailed verification that we all ultimately seek. In cross-disciplinary work, the most difficult task is often to reach agreement on basic questions of terminology and method. Researchers in this field seem to have crossed that threshold, and many of the walls between disciplines are coming down. We are now working on the creation of a common universe of theoretical discourse. There is gold to be mined here, and there will undoubtedly be more conferences, and a growth of collaborative work, on this theme.

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