

Why the behavioural sciences need the concept of the culture ready brain

Charles Whitehead

Independent researcher www.socialmirrors.org, UK

Abstract

From the conceptual gulf dividing social from biological anthropology this paper infers an ideological problem affecting science as a whole. Cultural biases have tended to inhibit or subvert appropriate theorising and research into unique aspects of the human mind, brain and behaviour. To resolve this problem I suggest that we need a systematic anthropological critique of ‘collective deceptions’ affecting western science, and greater anthropological collaboration with neuroscience and other disciplines. I discuss recent imaging studies which may contribute to a better understanding of the culture ready brain. Taken in conjunction with fossil and archaeological data, the findings seem more consistent with a ‘play and display’ hypothesis of hominid brain expansion than with current cognocentric hypotheses, suggesting new directions for research. Such research, I argue, could assist integration between behavioural disciplines.

Key words

problems in behavioural science • the human difference • culture and biology • collective deceptions • social mirror theory • social displays • communication • play • performance • cultural arts • self consciousness • mirror neurones • social brain • culture ready brain • neuroscience • neuroanthropology • anthropological neuroscience • palaeoanthropology • evolution • consciousness • Victor Turner

THE TROUBLE WITH ANTHROPOLOGY

Anthropology could be defined as the study of the human condition in the broadest possible sense. As such, and in view of the daunting consequences of human activity in terms of violence, deprivation, and destruction, one might imagine that anthropology is the most important of all the sciences and that other human disciplines – such as psychology and neuroscience – should be subdisciplines within anthropology. The trouble is, in actuality, few psychologists or neuroscientists think of themselves as engaged in ‘anthropology’, and (in my experience) few believe that anthropologists have much to say that would be interesting or relevant to their work. A recent review of social neuroscience (Adolphs, 2010) declares that the challenge of understanding the social brain requires input from ‘all relevant fields’ – then gives a list which does not include anthropology. As one distinguished neuroscientist commented (in response to my suggestion that anthropology is crucially important to consciousness science): ‘You do an empirical experiment and you get an empirical result. What can any anthropologist tell me that could change that?’ Clearly this scientist has never critically examined his own commonsensical assumptions, or considered that the hypothesis to be tested, the choice of experiment, and the interpretation of the result, are not in the least empirical.

The uninterest in anthropology extends beyond science. Governments do not routinely consult anthropologists before taking decisions with potentially momentous consequences, and until recently, there was very little interest in teaching anthropology below university level. Good citizenship, apparently, is not widely thought to require anthropological insight. And when it comes to research funding, governments generally have non-anthropological priorities.

So why isn’t anthropology more influential?

Part of the problem may lie in anthropologists themselves, many of whom would rather talk to each other than engage in cross-disciplinary dialogue. This, however, is a symptom rather than a cause. A more fundamental factor may be the poor predictive power of social and cultural anthropology. Intra-, inter-, and extra-personal influences on behaviour are incalculably complex. Even the actions of a single individual can be as difficult to forecast as the weather, and the outcomes of collective actions, such as the invasion of Afghanistan or interventions in the credit crisis, are impossible to predict. Poor predictability, however, is a problem shared by other behavioural disciplines. Further, despite the imprecision of weather forecasting, we still employ meteorologists, and anthropology has uses other than prediction. Through self- and social-understanding we could potentially change our behaviour for the better, or at least address social problems with greater cultural insight.

I suggest a deeper issue is involved. One of the first things I learned as a student at University College London is that social and biological anthropologists can barely talk to each other without lapsing into polemic. When Darryl Ford founded the UCL department he combined within it the ‘four legs’ of anthropology (biological, social, archaeological, and linguistic) in the belief that this would lead to a more integrated science. His expectation proved as erroneous as others in our field. When Leslie Aiello became head of the department, she told me that some years previously a faculty meeting had been held with the intention of resolving the differences between social and biological anthropologists. The only conclusion reached was not that the two parties favoured different ways of answering questions – they could not even agree on what a question was.

The conceptual gulf dividing social from biological anthropology indicates that something is amiss. This is more than a simple conflict between two anthropologies – it divides social anthropology from the rest of science as a whole (*cf.* Perry & Mace, 2010). Biologists have no special difficulty talking to and persuading psychologists, neuroscientists,

and others. It is the more esoteric musings of social anthropologists that rouse incomprehension, disbelief, or – more commonly – uninterest. There seems to be an impenetrable mental block separating those who have studied the ‘anthropological other’ from those who have not. It appears that the beliefs and ideology of western science do not survive unscathed following deep involvement with cultural ‘otherness’.

If so, then there is something seriously wrong with the ideology of science, and anthropology itself may not be entirely exempt from this – a loss of faith does not amount to a solution to scientific ills. Viveiros de Castro (1998) refers to science as ‘western mythology’. This is a polemical term which will not impress non-anthropologists, nor is it anthropologically correct – scientists do engage in empirical research and make progressively more reliable predictions within their fields of study, a practice opposed to traditional sources of authority such as myths. On the other hand, some of the commonsensical assumptions and beliefs of scientists might be plausibly described as ‘false consciousness’, as conceived by Marx and Engels (1846). Bourdieu (1972) pointed out the political conditions on which scientific practice depends, and the taken-for-granted way in which cultural fictions, which serve to validate privilege, are never critically examined however logically incoherent they may be. A more systematic critique of western science and scholarship is required – and is beginning to receive attention from anthropologists (e.g. Jackson, 2009), psychologists (e.g. Barušs, 2008, 2010) and neuroscientists (e.g. Turner, this volume).

One might expect that, if social anthropology has the potential to resolve deep errors at the heart of the western ‘scientific’ worldview, then social anthropologists should feel a surge of confidence and a determination to set about this all-important task. With few exceptions, however, the opposite has occurred. The bathetic ‘Finale’ to the fourth volume of *Mythologiques*, which led to the collapse of structuralism, may have been the critical factor provoking a crisis of self-doubt in (mainly European) anthropology. ‘Grand theories’ became

virtually taboo and the discipline focussed on increasingly local studies with increasingly modest aims. Some went so far as to make a virtue of despair, embracing postmodernism and abandoning the idea of anthropological theorising altogether. Perhaps more importantly, the hermeneutic tradition of intuitive interpretation (e.g. Dilthey, 1883-1911; Geertz, 1973), which has a well established place in social anthropology, is open to abuse. A distinguished anthropologist once confessed to me, with a wry smile, that the reason he entered this field is because it allows you to say pretty much what you like and get away with it. As a student I sat through many hours of seminar presentations in which social anthropologists did precisely that, and this allows biologists (quite justly) to dismiss our discipline as ‘not scientific’ (as expressly stated by Perry and Mace, 2010).

Cognitive and cultural anthropologists commonly duck the problem by defecting to the biological camp. Forgetting Durkheim’s injunction to ‘treat social facts as things’ irreducible to lower orders of explanation such as biology or psychology, and denying the ‘anti-biological’ character of cultural phenomena, they favour naturalistic explanations. Religion, for example, is explained by a genetically evolved ‘symbolic module’ (Sperber, 1994), ‘neurognostic processes’ (Laughlin *et al.*, 1992) or a hominid ‘mimetic controller’ (Winkelman, 2002). Boyer’s (1996) ‘cognitive theory of cultural representations’ claims that animism is natural because it is not natural – counter-intuitive ideas are believed because their un-believability makes them ‘salient’, and domain-specific modules are applied to the wrong domain because they have ‘inferential potential’. The inferential potential of applying a ‘theory of mind module’ to a tree or a mountain would seem to be worse than zero, and any animal doing this might be expected to face imminent extinction (Mithen, 1996). It is rather like arguing that all cameras take photographs of UFOs precisely because they are designed not to. Such genocentric and cognocentric notions are warmly applauded by evolutionary psychologists (e.g. Pinker, 1997) who are only too grateful for an excuse to discount the

disquieting anomalies of ethnographic data. Where biologists do acknowledge un-Darwinian behaviour, such as the chastity of priests, they resort to quasi-biological replicators – ‘parasitic memes’ – fashioned by random copying errors (Dawkins, 1989). Such approaches make cultural phenomena arbitrary, disembodied, and of dubious (if any) functionality.

What social anthropologists could be doing is exposing the political origins and self-contradictory character of ‘collective deceptions’ in western science (Whitehead, 2002, 2006). This is not to deny the utility of science within its zone of applicability. The power of science, however, is the result of its empirical method, and not its institutional self-legitimation – indeed there has always been a fundamental conflict between discovery and doxa (Kuhn, 1962).

The divisions within the behavioural sciences account for the failure to arrive at any consensual definition or theory of what makes human mind and behaviour unique (Henshilwood & Marean, 2003). This is curious because we appear to have an excellent implicit awareness of ‘the human difference’. My children, for example, laugh in all the right places when they watch Bugs Bunny cartoons, where much of the humour depends on animals behaving like humans and our intuitive recognition that this is absurd. The insights of cartoonists such as Chuck Jones are non-trivial, as illustrated in Figure 1. Primatologists have noted that chimpanzees, though they share many facial expressions with humans, do not have an expression for surprise. There are also no reported instances of apes checking each other’s facial expressions as if to say ‘Did you see what I saw?’ But as yet, to the best of my knowledge, no scientist has pointed out this simple example of uniquely human social mirroring. Chuck Jones, in this instance, seems to be more perceptive than the scientists.

[Figure 1 about here]

If scientists cannot define something which children understand effortlessly, then I think this is fair evidence of cultural obfuscation, And if you can't define what needs to be explained, your attempts to explain it are unlikely to be enlightening.

CULTURE AND BIOLOGY

The specific issue dividing biological from social anthropology is the question of whether enculturated human behaviour is 'biological' or 'anti-biological'. But are these mutually exclusive or even meaningful alternatives? Is there any utility in separating 'nature' from 'nurture', other than academic convenience? A genotype ('nature') specifies nothing unless there is an appropriate environment ('nurture') to read out the code and translate it into a chicken or a banana plant. Further, every major transition in biological evolution is 'anti-biological' relative to what went before. Major transitions are few and far between because they depend on cooperation, which is only beneficial in the long term (Maynard-Smith & Száthmáry, 1995). The short-term interests of selfish individuals tend to resist such changes which, when they occur, happen relatively abruptly, like shifting a log jam (Knight, 1998). All major transitions create *sui generis* emergent orders, so human culture is not unique in this respect. Other examples include the genetic code, chimerical modern cells, sexual reproduction, multi-cellular organisms, brains, and animal societies.

A common error in current Darwinian thinking is the assumption that biology is fundamentally competitive. It is true that red squirrels compete with grey squirrels, but they do not compete with moles or oak trees. An ecosystem is essentially mutualistic. It is equally true that alleles for blue or brown eyes compete with each other, but they do not compete with genes for bones or kidneys. In *The Selfish Gene*, Dawkins himself compared genes to oarsmen in a racing boat – to survive, they must pull together, or at least not rock the boat. Without cooperation there could be no competitors, and life could not even have begun.

A second common error is the assumption that ‘selfish genes’ are the prime mover in evolution. In strict Darwinism the prime mover is environmental threat. In the absence of threat, natural selection tends to *resist* change. It is un-biological to ‘explain’ behavioural change as *resulting from* genetic change or the *ex vacuo* emergence of domain-specific brain modules. Evolutionary psychologists surely know why brains evolved: as Cosmides and Tooby (1997) point out, brains are found only in animals that move. Brains are behavioural organs, and behavioural adaptation, being immediate and non-random, is vastly more efficient than genetic adaptation. So, in animals with brains, behavioural change is the usual first response to environmental threat. If the change is successful, genetic adaptation to the new behaviour will follow more gradually. Animals do not evolve carnivore teeth and then decide it might be a good idea to eat meat.

Evolutionary psychology and behavioural ecology tend to focus on ‘mental processes’ (determined by hypothetical structures not necessarily supported by brain research) and the fitness consequences of behaviour (Perry & Mace, 2010). Cultural influences are seldom addressed, and it is often assumed that culture will arrive at an optimal solution, adaptive at the ‘selfish individual’ level. Just how this is accomplished is not explicitly addressed (Barrett, Dunbar & Lycett, 2002; Boyd & Richerson, 2005; Richerson & Boyd, 2005; Cronk 1995; 1999: cited in Perry & Mace, 2010). Even dual inheritance theory (DIT) – or gene-culture co-evolution – remains essentially genocentric, with ‘culture’ thought of as a simple extension of animal cultures based on ‘social learning’ (*cf.* Boyd and Richerson, 2005; Richerson and Boyd, 2005). In DIT the evolution of culture is perceived as gradual, cumulative, and closely analogous to genetic evolution. The creative origins of cultural ideas and structures is seldom considered, along with human behavioural universals that make creativity possible. A ‘big bang’ origin of culture and language, implicit in the notion of culture as *sui generis* emergent order, is not on the agenda. Unsurprising discoveries such as

the link between dairy farming and genes for lactose tolerance are hardly likely to revolutionise anthropological science, whilst evolutionary principles such as ‘random variation’, ‘cultural drift’, and ‘transmission bias’ seem unlikely to shed any light on ubiquitous cultural phenomena such as religion or magic – often assumed to be ill-informed antecedents of modern science, or an inevitable response to self-conscious awareness of our own mortality, by those who unknowingly reinvent the obsolete theories of the British intellectualists.

A striking feature of biological and quasi-biological approaches in anthropology is a tendency to ignore proximate mechanisms (primarily the brain). Where biological anthropologists have investigated the brain, their research has been ethnographically naïve (see below). Social anthropologists are also not commonly interested in brains, but in this respect are following a reasonable precedent set by Simmel and Durkheim. There is no such precedent in biology. On the contrary, there is a well established respect for Tinbergen’s (1963) ‘four whys’. That is, to fully account for a behavioural trait, four independent explanations are required: how it evolved (phylogeny), how it develops (ontogeny), how it is adaptive (ultimate mechanism), and how it is accomplished (proximate mechanism).

In order to resolve the conceptual differences dividing biological from social anthropology I think we need to take these four questions seriously. If we accept that major transitions in evolution lead to the emergence of *sui generis* systems which are ‘anti-biological’ relative to what went before, then this affects the answers we give to all four of Tinbergen’s questions. Purely genetic explanations will no longer suffice. Emergent orders have their own internal logic and top-down causality. They create collaborative societies of individuals who must sacrifice some of their biological ‘selfishness’ to the larger interests of the collective whole. The chastity of priests may serve the land-owning interests and spiritual authority of the Catholic church, just as cell death serves the interests of a well-formed

multicellular body. New levels of cooperation create larger ‘selfish collectivities’ which in turn introduce new levels of competition where collective interests collide.

Some of the questions we need to answer have been neglected or virtually outlawed in social and biological anthropology:

1. To explain the ‘phylogeny’ of human culture we need not only to explain pre-cultural hominid evolution including the development of a culture ready brain (Whitehead, 2010a), but also the historical manner in which human culture came to be. Biologically oriented researchers tend to assume continuity between ape and modern human cultures, a view which conflicts with inferences drawn from ethnographic data (see below). Attempts to minimise the difference between ape and human cognition are not necessarily relevant to cultural origins. Chimpanzee colour naming (Matsuzawa, 1985a) and numeracy (Matsuzawa, 1985b) appear much less pertinent when one considers that many human languages today lack more than two colour terms or numbers higher than two. Although Davidson and McGrew (2005) did find that chimp tool making lacks the creativity displayed in human tool use, their suggestion that this may account for the differences between human and ape cultures is not convincing. Previously, McGrew subscribed to the view that any difference between human and ape tool making reflects a difference of need rather than ability (Wynn & McGrew, 1989), whilst Davidson dismissed Acheulian handaxes – perhaps the most ‘creative’ technological development during the Early Stone Age – as not tools at all, but functionless ‘discards’ (Noble & Davidson, 1996). Creativity in tool use is likely to be a consequence rather than a cause of modern human culture (*cf.* Gamble, 1993; Toth *et al.*, 1993). Innovations in stone tool design are few and far between prior to the Upper Palaeolithic in Europe, whilst in Africa there are people living today who use Middle Stone Age tools, but nevertheless have modern culture.

Technological ‘advances’ certainly do not correlate with phases of brain expansion during human evolution, suggesting that other abilities may be more critically important for understanding human difference and the emergence of modern culture (Whitehead, 2003, 2010a). Social anthropologists, for their part, seem reluctant to address phylogenetic issues. Knight’s (1991) menstrual sex-strike hypothesis provoked hostility in some quarters simply because any theory of cultural origins is regarded as heretical.

2. Ontogenic studies of how culture interacts with developing human individuals are more anthropologically acceptable, though the bulk of such research is still the domain of psychologists. Colwyn Trevarthen (1995), for example, has discussed ‘The child’s need to learn a culture’, and in subsequent research has found ‘proto-conversational’ structure in musical and playful interactions between mothers and infants (e.g. Gratier & Trevarthen, 2008). Studies of this kind demonstrate the spontaneous autonomy and creativity of human infants, who proactively explore and engage with their social and cultural environment, and are instrumental in their own enculturation. This implies the existence of a culture ready brain, and does not support simulation theory (which assumes children are merely ‘copy cats’) or the notion that culture is passively acquired through ‘social learning’. Biology meets culture in the spontaneous and joyful exchanges between infants, carers, and peers, and research of this kind holds some promise of greater integration between the behavioural sciences.
3. The question of ultimate mechanisms must address the ‘reproductive fitness’ of groups as well as individuals – a concept distasteful to many biologists. The genetic fitness of human individuals, however, can be seriously compromised by cultural engagement. An extreme case is that of the Marind Anim, whose warrior lifestyle led to endemic male sterility. Nevertheless, whilst genetic lineages were being extinguished, the cultural

system thrived and expanded by enforced conversion of neighbouring groups and abduction of their children (Keesing & Strathern, 1998: 120).

4. Finally, Tinbergen's question of proximate mechanisms implicates not only the workings of human institutions, as well recognised by social anthropologists, but also human behavioural universals and their associated brain functions, without which cultural systems could not operate.

We further need to define in what specific sense human culture might be considered 'anti-biological' (i.e. discontinuous with ape cultures). Lévi-Strauss (1949) distinguished human biological from cultural behaviour by noting that the former is universal whereas the latter is governed by culturally variable rules. The idea that social rules exist to control biological behaviour also provides the basic theme of Freud's (1930) *Civilization and its Discontents*. Marshall Sahlins (1960) further observed that in apes, sex controls society, whereas in humans, society controls sex. He inferred that human culture must have begun with a revolutionary inversion of a primordial ape-like social order. There has been a great deal of research on apes since Sahlins wrote, but this hardly detracts from the pertinence of his observation, or the fact that sexual modesty appears to be a uniquely human universal. Sahlins hit the 'anti-biological' nail squarely on the head, which is more than can be said for many behavioural scientists today.

The term 'anti-biological', defined in the context of major evolutionary transitions, implies emergent new levels of cooperation and competition. According to current biological theory only two kinds of intraspecific cooperation can evolve by sequential genetic mutations – kin-based (Hamilton, 1967) and reciprocal (Trivers, 1971). Breast feeding in mammals is an example of kin-based altruism, and blood sharing in vampire bats is an instance of reciprocal altruism. We humans, however, display a third kind of cooperation – 'strong

reciprocity' – which transcends such selfish-gene limitations. For example, following the Twin Towers attack in New York, rescue workers risked and sacrificed their lives to save complete strangers from whom they expected nothing in return (to call this 'reciprocity' is of course a biological misnomer). The suicidal terrorists likewise transcended selfish gene constraints. The 9/11 attack also illustrates an emergent level of competition, in this case a conflict between two ideologies. In the light of current biological theories of cooperation, it may be no coincidence that all known human societies are structured by formal systems of expanded kinship (lineage clans, nation states, etc.) and reciprocity (gift exchange, marriage, etc).

CULTURAL DISTORTIONS OF PERCEPTION

Societies governed by rules and structured by formal systems of kinship and exchange need to make their own artificiality appear natural, inevitable, and 'right' (Bourdieu, 1972; Marx & Engels, 1846). Social members need to be persuaded that the sacrifice of their own self-interests is worthwhile, or better still that no sacrifice is involved at all. Cultural fictions serve this end, falsifying members' perceptions of themselves and of the world they live in.

In societies with animistic belief systems, anthropologists have identified inflated (Johansen, 1954; Sahlins, 1981; Mauss, 1925) and fragmented (Leenhardt, 1949; Radin; 1956) or at least partible (Strathern, 1988) self- and other-perceptions. Animistic beliefs commonly include perspectival concepts which implicate highly mutable selfhood. A costume change in ritual, for example, may be held to create an animal identity, and transformation into an animal is an ever present danger should one happen to meet an animal in human form and mistake it for a human (Viveiros de Castro, 1998). These highly un-western self perceptions have been attributed to the formal systems which, I have suggested, were at one time essential to the emergence of large-scale cooperation in humans. Inflated

selfhood has been attributed to extended kinship systems (Mauss, 1925; Whitehead, 2003, 2010b), and fragmented or partible selfhood to processes and relationships of exchange (Leenhardt, 1949; Strathern, 1988). Animistic beliefs themselves have been linked to gift exchange (Mauss, 1925); whilst perspectival views with their associated instabilities of self-perception have been attributed to classificatory kinship (Viveiros de Castro, 1999).

Similarly, western perceptions of self and reality may be explicable in terms of political and economic processes (Whitehead, 2010b). Individualism, for example, suggests a politically motivated atomisation of society, since nation states have a vested interest in suppressing alternative forms of collectivised identity. Although the methodology of science is designed to minimise cultural and other sources of bias, as a political institution – dependent on public respect, employment, and funding – science tends to conserve western cultural biases such as individualism and the Protestant work ethic, and in addition creates collective deceptions of its own, including cognocentrism, logocentrism, genocentrism, and physicalism.

Individualism, thankfully, is in decline in the behavioural sciences, although the concept of the ‘social brain’ is still somewhat individualistic. In his 1999 discussion of the social brain, Adolphs does not mention music, song, or dance. Non-representational art is described as ‘non-social’, and his list of social brain structures does not include the massively expanded inferior parietal lobule or classical language areas. He even asks ‘Is it possible that language evolved primarily to subserve social behaviour?’ Such thinking persists despite widespread acceptance of the ‘social intelligence’ hypothesis, which holds that brain expansion in primates is an adaptation to the complexities of social life. If so, then any brain structures which are expanded in primates, relative to other mammals with similar body mass, are likely to serve social functions – and this applies to the bulk of the human brain.

In reacting against behaviourism, cognitive science emphasized the brain as a thinking rather than a doing organ. Although the computer metaphor for mind and brain is now thankfully in decline, the associated notion of ‘information processing’ remains dominant. This implicitly assumes an input-processing-output model of cognition which, however, represents only one half of a circular process (Cisek, 1999). Neglect of output-first brain functions partly accounts for the relative lack of neuroscientific studies of social displays such as dance and pretend play (see below). If brains only evolve in animals that move, then behaviour (output) must be phylogenetically prior to cognition (processing) and sensation (input). This is equally true ontogenetically – the foetal brain puts out efferents to muscles before it receives afferents from sense organs (Trevarthen, 1985). As the great educator, Maria Montessori (cited in Lillard, 2001), observed: ‘The mind will follow the hand’, which was her way of saying that doing comes before thinking. A child learns to count on her fingers before she can do so in her head, and plays with toys before she can internalise pretence in role-play. The mathematician Giuseppe Longo (1999) has argued that the whole of mathematics, even highly abstract ideas such as infinity and imaginary numbers, can be reduced to motor actions such as grasping.

The central belief of the cognoparadigm, however, is older than computing – namely, that human beings are set apart from the rest of the animal kingdom by a single quality known as ‘general intelligence’, and that this is the major reason for our relatively large brains. The idea that human intelligence is the ultimate pinnacle of biological evolution may be little more than colonialist propaganda, suggesting that ‘scientific’ societies are the ultimate pinnacle of cultural evolution – and hence morally entitled to dominate others who formerly managed perfectly well without the blessings of ‘modernity’. In addition, the Protestant work ethic and the industrial revolution have given us a world in which work is valued over play, logic over imagination, and science and technology over the arts. Hence

brains are conceived to be specialised for linear reasoning, ‘executive control’, and planning goal-directed instrumental actions. As Gazzaniga (2000) observes: ‘The human brain is a bizarre device, set in place through natural selection for one main purpose—to make decisions that enhance reproductive success.’ What is being denied here is just about everything that makes us human. Colwyn Trevarthen, based on forty years investigating childhood in various cultures, found that cognitive science simply cannot accommodate the proactive creativity of human infants. ‘Cognitive science,’ he writes, ‘restricting the role of motives and emotions, puts childhood play and imagination behind bars’ (personal communication).

Intelligence is notoriously as difficult to define as the ‘human difference’, and for the same reason. Hans Eysenck defined intelligence as ‘that which intelligence tests measure’. Merlin Donald (1991) pointed out that a great deal of what we regard as human intelligence is dependent on cultural aids such as pencils, paper, writing, and numerals. The ‘Flynn effect’ – the discovery that, since IQ testing began, scores have been rising at an average rate of three points per decade, and faster than that in ‘developing’ populations (Flynn, 2007) – suggests that what these tests measure is culturally acquired skills, though this inference is still contested.

There may be no such single ability as ‘general intelligence’ – commonly referred to as ‘*g*’ – which IQ tests are presumed to measure. Wason tests show that humans are much more successful when solving problems requiring social reasoning than matched tasks involving abstract logic. Brain imaging studies contrasting social with ‘general’ reasoning show that ‘*g*’ uses a subset of the brain areas involved in social reasoning, and these ‘general reasoning’ areas are more highly activated by social than non-social tasks (Gallagher *et al.*, 2000). It may well be that ‘*g*’ is a spandrel – or side-effect – of our evolved social abilities.

The belief that big brains are mainly an adaptation for non-social reasoning, however, persists with little empirical encouragement. As early as 1884, Sir Francis Galton examined more than 9,000 visitors to the London Exhibition and found to his great disappointment that eminent British scientists could not be distinguished from ordinary citizens on the basis of head size (Atkinson *et al.*, 1993: 458). The classic case of Phineas Gage, and similar cases since, show that massive lesions in prefrontal cortex do not impair intellectual functions, though they do lead to a catastrophic loss of empathy and social concern (Damasio, 1994). The parietal lobes can also suffer severe trauma without affecting standard IQ measures; and the entire right cerebral hemisphere can be removed without affecting IQ (Lishman, 1998). Hydrocephalic individuals with significantly reduced neocortical volumes can have high IQs, and even microcephalic dwarves can achieve good language competence (Walker, 1993).

The social intelligence hypothesis has done much to diminish the theoretical emphasis on *g*. Nevertheless, since 1993, there has been a spate of imaging studies attempting to correlate variations in human brain size with IQ. Researchers did indeed find a modest positive correlation and this has been acclaimed as a vindication of the studies and their underlying ideology. A meta-analysis of 37 such studies, involving 1,530 people, appeared in the journal *Intelligence* with the title 'Big Brained People are Smarter' (McDaniel, 2005). This analysis yielded a best estimate for the population correlation (*r*) of 0.33, suggesting that the intelligence factors measured are associated with around 11% (r^2) of brain volume, and so cannot account for the bulk of brain expansion during the last 2.5 million years.

One problem with these correlational studies is that the standard measures used are individualistic. IQ rating scales were not designed to assess factors of likely relevance to human brain expansion, but rather reflect the idiosyncratic educational needs of post-industrial nation states. In particular, the Wechsler Adult Intelligence Scale (1939) involves abilities such as numeracy and literacy which developed during a period when brains were

getting smaller, and similar criticisms apply to other tests used by the researchers. All in all, the small correlation between IQ score and brain size is impossible to interpret. These studies represent a prodigal waste of research funding, which could have been avoided had someone consulted a palaeoanthropologist (who would have recognised the evolutionary irrelevance of IQ tests) or a social anthropologist (who would have seen their cultural specificity). The very name by which scientists identify our species may be a red herring. Had Linnaeus plumped for *Homo ludens* or *Homo collaborans*, rather than *Homo sapiens*, we might be seeing more relevant research today.

Finally, this brief discussion of collective deceptions would not be complete without some mention of physicalism, the fundamental paradigmatic assumption pervading all western science, and the ground in which other deceptions – such as cogno- and genocentrism – are rooted.

Physicalism is the belief that the cosmos evolved for billions of years ‘in the dark’ – that is, there was no consciousness until brains of sufficient complexity evolved. The so-called ‘hard problem’ of consciousness (Chalmers, 1995) is to explain how consciousness ‘arises’ from ‘physical’ processes. There are many difficulties in understanding the brain, but there is no obvious reason why, at least in principle, its workings should not be entirely explicable using conventional scientific methods. The ‘hard problem’, however, is ‘hard’ in the special sense that we cannot even conceive of any way that consciousness could be explained in physicalist terms, or be sure that we would recognise an explanation even if we had one. If we accept the physicalist dilemma as valid, however, we do not so much have a ‘hard’ problem as an impossible one. For the ‘physical’ is paradoxically conceived as ‘all that exists’ and ‘everything except consciousness’.

Jordanova (1980) claims that physicalism resulted from a conflict of authority between two groups of dominant males – Enlightenment scientists, most of them aristocrats,

and the Christian clergy. Until the 17th century, the Church held a monopoly on the ‘truth market’, and scientists – though many of them were devout Christians – needed to establish their own authority, based on first-hand investigation as opposed to tradition. Scholars still cannot agree on what motivated Galileo’s decision that science should not study the whole of human experience, but only such phenomena as can be counted, weighed, and measured (Drake, 2001). This narrowly mathematical approach accounts in large part for the success of the physical sciences, but at the same time created a sharp distinction between what is regarded as ‘objective’ (meaning that it confers authority on scientists), whilst consigning the rest of human experience to the realm of the ‘subjective’ (meaning that the opinions of non-scientists are of little worth). This subjective-objective distinction underpins physicalism and creates the ‘impossible problem’ of consciousness.

THE CULTURE READY BRAIN

So far I have discussed a number of problems affecting the behavioural sciences, and suggested actions which social anthropologists could take to resolve them. I will summarise these in the concluding section. This section addresses one of them, namely anthropological neuroscience.

The social brain concept (Brothers, 1990), if rather long in coming, has proved fruitful, having led to important advances in theory and knowledge, as well as several new subdisciplines as brain scientists began to collaborate with social scientists. The union of social psychology with neuroscience led to social cognitive neuroscience (Singer, Wolpert & Frith, 2004), social neuroscience (Cacioppo *et al.*, 2004), and neural hermeneutics (Frith, 2003), whilst the union of cultural psychology and neuroscience produced cultural neuroscience (Chiao & Ambady, 2007).

Neuroanthropology, however, has an earlier and independent origin, an initiative coming from cultural rather than brain science (Laughlin & d'Aquili 1974; TenHouten, 1976: cited in Domínguez *et al.*, in press). A central aim of early neuroanthropology was to apply cognitive neuroscience to human cultural behaviour. This approach had one serious flaw. In the early decades of cognitivism, neuroscience focussed mainly on rather basic processes, notably vision, and paid limited attention to uniquely human abilities other than language and non-social 'cognitive tasks' (Singer, Wolpert & Frith, 2004). Consequently, it had little to offer to the first neuroanthropologists, who had to flesh out the lack of substance with borrowed ideas such as Jungian archetypes and hypothetical constructs such as 'neurognostic processes' (Laughlin, McManus & d'Aquili, 1992).

So, when proposing an anthropological neuroimaging study in 1999, even though the 'social brain' was gathering interest and imaging research into 'theory of mind' was underway, cognitive neuroscience still seemed to be somewhat lacking in anthropological utility. Since that date there has been a veritable tsunami of brain studies by social neuroscientists, so much so that it is difficult for any one individual to take a step back and review it all (Adolphs, 2010). Biological anthropologists have contributed to this research, many of them comparing human with ape brains. Such studies have often focussed on micro-architectural details – such as glia-neurones (Sherwood *et al.*, 2006), dopaminergic innervations (Raghanti *et al.*, 2008), cytoarchitecture of Broca's area (Schenker *et al.*, 2008) and minicolumns and inhibitory GABAergic interneurons (Raghanti *et al.*, 2010) – with no clear idea of what their function might be, how they interconnect or interact in the brain, or how they fit into an ethnographically informed view of human-ape differences. Most researchers adopt a rather narrow view of such differences, basically reducible to the 'two Cs' – Cognition and Communication – and reflecting two cultural biases – the cognoparadigm and the logoparadigm (e.g. Deacon 1992; Rilling *et al.*, 2007; Sherwood *et*

al., 2008; Allen, 2009), Such research is of course interesting and potentially useful, but the problem remains that no one is joining up the dots or providing a big picture of the brain or of human behaviour. As one refreshingly honest review explains, the basic aim of social neuroscience is to map social functions onto the brain, but the problem is that no one knows just which brain structures to investigate or which functions need to be mapped (Adolphs, 2010). Because of the cognitive/social intelligence paradigm, what is being universally ignored is output-first behaviours which, I submit, constitute the essence of our humanity. In the cartoons that I mentioned earlier, animals dance, sing, tell stories, assume different roles, wear clothes, smoke cigars, and so on. These are all output-first behaviours. For example, someone smokes a cigar (output) in order to stimulate nicotinic receptors in the brain (processing) and experience a sense of relaxation and satisfaction (input). Even where climate does not necessitate clothes, people conceal their genitals (output) in order to influence self/other perception (processing) and reduce sexual stimulation (input). The function of the output is to change the input (Cisek, 1999).

In my view, the early neuroanthropologists did little to moderate the cultural biases affecting biological anthropology and neuroscience. Rather, they repeated the same naturalising error as cognitive anthropologists – ‘decamping to the enemy’ and using western culturally-embedded ideas to explain global cultural phenomena. My interest was the other way round – to apply anthropological insights to neuroscience. Until we have more anthropologically relevant brain research, I do not think neuroanthropology can achieve its aim of applying neuroscience to culture.

There were two main reasons why, in 1999, I thought that anthropological brain research might help to bridge the gulf dividing social from biological anthropology. Firstly, at that time, both biological and social anthropologists generally ignored proximate mechanisms at the neurological level. As the brain is crucially involved at the interface between biology

and culture, I felt this could provide a nexus of common interest and convergent theorising. Secondly, I was dissatisfied by individualistic and cognocentric accounts of brain expansion during human evolution. Even the social intelligence hypothesis did not seem sufficient to explain hominid encephalization: in part because it applies to all primates; and in part because there is a lot more to human behaviour than just intelligence. A more culturally relevant hypothesis of brain expansion might help to allay the suspicions of social anthropologists roused by former abuse (Perry & Mace, 2010) and continuing misuse (Knight, 2008) of Darwinism, whilst raising awareness of ethnographically validated issues in biological anthropology, psychology, and neuroscience – in other words, identifying true cross-cultural universals of behaviour, and defining what needs to be explained.

I had no doubt about the kind of imaging studies that were needed. I took my cue from social mirror theory and the work of Victor Turner, who himself advocated anthropological brain research (Turner, 1983). Social mirror theory holds that ‘mirrors in the mind depend on mirrors in society’ (Whitehead, 2001) – that is, reflective awareness of one’s own experiences, and those of others, depends on social displays which make experiences public. The ‘social mirror’ gloss is relatively recent (Moses, 1994), but the basic concept was first worked out by Wilhelm Dilthey (1833-1911) and James Mark Baldwin (1894). Dilthey claimed that we can know our own ‘subjective depths’ through the ‘meaningful objectifications’ of others, and Baldwin held that the self was formed by a dialectical interchange with others. One influential version of social mirror theory was proposed by George Herbert Mead (1934). In common with Baldwin, Mead believed that self-awareness and other-awareness emerge simultaneously. But whereas Dilthey’s ‘meaningful objectifications’ and Baldwin’s social dialectic might involve anything from a facial expression to a song, dance, or verbal exchange, Mead specifically theorised that the human level of reflective consciousness was made possible by role-play. Role-play, according to

Mead, enables us to put ourselves in the shoes of ‘the generalised other’, and to observe our own minds from this third-person vantage point. Mead also emphasized the histrionic character of everyday life – that is, much of what we do whilst we are awake involves role-play. Irving Goffman (1959) applied Mead’s ideas to anthropology, noting how many commonplace routines resemble scripted mini-plays. Similarly, Victor Turner (1982) adopted Dilthey’s version of the theory but, like Goffman, also stressed the special significance of role-play. Turner held that human life alternates between the ‘structural’ role-play of everyday routine and the ‘anti-structural’ role-play of ritual, theatre, entertainment, and the arts. Of course, Turner – in common with most social anthropologists – was also aware of the cultural importance of other kinds of display, notably song and dance.

In the context of output-first behaviours, one conspicuous difference between humans and apes is that humans possess a formidable armamentarium of social displays – unprecedented in any other animal (Whitehead, 2001). Furthermore, we have three distinct kinds of display, two of which – play and performance – are functionally distinct from communication. Whereas communication is goal-directed and manipulative (Krebs & Dawkins, 1984), play is autotelic (Turner 1982) – pursued ‘just for fun’. Play has exploratory and developmental functions, but no instrumental goal or manipulative intent. Performance (for example, the balletic displays of dolphins or choral song of gelada baboons) is both playful and manipulative, and has functions additional to communication – namely, social grooming and entrainment. Burling (1993) pointed out that human communication has at least three modes – affective, mimetic, and conventional – where conventional communication includes several systems in addition to language. The same three modes have homologues in play (embodied play, pretend play, and games-with-rules) and performance (e.g. song-and-dance display, representational image-making, and the cultural arts).

According to social mirror theory, it is this extraordinary range of highly elaborated displays that accounts for human self- and other-consciousness. If the social intelligence hypothesis can only partially account for brain expansion in hominids, and bearing in mind that social intelligence must in part be dependent on social displays, then it is conceivable that social displays themselves may have been a factor contributing to human encephalization. Displays variously involve multimodal integration, timing precision, skill, and – in the case of role-play – modelling more than one mind in parallel. So the execution as well as the perception and interpretation of displays must make demands on the brain, and the selection pressures responsible for the proliferation of human displays (Whitehead, 2003, 2008, 2010a) would promote increasing size and/or connective complexity in relevant brain areas.

The ‘play and display’ hypothesis (*ibid*) holds that song-and-dance display had already emerged by 2.7 million years ago (mya), when the first unequivocal stone tools were being used for butchering meat. This triggered the first ‘grade shift’ during which the genus *Homo* appeared and, by 2.5 mya, cranial capacity had doubled from around 400 to 800 cm³. Song-and-dance display pre-adapted the brain, and provided the requisite levels of social insight and social trust, for the later expansion of mimetic and pretend play abilities. However, further expansion of the brain required dietary change (probably provided by cooking meat) to lift the metabolic and nutritional ‘lid’ on brain expansion (Aiello & Wheeler, 1995). This change triggered the second grade shift, from 0.5 mya (0.7 mya in Asia), when cranial capacity again doubled to around 1,500 cm³. Following the agricultural revolution around 10,000 years ago, there was a third grade shift in which cranial capacity was reduced by around 12%. If the ‘play and display’ hypothesis is correct, then this decline suggests a diminished need for some aspects of display behaviour – perhaps timing precision – in a strongly hierarchical society where social integration is imposed from above.

To compare the play and display hypothesis with other hypotheses of hominid brain expansion requires a detailed examination of archaeological and fossil data. I have reviewed this evidence elsewhere (Whitehead, 2003, 2008, 2010a) and can only sketch a broad overview here.

Archaeology

The archaeological record does not show overt evidence of social displays much before about 300,000 years ago, when late *Homo erectus* began to use red ochre and haematite, possibly as body paint (Watts, 1999). However, the fact that the first stone tools were used for butchering meat suggests very high levels of social trust immediately before the first grade shift. When chimpanzees capture an animal they tear it apart and eat it in a general mêlée – grabbing their share before the others eat it (Teleki 1973, 1981; Strum 1981). Apes cannot afford the luxury of butchering meat because they cannot trust each other to share such a valuable resource. Further, the earliest ‘butchery scatters’ are all at riverside sites, suggesting little fear of dangerous predators who came to drink there. A likely implication is that the early tool makers operated in large well coordinated groups, capable of outfacing powerful social carnivores such as lions and hyenas. Taken together, these two factors are at least consistent with a hypothesis of song-and-dance display at this early date.

During the second grade shift, late *Homo erectus* was not only using red pigments, but also began to assemble collections of attractive objects such as shells, pebbles, and fossils, some of them carried from considerable distances. Collecting behaviour continues in unbroken succession right through to the ‘wealth displays’ of modern times. During this second grade shift, engraved objects and pierced bone pendants also appear, and the first putative representational image – the Berekhat Ram figurine – is over 230,000 years old. This

object might be better thought of as a toy rather than ‘Art’ with a capital ‘A’, suggesting that the other display evidence is associated with expanding mimetic and pretend play abilities.

Fossil evidence

Fossil cranial casts lack surface detail, are often incomplete, and provide no information about internal structure of the brain, but they do provide broad clues concerning which parts of the hominid brain were expanded during the two grade shifts. Some enlargement of superior parietal and possibly premotor cortices is apparent in australopithecines, before the first grade shift (Tobias, 1987). By the end of this period of brain expansion, habiline crania show the first clear evidence of an enlarged inferior parietal lobule and prominent Broca’s and Wernicke’s ‘speech’ areas (*ibid*). Expansion of the prefrontal lobe, and further expansion of multimodal integration areas, began during the second grade shift.

Other changes apparent from around 0.5 mya include expanded hypoglossal and upper thoracic vertebral canals, and descent of the larynx to form a large tuneable pharynx. Dunbar (2009) interprets these changes as evidence for either speech or song, but the play and display hypothesis postulates an origin for song-and-dance display two million years earlier. An alternative explanation for these adaptations might be that song displays were becoming more distinctly melodic, requiring fine pitch control.

Imaging research

The role of neuroimaging in my own research is to establish how social display behaviours map onto the brain regions expanded during the first two grade shifts. I judged that role-play (introjective play) would be the most revealing and should be investigated first. Subsequent studies would include investigations of projective pretence (using objects as representations), song, dance, decorative art, and representational image making.

A role-play study, using functional magnetic resonance imaging (fMRI), was conducted in collaboration with David Craik and Robert Turner (Whitehead, 2003; Whitehead *et al.*, 2009). Despite its theoretical importance, there had been no previous imaging studies of role-play. In fact there had been no such studies of pretend play more generally, nor of other major forms of human display such as song and dance. Even today there has been relatively little neuroscientific research on social displays other than language, music, and narrative.

At the time of our experiment mirror neurones in monkeys (Rizzolatti *et al.*, 1996) were a relatively recent discovery. Mirror neurones fire when a monkey performs an action – such as grasping a nut to eat it – and also when the monkey sees another individual – monkey or human – perform the same action. This important discovery was hailed as having the potential to explain human sociality (Knoblich & Sebanz, 2006), including theory of mind (Gallese & Goldman, 1998) and language (Gallese, 1998; Rizzolatti & Arbib, 1998). Of course mirror neurones – common to monkeys as well as humans – cannot be sufficient to explain uniquely human abilities.

The discovery of mirror neurones might well have revived interest in social mirror theory (which tacitly implies the existence of mirror systems); but the Parma team, headed by Giacomo Rizzolatti, favoured simulation theory (Gallese & Goldman, 1998). Since then there have been over forty imaging studies of manual action and tool-use (review: Grèzes & Decety, 2001), but only four of dance (Calvo-Merino *et al.*, 2005, 2006; Cross *et al.*, 2006; Brown *et al.*, 2006), two of pretence (German *et al.*, 2004; Whitehead *et al.*, 2009), and one of role-play (*op. cit.*). Three of the four dance studies (Calvo-Merino *et al.*, 2005, 2006; Cross *et al.*, 2006) were not intended to establish the neural correlates of dance, but rather examined dancers watching familiar and unfamiliar dance moves to assess the effects of practice on the

sensorimotor mirror system. That is, dance was simply regarded as an interesting alternative to tool-use or manual grasping action.

[Figures 2, 3, 4, & 5 about here]

What this imaging research shows is, firstly, that tool use and object manipulation involve the presumed sensorimotor mirror system in opercular prefrontal (including Broca's area) and superior parietal cortices (Figure 2). Broca's area is classically associated with motor sequencing for speech, but it is clearly involved in many kinds of motor action. The superior parietal lobule is a navigational area which coordinates body-part movements in visually mapped space. Praxic manual actions involve these regions mainly in the left hemisphere, whereas communicative gestures are associated with more bilateral activity. Note that expansions of sensorimotor mirroring areas are apparent in very early hominid cranial casts.

Secondly dance, as would be expected, also activates the sensorimotor mirror system, and does so more bilaterally than tool-use. There were also several activation loci in auditory cortex, even when observing silent videos of dance – perhaps due to imagined music. In addition, there were bilateral activations in the temporal pole, precuneus/posterior cingulate, and inferior parietal lobule (Figure 3). The latter, a multimodal integration area, is known to have expanded during the first grade shift.

Thirdly, the activation pattern associated with projective pretence included areas that were the same as or close to those implicated by dance, though with no significant activity in the precuneus/posterior cingulate. In addition pretence activated areas of major social importance – ventromedial and orbital prefrontal cortex (Figure 4). This prefrontal region, together with parietal and temporal areas activated by pretence, have been regularly reported

in ‘theory of mind’ studies (Table 1). German *et al.* concluded that the intentional stance is automatically engaged when observing pretence. However, this finding is equally consistent with social mirror theory, according to which pretend play scaffolds the development of mental insight, suggesting that ToM may ‘piggy-back’ on brain structures originally dedicated to pretence.

Our pilot study of role-play was problematic in that it showed less brain activity in role than control tasks, possibly because the latter involved implicit role-play. However, many authors have assumed continuity between role-play, narrative, and daydreaming (or ‘theatre of mind’) (review: Marr, 2004). As we identify with characters in a story this presumably involves mental role-play. In our role-play study, major areas common to narrative were activated when participants switched from role to control tasks, perhaps indicating increased activity due to dissociation during the shift from explicit to implicit role-play. Figure 5 shows major areas activated in studies of narrative, in the expectation that a more definitive study of role-play will show similar neural responses.

Tasks related to story-telling activate all the areas associated with both dance and projective pretence, with the addition of dorsolateral prefrontal cortex, which may act as a ‘way station’ linking inferior parietal and ventromedial prefrontal areas. Several of these brain regions are also known as ‘deactivation areas’ because they are more highly active during supposed ‘rest’ periods than during laboratory tasks designed to investigate cognitive functions. Participant feedback suggests they are daydreaming when ‘resting’ – in other words telling themselves stories.

The findings summarised in Table 1 are consistent with an evolutionary sequence from song-and-dance display to role-play, as assumed by the ‘play and display’ hypothesis, suggesting that increasingly sophisticated forms of display evolve by extension or proliferation and re-adaptation of older mirror systems (as proposed by Arbib, 2002).

	Tool use	Dance	Pretend play	Role-play & narrative	Deactivation areas	ToM
Superior parietal	L	R<L	R+L	?		
Prefrontal operculum	L	R<L	R>L	R>L		
Inferior parietal	L	R<L	L	R+L	R+L	
Parietotemporal/superior temporal		R>L	R	R+L	R	R+L
Temporal pole		R+L	R	R+L	R+L	R+L
Posterior cingulate/precuneus		R+L		R+L	R+L	
Ventromedial/orbital prefrontal			R+L	R>L	R+L	R+L
Dorsolateral prefrontal				R>L		

Table 1. Regions of interest associated with three forms of social display, compared with tool use, deactivation areas, and “theory of mind” (ToM). Deactivation areas may equate with ‘theatre of mind’ (ThoM). R = right hemisphere; L = left hemisphere.

The ‘play and display’ hypothesis clearly needs – and justifies – further research into the neural correlates of social displays. The imaging evidence I have reviewed also illustrates one area where anthropologists and neuroscientists can usefully collaborate.

CLOSING REMARKS

I began this essay by asking why social anthropology is not accorded the respect that one would expect for a science of culture. From the treatment of human culture in other disciplines, it is clear that many scientists reduce human culture to something continuous with ape behaviour, denying the several Rubicons that separate us from non-humans. We live in a culture-denying culture, one which is largely antipathetic to social anthropology.

Reductionist, deterministic, mechanistic, cognocentric, logocentric, and/or genocentric accounts are supported by faulty Darwinian arguments. However, it is equally true to say that social anthropologists have not been entirely effective in refuting such arguments.

From the conflict between social and biological anthropology and the failure to define the ‘human difference’ I have inferred an ideological problem affecting western science as a whole, which is particularly evident in the ‘impossible problem’ of consciousness. The conceptual schism within anthropology appears to be continuous with the conflict between

materialism and idealism which arose from the Enlightenment and Counter-Enlightenment, and the commonly assumed incompatibility of science and spirituality.

I have suggested a number of actions which (at least some) social anthropologists could take to resolve the problems affecting the behavioural sciences, viz:

1. We need a systematic anthropological critique of western scientific ideology. What social anthropologists could be doing is exposing the political origins and self-contradictory character of collective deceptions in western science.
2. We need greater collaboration between social anthropology and other disciplines, both by engaging in cross-disciplinary debates (as in consciousness studies, for example), and multi-disciplinary research. Anthropological neuroscience and developmental psychology are promising options, since both address the interface between biology and culture. Since the advent of the social brain concept, brain scientists have become increasingly interested in the social sciences, and this is an opportune time for interdisciplinary collaboration.
3. All the behavioural disciplines need to pay more systematic attention to Tinbergen's four questions.
4. We need to define in what specific sense human culture might be considered 'anti-biological'.

To these I would like to add a fifth, which follows from the 'anti-biological' issue. I think we should eschew the words 'symbol', 'symbolism', and 'symbolic' throughout the behavioural sciences, at least until we have broken the habits associated with these terms. I say this because the notion of symbolism obscures the cause of all the problems I have mentioned above.

In disciplines that study the mind it supports representationalist understandings of cognition, and little-black-box theories of behavioural evolution. For example, Angeline Lillard (2001) – in her ‘twin earth’ hypothesis attributing self-consciousness to pretend play – feels it necessary to postulate a ‘symbolic module’ to get pretend play off the ground. It is far more plausible that pretend play is the basis of what we call ‘symbolism’ – using toy things to represent real things.

The problem is most evident in social anthropology, which commonly defines itself as the study of ‘symbolic culture’. Here the term is too often used as a ‘here-be-dragons’ plaster, applied to any cultural phenomenon which strikes the anthropologist as odd, mad, or baffling (*cf.* Sperber, 1975; Gellner, 1992; Whitehead, 2003). Worse, it conflates two developmentally related but functionally distinct phenomena – ‘honest fakes’ (Knight, 2008) and ‘dishonest fakes’ (Whitehead, 2008b).

As an example of an ‘honest fake’, Knight mentions two children agreeing to pretend that a broom is a horse. The broom-as-horse is clearly a fake, but there is no deceptive intent. Knight uses this idea to explain the origins of language, and defines human culture as ‘run-away pretend play’.

By ‘dishonest fakes’, I mean false consciousness or collective deceptions. Human cultures depend on wholly-believed-in make-believe – which just happens to be one widely accepted definition of the hypnotic state – incorporating a cohesive set of obfuscations concerning what it means to be human. Collective deceptions, operating in western science, account for all the problems I have mentioned above. Cognocentrism, genocentrism, and the others are all rooted in the ‘master deception’ of physicalism. The demise of a logically incoherent worldview may be the necessary precondition for anthropology to fulfil its proper and necessary role within the sciences.

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FIGURES AND LEGENDS



Figure 1. Two coyotes react to the sight of another animal behaving like a human being (based on an unidentified Chuck Jones cartoon. Redrawn by Jon James)

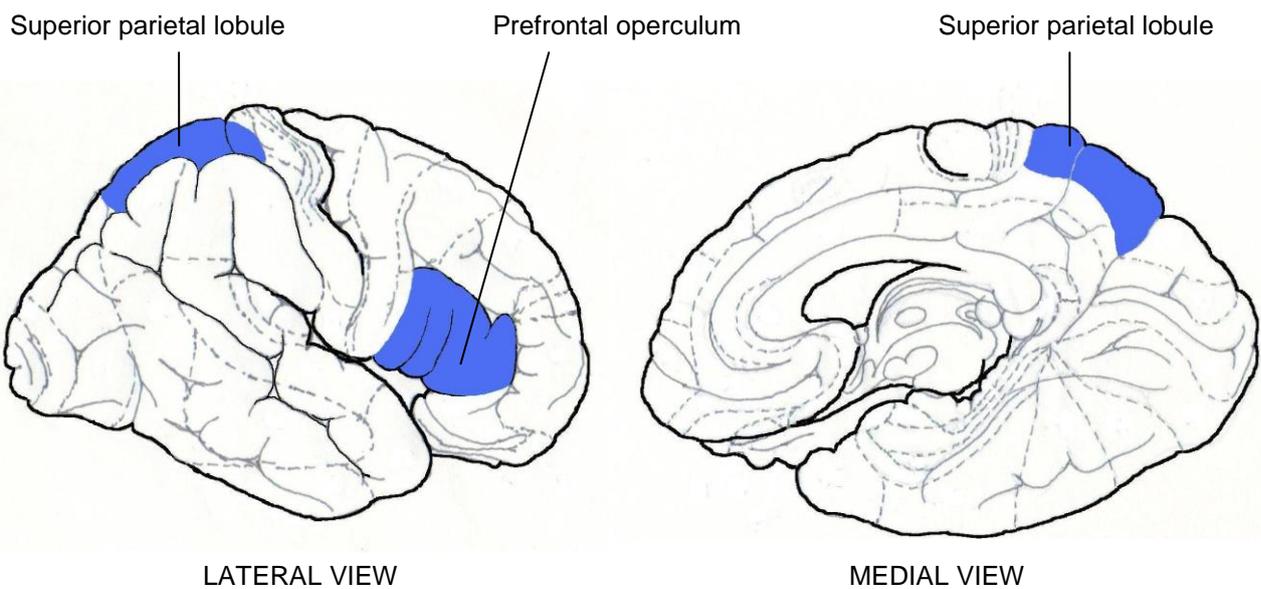


Figure 2. Right cerebral hemisphere showing sensorimotor mirroring areas. Praxic actions such as tool use activate this system mainly in the left hemisphere.

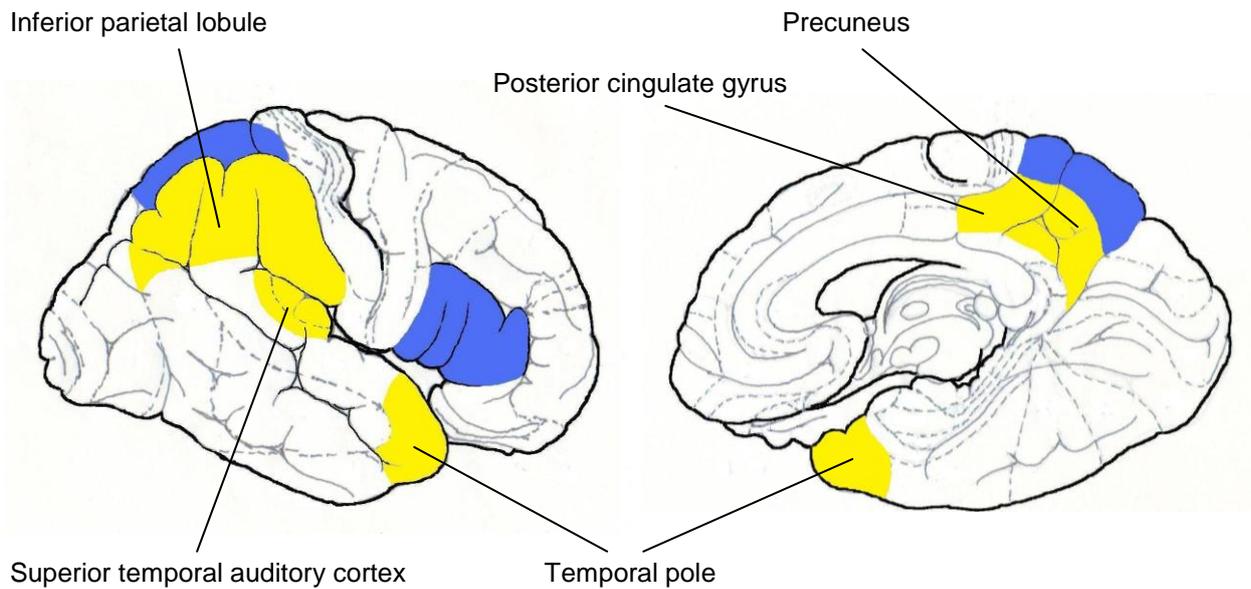


Figure 3. Major cortical areas in which activation loci were associated with dance. These include the sensorimotor mirror system

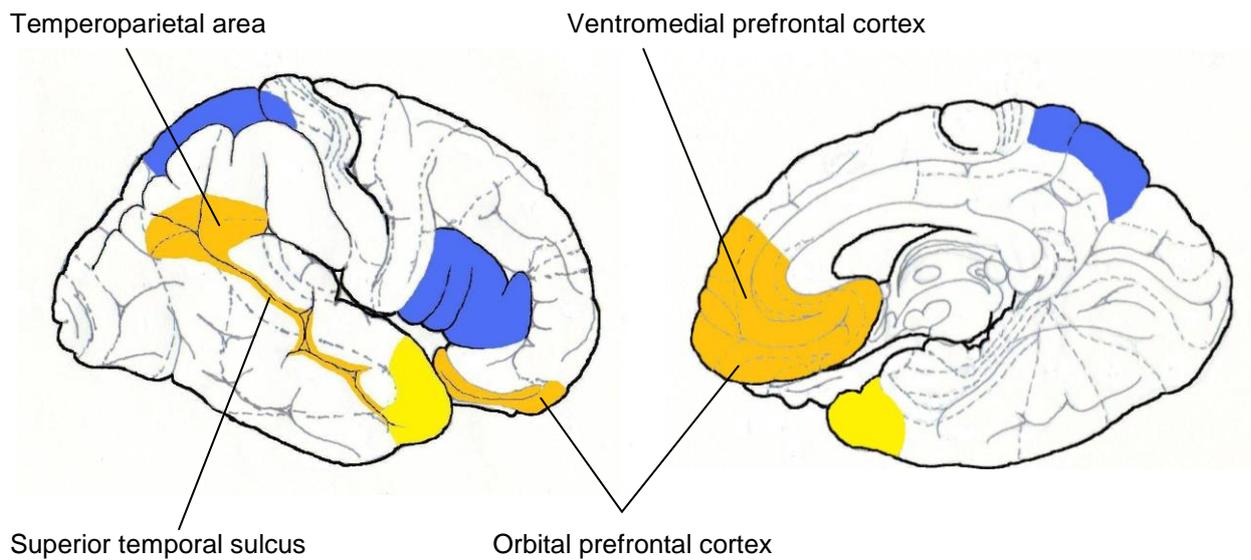


Figure 4. Major cortical areas in which activation loci were associated with projective pretence include some common to dance

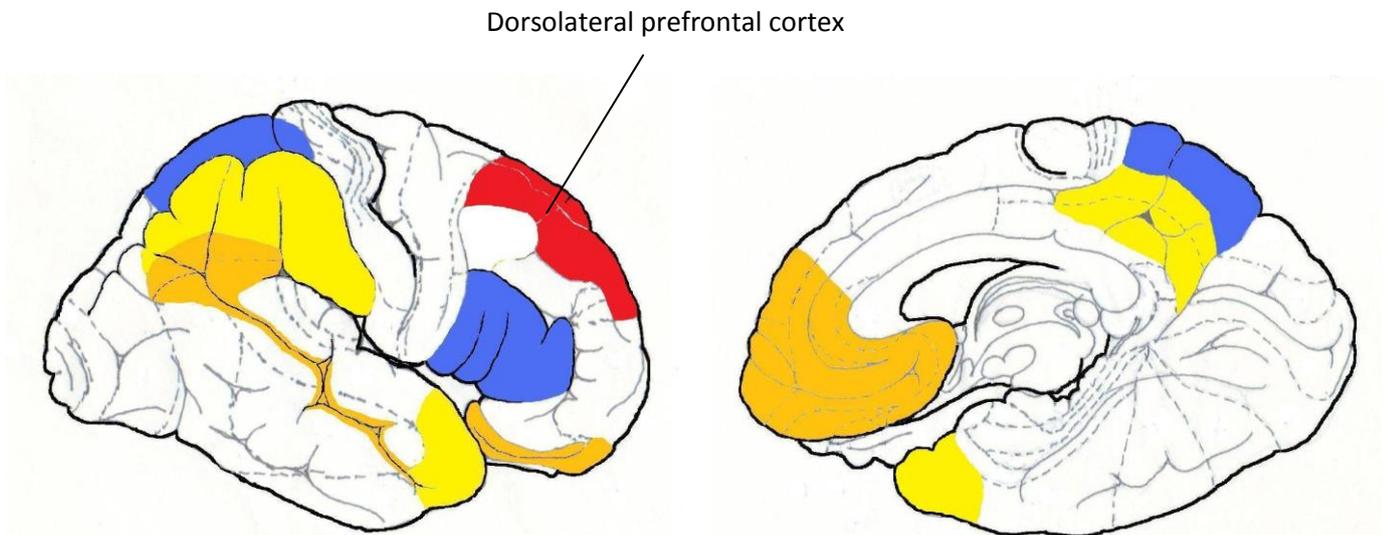


Figure 5. Major cortical areas in which activation loci were associated with narrative and role-play include all major dance and pretend play areas