From mimetic to mythic culture: Stimulus equivalence effects and prelinguistic cognition

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Donald's insistence that to understand cognition fully we must consider its emergence is probably the most important contribution of this book. Without ontogenetic and phylogenetic perspectives, contemporary cognitive science is indeed insufficiently constrained, and Donald's evolutionary stages indicate some possible limitations on (and extensions of) our models of mind. The precise nature of the emergence of mind will no doubt be found to differ from Donald's scheme, but, if it is not to be his, then some other such story is required. As a nonpaleontologist, I found the notion compelling that at least three major cognitive changes must have been accomplished in the transition from ape to *sapiens*.

I was uncomfortable, however, with the account of the second transition, from mimetic to mythic culture. For Donald's thesis to succeed he needs to demonstrate that cognitive abilities exist which (a) differentiate early humans from *Homo erectus* and the earlier ape culture, but which are (b) possible in the absence of language and (c) entailed in the emergence of language. Several sources of evidence are adduced to support this view. The case of Brother John dramatically illustrates the skills available to humans who have acquired but temporarily lost language, but it fails to show conclusively that there are intellectual skills beyond mimesis yet prior to language. Another line of attack, drawing on the theory of mental models, is initially more promising, but this fails too. As Donald himself states, "the specific adaptation that led to symbolic language is not easy to single out in Johnson-Laird's paradigm" (p. 232), though, as he rightly indicates, his overall thesis does not stand or fall on this basis.

Ironically, recent behavioral rather than cognitive research may help bridge Donald's gap. Studies of stimulus equivalence and transfer of function have demonstrated a range of skills that are exhibited fully only by cognitively developed humans, but that can be displayed in simpler ways by prelinguistic children, adults who have lost aspects of language, or children in the early stages of acquiring language (e.g., Barnes et al. 1990; Devany et al. 1986). Yet, despite rigorous training attempts, none of these skills has been successfully acquired by any primate, whether skilled in American Sign Language or otherwise (e.g., Dugdale & Lowe 1990). The essence of equivalence effects is as follows: humans who are taught to pair arbitrary stimuli, such as abstract shapes or nonsense syllables, in a limited number of ways, are then able to exploit other interstimulus relationships that have not been specifically taught. To take a simple example, having been taught to select stimuli B and C in the presence of A, subjects are capable of selecting A (rather than an alternative stimulus) in the presence of either C or B, and B in the presence of C.

Simple operations such as identity, symmetry, and basic transitivity are easily captured by equivalence research but more complicated transitive chains, which can also be brought under contextual control, can also be demonstrated, as can other relations, including sameness. It can be shown that such effects do not merely involve discriminative responding to stimuli but depend on the subject's responding to the complex of relations between them. It is thought that such relational responding emerges through repeated and detailed interactions during development with key stimulus domains so that in later life a small amount of learning allows a large amount to be derived for free. The human brain is obviously designed to learn how to learn, and does so with accomplishment. Transfer of function phenomena extend these effects and demonstrate how a response, originally paired with one stimulus, can become attached to a range of other stimuli through a network of stimulus equivalence relations. Thus, it becomes possible to respond in essentially similar ways to domains of stimuli that differ in content but have the same structural interrelationships. Combined together, equivalence and transfer effects have been used to develop non-Skinnerian, but still behavioral accounts of language development, in which, for example, the interaction between semantic and syntactic variables can be mimicked (Wulfert & Hayes 1989); they can also be used to illuminate general situations in which domain invariant information is teased apart from domain specific information (Barnes & Hampson 1992). It seems likely that such effects also underpin more complex analogical and metaphorical reasoning processes (Lipkens 1992).

Donald acknowledges the need for such abilities when he hypothesizes the initial use of language "to construct conceptual models of the human universe" (p. 215), and the need for a metaphorical thought which "could compare across episodes, deriving general principles and extracting thematic content" (ibid.). At its simplest, stimulus equivalence behavior allows humans to compare and contrast across episodes in which there are obvious physical similarities and differences, but through time such responding can become "arbitrarily applicable," allowing concepts like sameness, difference, opposition, and so on to emerge without reference to any underlying (perceivable) physical dimensions (Steele & Hayes 1991).

A further observation concerns the putative transition beginning in the later development of ape-culture. Donald entertains the social-intellect and self-awareness hypotheses as possible causes of cognitive improvement, and asks: "Is it possible that the cognitive adaptations that were needed to allow large groups to cohere were the same that enabled self-awareness?" (p. 147). A persuasive argument along these lines has been offered by Humphrey (1983), who claims that introspection may have evolved precisely for this reason, to allow its users to become "natural psychologists" and to predict the behavior of their fellows in social settings. Exactly where in the order of emergence introspection arises can be disputed; Humphrey would probably put it later than Donald, but its relatedness with social cognition looks very likely.

There is much additional food for thought in Donald's book. The stress on external storage will give added impetus to current work in this area. The invention of symbol systems is crying out to be studied. The order of emergence of episodic and semantic memories reverses the order that many psychologists, perhaps uncritically, take for granted, and the extensions of working memory are intriguing, if controversial. Verdict? A great synthesis that should be treated as such, with the benefits of the whole not forgotten even if many of the parts are shown to be inaccurate.

The evolved mind

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I reviewed Donald's book favorably just a few months ago (Jerison 1992) and make the present comments on the basis of notes but with little rereading, so they represent what stuck with me rather than a new and immediate impression. In my earlier review I wrote: "Of the analyses of mental evolution, most are better at evolution than at mind. Donald is better on mind, with an up-to-date picture of human and animal cognition. I recommend the book." Having praised the book, I can now comment more contentiously, to raise issues that need special discussion with the author.

The book is an excellent introduction to cognitive psychology's evolutionary dimension, with important additions from cultural anthropology and quantitative neurobiology. I thought the presentation of cognition was especially illuminating, in particular the discussion of the aphasias. Donald presents an unusual memory model for the growth of the mind, one that is much more acceptable for evolutionary analysis than Piaget's recapitulationist model, which I criticized in these pages (Jerison 1982). But Donald is not strong on the fundamentals of evolutionary biology. He remains close to Piaget and the Aristotelians by implying a progression of adaptations to increasing levels of excellence with his episodic, mimetic, and mythic grades of culture. The implicit scale of nature raises warning signs to evolutionists (Hodos & Campbell 1969). There may have been a disclaimer of teleological intent, but I don't remember one.

More serious than the philosophical slip into scales of nature, none of the discussions of evolutionary theory were satisfactory. There was a brief reference to punctuated equilibria contrasted with gradualism and a nice section on pp. 137–41 contrasting Dunbar's (1993) social structure approach with the ecological approach of Harvey and his colleagues (cf. Harvey & Pagel 1991). The issues concerned the rate of brain enlargement and the selection pressures that led to it in primate societies. (For reasons not clear to me, Donald seems to prefer Dunbar over Harvey.) On cladistics, sociobiology, fitness, and other topics that have exercised evolutionists, there was nothing, yet these should be factored in somehow. I offer a three-item, didactic bibliography, on evolution rather than mind, that could begin to fill the gap: Dawkins (1987), Harvey and Pagel (1991), and Maynard Smith (1982). [See also Maynard Smith: "Game Theory and the Evolution of Behaviour" *BBS* 7(1) 1984.]

Donald's evolutionary biology is mainly a recital of fossil history to compare more progressive with less progressive adaptations. I have done exactly this, and I know that it is not enough. We have to seek such scenarios, and that is part of the story, but they should suggest specific selection pressures. What real advantage accrued from an enlarged brain if the enlargement was almost too small to measure (e.g., an increase from 300 g to 400 g)? If hominids with 800 g brains replaced those with 400 g brains, what was so great about them? And what is so great about being so smart? The answers must be at least partly the sort that Donald offers, but his chain of reasoning has gaps. It might be clearer were it presented as part of a formal model of the sort that evolutionists have offered for other traits that have evolved.

As a general rather than a technical book, Donald's is excellent in combining breadth with depth. I found no scholarly gaps in his discussion of cognition. I found one important omission in primatology, and it can be filled by a single reference: Cheney and Seyfarth (1990; see also multiple book review: *BBS* 15(1) 1992). That book was certainly too recent to be included, but the Seyfarth-Cheney work on social signals in monkeys has appeared in many publications during the past decade and is important evidence for the analysis.

I enjoyed the emphasis on paleoneurology and offer the following unusually well-documented reviews to supplement and update Donald's bibliography: myself (Jerison 1991) for an updated general perspective. For the primate literature: Falk (1987), Martin (1983; 1990), and Tobias (1967; 1971; 1990). On quantitative comparative neurobiology: all of the above, and Armstrong (1990) and Hofman (1989).

I have space for a final criticism, and not just for Donald but for most of my evolutionist friends who are concerned with mental evolution. Complex social behavior is almost always seen as a fundamental advantage that accrued to hominids as they became bigger brained and more nearly human. Yet complex social behavior evolved in many other animals, in particular, in invertebrates among ants and bees, and this was without much if any "brain" (head ganglion) enlargement. I would not argue that complex behavior is not related to brain enlargement in mammals (though Macphail [1987] might). Rather, I would argue that the selection pressure cannot simply be in favor of complexity, group enlargement, or higher mental processes. It must have been a kind of engineering challenge, as it were, an environmental challenge that could only be met by "investing" in more neural information-processing tissue. If social complexity were the requirement, I would argue, it could be achieved and controlled by a much smaller amount of neural tissue than has appeared in highly encephalized species of mammals. (The same is true of complex communication.) The challenge was to cognition, of course, to "knowing the world," and it would have to be fundamental to scenarios of the sort developed by Donald.

The gradual evolution of enhanced control by plans: A view from below

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I find Donald's proposal of a mimetic stage preceding language in hominid cognitive evolution both plausible and suggestive, but I doubt both the biological need for a special "mimetic controller" (p. 196) and the explanatory value of positing one. Why should a new capacity for modeling episodic actions bring with it the capacity for modeling the more permanent features of social structure and a sensitivity for rhythm as well? And why should another spatial map, when added to the dozens we had already, make a cognitive difference of the desired kind? I shall suggest that the early cognitive transitions among hominids may have been mediated more by an incremental change in motivational-affective-cognitive connectivity that enhanced an already present capacity for the control of action by plans.

Donald's way of drawing the distinction between hominid and ape cognition seems unfair not only to the apes but also to his own account, which allows them episodic awareness and perception of events, including social events (pp. 148–57), and episodic memory (p. 150). How then can "their lives" be "lived entirely in the present" (p. 149) and in egocentricity (p. 171)? Donald infers from the presence of pointing behavior in 14-month-old humans, but not in chimpanzees, that chimpanzees lack "the ability to attribute intention to the mother's gaze. Chimpanzees lack this central component of intentional gazing and pointing: the ability to realize the intentions of others" (p. 171). But chimpanzees in the wild respond to what can be learned from each other's gazing at others as well as at themselves (Goodall 1986, p. 384) and show knowledge of each other's intentions and intentional communication (Goodall 1986, pp. 36–39, 124–25, 139, 143–44, 576–92), capacities that laboratory experiments with juveniles to some extent confirm (Premack & Woodruff 1978; Woodruff & Premack 1979). Pointing seems a special case, a species-specific adaptation of our own that does not come much more naturally or comfortably to the top-heavy chimpanzees than pointing with the limbs we use for locomotion comes to us. Still, it has been observed in the laboratory (Woodruff & Premack 1979) and other intentional gestures are found in the wild (e.g., Goodall 1986, pp. 141, 144, 570), which Donald denies (p. 128). Against Donald's claims, chimpanzee thought and communication often seems to be intentional (p. 171) and representational (p. 173) in the same way our own unreflective thoughts and communications surely are.

Still, our mimetic culture of "re-enacting and re-presenting" (p. 169) seems, as Donald says, to be one way we are beyond the chimpanzee. If it is not by some unique power of representational thought or communicative intention, then by what? The answer may lie in our greater passion for rethinking and improving upon our thoughts and actions, mainly a functional and cultural difference, but presumably facilitated at some stages by heritable changes in our brains. If, partly following Damasio et al. (1990), we regard obsessive-compulsive personality disorder (American Psychiatric Association 1987, pp. 354–56) and sociopathic or antisocial personality disorder (APA 1987, pp. 342–46) as marking a dimension from overcontrol to undercontrol by plans, worries, regrets, and values, we may regard hominid cognitive evolution as a movement from the impulsive toward the obsessive end of the spectrum. It may be fanciful to suggest that formerly promiscuous australopithecines pair-bonded because they became obsessed with each other or became bipedal because they couldn't let go of their favorite sticks and stones. But some fairly obsessive personalities must have emerged by the time of the advanced tool-making cultures. Chipping and flaking stones to the point of perfecting a demanding craft requires not a carefree ape but rather an obsessively perfectionistic one – one who dwells not just on past failures but also on minor failings and learns from both. To invent, it is not enough to represent. One must also be motivated. If inventors did not care deeply, even to the point of major emotional investment, the hard work of pondering and experimenting would never get done (Ochse 1990).

It is plausible, if still speculative, to implicate the ventromedial or orbital prefrontal cortex in these functions as well as in their pathologies of excess and defect, as Damasio et al. (1990), among others, have done. The connectivity of these areas and the consequences of damage to them suggest "a primary deficit in accessing the central representations of reward and punishment" (Goldman-Rakic 1987, p. 402). That affect and motivation are needed for intelligent control emerges most poignantly in the case of patient EVR (Saver & Damasio 1991), who, with language, means-end strategic thinking, declarative social knowledge, and a general above-average intelligence otherwise intact, was unable to exercise ordinary judgment in making choices in real life because of ventromedial prefrontal lesions and the resulting higher-cognitive/affective-motivational disconnection. No cold representation of a goal, it seems, can provide intelligent "command" or "control" (p. 369) if unconnected with the older systems; we still need to choose options and to revise goals in the light of experience.

From this viewpoint, our specialized neocortical acquisitions are only peripheral devices relative to our brain's common mammalian core. It may be not so much that apes "cannot represent a situation to reflect on it" (p. 160) as that they are not moved to reflect long on the past when they do recall it – because they are not moved by retrospective pride, regret, guilt or shame. The major gap the hominid brain had to cross, then as now, seems more like one between new ways of knowing and older ways of feeling and deciding than a purely representational gap. Apes already deal with this problem when they use rhythmic sound and motion to self-regulate arousal and to channel attention so as to keep themselves on task. Perhaps rhythmic tooth-clacking, foot-tapping (Goodall 1986, pp. 131, 389–91), and handclapping (de Waal 1989, p. 162) chimpanzees also increase motivation to groom by mimetically representing the louse-biting that is the groomer's most intense reward. Mimetic representation and higher cognitive control come in all degrees – but they must always accommodate to local taste.

Language equals mimesis plus speech

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Donald's distinction between mimetic and mythic representations corresponds closely to the philosophical distinction between natural signs and conventional signs. Although convention is a distinguishing characteristic of human language, its emergence from mimesis would not be difficult to explain. On the other hand, intentionality and referentiality are difficult to explain and are the characteristic features of human language. Mimetic representation plus a speech adaptation is therefore sufficient for mythic culture and we need not hypothesize a second representational transition.

A natural sign (Grice 1957; Schiffer 1972) bears some nonarbitrary relation to its referent. In the case of utterances, this relation is usually auditory. For example, if it is common knowledge among the members of some population that the utterance "grrr" resembles the sound dogs make when they are angry then the members of that population may use the utterance "grrr" to communicate to one another that they are angry; that is, they may use the utterance "grrr" to represent anger.

This kind of representation by natural signs is precisely the sort that Donald calls "mimetic" – the ability to mime, or reenact, events (p. 16). He asserts that mimetic representation is intentional: the objective of mimesis is the representation of an event, which requires an ability to understand the intentions of others (p. 171). He also asserts that mimetic representation is referential: mimetic signs are distinguished from their referents (p. 172).

If a system of representation by natural signs is already intentional and referential, the only difference between it and a system of representation by conventional signs is the arbitrariness of the relation between sign and referent. A conventional sign (Lewis 1969) bears only an arbitrary, nonnatural relation to its referent. The distance between an intentional, referential system of natural signs and a full-blown conventional language is not so far.

Given a system of representation by natural signs that is already intentional and referential, it is easy to understand how conventions would develop. In a mimetic representation, the sign is already distinguished from its referent and taken to be a representation of it. Successful mimetic communication consists in the speaker and hearer's taking the utterance as something which it is not (for example, taking the utterance "grrr" as a real expression of anger and at the same time understanding that this is the effect it is supposed to have). Once this ability is in place, the arbitrariness of the sign is irrelevant. In the early stages of representational development, natural signs may help with the identification of referent from sign; however, once intentionality and referentiality have been achieved, signs would naturally become stylized and conventionalized over time.

Suppose, for example, that "grrr" is uttered initially in a very careful, highly articulated way. If the representation is to be mimetic in Donald's sense, the utterance must be different enough from the natural sign (a real "grrr" by an angry dog) that a hearer could distinguish them. If a hearer can distinguish an utterance from its referent, the sign is not natural – it is not the sign itself, but some nonnatural substitute for it. This particular substitute is used from among an unbounded number of choices (of inflection, volume, pitch, tone, facial expression, physical proximity, etc.). The selection from among these options is arbitrary to at least some degree, and the utterance is conventional to that extent. Initially, there may be very few degrees of freedom: only very few utterances, all of them within a very small deviation of each other, will produce the desired effect rather than being taken as a real "grrr" or interpreted some other way.

However, as more members of the community become familiar with this kind of representation, the sign will naturally become more conventional. Over time, more utterance types bearing less relation to the natural sign will produce the desired effect. Eventually, the sign may become completely conventional, in that it is used without any reference to the natural sign that inspired it. (Consider, for example, the English word "growl," which is learned and used conventionally, although it bears a strong onomatopoetic relation to its referent.)

When a system of representation is conventional (i.e., when its signs bear arbitrary relations to their referents), it is mythic in Donald's sense. With a system of conventional signs, a culture could describe and define arbitrary events and objects. But the transition from mimetic to mythic representation that I hypothesized in the previous paragraph does not require any new cognitive skills or apparatus. Once the mechanisms of intentionality and referentiality are in place for natural signs, the stage has been set: conventional signs are only a refinement. Donald himself gives such an argument, calling the intermediate stages "gestures" (pp. 220–25).

I do not want to suggest that the development of spoken language out of mimesis did not require or cause anatomical or cultural changes. Rather, gestural mimesis has all the features of language except speech; or to put it another way, natural human language is vocal gestural mimesis.

Donald has two arguments against this position. The first is that language requires inventing symbols "from whole cloth" (p. 3, see also p. 235). I have already demonstrated that this is not so: the invention of symbols is a natural and predictable consequence of mimetic representation that requires no new cognitive skills. Donald's second argument, that language is distinct from vocal mimesis, depends on data from aphasia studies which show that mimetic and linguistic abilities can be lost differentially. All the data Donald cites (p. 224), however, are consistent with the thesis that mimetic and linguistic representation are the same whereas gestural production and speech production are handled by different modules.

I conclude that Donald has failed to provide a cognitive motivation for the second transition. Developing conventional systems of representation from natural ones would not require any massive changes in cognitive architecture. The intentionality and referentiality of human language are difficult to explain, but once we have them, we get convention for free. Given Donald's account of mimetic representation, the development of speech would have both required and resulted in massive anatomical and cultural changes, but it could not have constituted a new form of representation.

Lessons from evolution for artificial intelligence?

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In this ambitious book, Merlin Donald has tried to give an evolutionary account of how and why modern humanity has ended up with its cognitive abilities. Of necessity highly speculative in places, this account draws on evidence from a multitude of disciplines, including archeology, anthropology, neurology, and cognitive science, to create a plausible theory of the evolution of the modern mind. In this theory human cultures have gone through a variety of stages – episodic, mimetic (the most speculative of the stages), mythic, and finally theoretic. By making the assumption that evolution builds on what went before, and by trying to give an account of the cognitive abilities needed to support each type of culture, this theory results in a proposal about the high-level architecture of the representational systems in our minds. This architecture parallels the stages of cultural evolution proposed by Donald – so there is an episodic module, a mimetic module, a linguistic module, and finally a visuosymbolic module. Each of these modules builds upon the preceding modules; this enables Donald to give an account of the origins and importance of gesture while speaking, for example, because gesture arises from our mimetic abilities and our linguistic abilities are founded on them. It is interesting that the connections from the visuosymbolic module to the other modules and to external knowledge stores may still be evolving. Indeed, one of the conclusions of this book is that human evolution is now no longer a matter of anatomy – it is operating largely on a cognitive and cultural level, with our external knowledge stores effectively acting as part of our mental "hardware." To study the future evolution of the human mind will therefore involve looking at the evolution of this external symbol store and at the parallel evolution of our cognitive abilities that goes along with it.

There are several implications of all this for artificial intelligence. First of all, if our linguistic abilities are not simply provided by an isolated computational unit that evolved spontaneously as a whole but are intricately linked to, and based upon, earlier and more basic abilities (the episodic and mimetic abilities that earlier hominids possessed), then perhaps we should be trying to build artificial episodic and mimetic minds first, before trying to build machines with linguistic abilities. As Donald himself points out, it is just about conceivable that we could model episodic abilities at some point in the foreseeable future. We already know quite a lot about the processes underlying low-level vision and about how connectionist networks can categorise their input data. Since the main ability of episodic minds is that of classifying and remembering events (episodic perceptual data), this might, with a large stretch of the imagination, be achievable.

How to achieve an artificial mimetic mind is rather more problematical. It would presumably, at the very least, involve us in embedding the "mind" in a robot, in a society of other robots with similar abilities. Such a robot would have to be able to recall some event, and reenact (mime) it to other robots for the purpose of communicating information. Equally, it would have to have the ability to understand the purpose behind such mimes performed by other robots. This kind of level of intelligence and intentionality is certainly way beyond anything currently achievable. However, it should be noted that we can't really even begin to address this until we know how episodic representations work, since episodic abilities are (according to Donald's theory) a prerequisite for mimetic ones.

All this is closely connected to what Harnad (1990) calls the "symbol grounding problem." This is a version of the problem pointed out by Searle (1980), that a closed symbolic system seems to lack any potential for understanding the symbols it is manipulating. Any meaning they possess is attributed to them by an outside observer. Harnad tries to address this by ultimately grounding the symbols in physical reality via perceptual and effectual apparatus. It is interesting that Donald's proposal to base our linguistic abilities on perceptual (episodic) and effectual (mimetic) abilities is similar, although in some ways it is more specific about exactly what underlying representational properties are needed before a symbol system can be built on top of them.

If Donald's theory is right, then it may be that the subarea of Artificial Intelligence known as Artificial Life is proposing the right way to proceed. Rather than tackling the pinnacles of human cognitive abilities (like language) we should perhaps be aiming at understanding much simpler life forms. At the moment, insect level intelligence is about all that has been understood, but if we can build from these to higher mammalian lifeforms then the ultimate goal of understanding the human mind might at last be achievable. The contribution of this book to AI may lie in its proposal of an architecture that gives us definite milestones to work toward, milestones that model our evolutionary history. It certainly places a lot of responsibility on those working in the HCI (human-computer interaction) field, as they may be shaping the future evolution of the human mind.

Correct data base: Wrong model?

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Donald's synthesis of the neuropsychological, primatological, and archeological-cultural data, the first attempted, is brilliant. His chapter on "external information storage" is a classic. However, I demur slightly.

1. Donald asserts that chimpanzee capacity and culture is "episodic." Unfortunately, studies of chimpanzee and ape behavior and capacity in the wild and laboratory have tended to be studies of "episodic" behavior and capacity, much as early studies of brain function and dysfunction tended to be observations primarily of localized function and capacity. There is growing evidence of the inadequacy of these data. Boesch and Boesch-Ackermann (1991) observed chimpanzee mothers at the Tăi National Preserve (Ivory Coast) "providing their young with hammers and then stepping in to help them when the inexperienced youngsters encounter difficulty. This help may include carefully showing how to position the nut or hold the hammer properly." The mother had underlying potential "mimetic" and "pedagogical" skill, could voluntarily "represent knowledge," had an abstracted, "nonepisodic" generalized knowledge of tool-mediated equations and skills and a sense of "self" *and* of the limitation of knowledge in the "other." The authors noted that Tăi chimps use tools in nineteen ways and have six ways of making them, having thus generalized beyond the episodic. I have additionally suggested the presence of the far more important "potential variable capacity" (PVC) for thinking, mapping, and modeling behavior and concepts in time and space (cf. Boehm 1988; Wallace 1989).

2. Donald states that *afarensis* had primarily "episodic" capacity. The bipedal, two-handed, visually mediated "potentially variable capacity" (PVC) of the australopithecine probably initiated a significantly more complex referential realm. This necessitated a more complex visual evaluation and mediation of processes, relations, objects, materials, and the mapping and modeling of these and of group action in time and space. *Afarensis* could make and use a greater range of tools in a greater range of referential contexts than the chimpanzee. That visually mediated PVC probably was not and could not be primarily "episodic."

An increase in mother-child "pedagogical" exchange by the *voluntary* vocalized nonlinguistic (and gestural?) marking of the negative ("don't"), affirmative ("do," "approved"), and of value ("pay attention," "important," "unimportant," "kin-related," etc.) would not only have begun the *cultural* referencing and categorization of behaviors, processes, relations, and objects but it would have begun the symbolic structuring of a protohominid, value-marked, cultural-social realm. That form of affirmative-negative marking is incipient among the chimpanzees and has been noted in the wild. By its incipience and potential the mode would have been subject to selection as the brain enlarged, the articulatory apparatus altered, and the hominized realm became referentially more complex. "Abstraction" and "categorization" would thus have been instituted at a nonlexical, nonsyntactic and increasingly nonaffective level, first during ontogeny in the child, then more adaptively in the more complex subsistence and social realm of the adult (cf. Gibson 1983). Even within such early modes, aspects of "time," "periodicity," and "space" would have begun to be experientially marked as an aspect of cultural structuring (cf. Wallace 1989). Such capacities, probably already "potential" with the australopithecines, would have become significant with *habilis* (cf. Steklis 1983).

3. Donald suggests that *habilis* had not reached the stage of "mimesis" or "proto-language." Tobias, our leading *habilis* specialist, has, with others, indicated that "the two major cerebral areas governing spoken language in modern man are well represented in the endocasts of *habilis*" (Tobias 1987, p. 741). Tobias and others have, as a result, suggested the beginnings of some level of habiline "proto-language."

I have further suggested that the explosive increase in *habilis* brain volume led to an increase in the capacity for the visual evaluation and mediation of an increasingly complex, two-handed, tool-mediated referential realm, including an increased capacity for the visual evaluation and mediation of more complex problem-solving and more complex social relations – the creation, therefore, of a more complex realm in time and space. Referential marking involving *both* vocal and gestural communication were probably present to differentiate this increasingly complex realm. The adaptive success and corollary evolution of vocal and visual forms of referencing probably instigated the selection for, or a "punctuated" mosaic leap in, these capacities, leading to *Homo erectus* and the near modern brain. Laitman et al. (1992) indicate that *erectus* laryngeal morphology suggests a capacity for linguistic articulation perhaps at the level of a six-year-old child. A six-year-old child has syntax and a lexicon, and it would have been the acculturated *adult* who used that capacity, not the child. It was probably, again, the full set of referencing capacities mediated by the near-modern brain that made the *erectus* adaptation and dispersal possible; this evolved PVC was not merely "mimetic." It was this set of highly evolved and developing visually mediated referencing and problem-solving capacities that was selected for, both in Neanderthals and *Homo sapiens* (Eccles 1989, pp. 117–39; Jackendoff 1988; Marshack 1985; 1990; 1991a; in press).

4. This reviewer has published the most complex example of "external information storage" to come from the European Upper Paleolithic, ca. 10,000 B.C. (Fig. 1). The analysis describes the sequence of visual, cognitive, problem-solving strategies and evaluations involved in a 3½-year accumulation of a prewriting, prearithmetic "notation" (Marshack 1991c). That

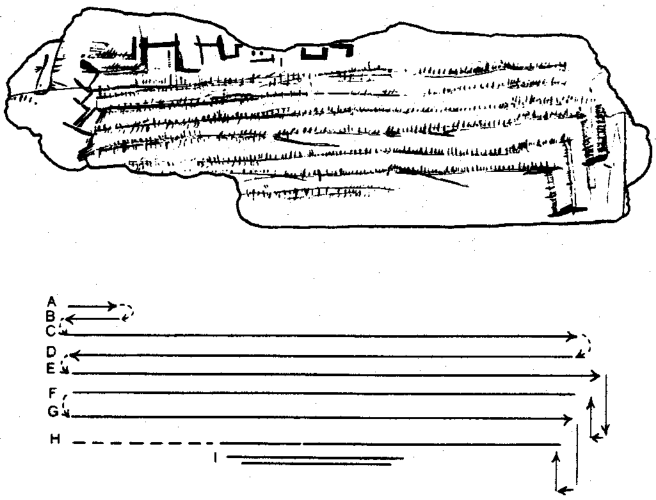

Figure 1 (Marshack). *Top:* Upper Paleolithic prewriting, prearithmetic notation for 3½ years incised on a fragment of scrap bone, from Tăi, France, terminal Magdalenian period, ca. 10,000 B.C. The accumulation documents a complex sequence of changing visual problem-solving, cueing, and abstracting strategies. The boustrophedon, serpentine accumulation conceptually creates an image of periodic time (Marshack 1991c, p. 31).

Bottom: Schematic line drawing showing the serpentine manner of accumulating marks on the Tăi plaque. The conceptual year was divided in two, with notational turns apparently occurring at the solstices. This is the most complex example of "external information storage" to come from the Upper Paleolithic.

notation represents one culmination of the hominid capacity for thinking and problem-solving in time and space. The notational research has been part of a fundamental reevaluation of Upper Paleolithic images by this reviewer, who has argued for the referential and time-factored contents of these early symbol systems and the relevance of the human visual system and of time-factoring cognitive processes for understanding the origins of both language and "external symbolic storage" (Marshack 1972; 1976; 1984; 1985; 1988a; 1988b; 1989a; 1989b; 1990; 1991a; 1991b; 1992). That inquiry into how and in what contexts these image systems were made and used, and their referential contents, is based on direct analysis of the body of Paleolithic symbolic materials. The inquiry was directed by an underlying model of mosaic evolution of the primate "potentially variable capacity," and a developing capacity for categorization, evaluation, and problem-solving in time and space – with cross-modal "marking" in (and of) the hominid-to-*Homo* referential realm. Such a mosaic hominizing trajectory probably did not proceed by Donald's encapsulated stages (Marshack 1991c and above). Donald's synthesis thus properly sets the stage for an ongoing interdisciplinary reevaluation and debate concerning evolution of the extraordinarily complex human capacity.

Apes have mimetic culture

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In his intriguing and wide-ranging book, Donald provides a problematic model of early human cognitive evolution. Although he presumes that humans "invented" symbolic language all at once, such "invention" is unlikely. Rather, like the evolution of visual symbols (Davis 1987), language was built upon transitional abilities (Miles 1991; 1993). Any extrapolation from invention in ontogeny to invention in evolution is inadequate; children normally invent symbols, but the evolutionary question is *why* humans invent symbols so easily. The evolution of vocal speech is never explained, but like self-awareness and vocal control it is presumed to come about because it was useful. Usefulness, however, does not create abilities. Although language is presumed to be an adaptation to newly evolved culture, no evidence is provided that this culture initially supported language, rather than the reverse.

The analyses of Brother John and of deaf people prior to "linguistic" enculturation are problematic, in that both people had a linguistic and cultural basis for their nonlinguistic behavior. Donald argues that because these people could not speak their behavior occurred in the absence of or independently of language. But the fact that language was inaccessible to consciousness does not mean it was inoperative. In fact, Brother John usually spoke (and thus his mind was organized linguistically), and deaf people's invented sign systems share many of the complexities of spoken language (Goldin-Meadow 1982), so that these systems are linguistic and not simply "mime." These people do not provide "glimpses . . . of the human mind without symbolic language" (p. 165). Indeed, Brother John's adventures in Switzerland remind us of our own in France, knowing little French. By contrast, people who have had no communicative experience have great difficulty participating in complex nonlinguistic interactions (Curtiss 1977). Thus, the extrapolation from Brother John and deaf people to a form of culture without language seems inadequately supported. When Donald claims, without citing evidence, that "Nonsigning deaf children . . . play essentially the same games as hearing children" (p. 174), one wonders if these children are actually "nonsigning"; if they are, how do they learn the rules of games?

Donald ignores a lot of information about cognitive abilities of apes. He argues that wild apes fail to show symbolic activity in the wild and live "entirely in the present" (p. 149); he thereby discounts evidence of planning, gestural communication, toolmaking, pretense, and teaching of tool-use by apes in nature (e.g., Boesch 1991; Goodall 1986). Contrary to Donald's insistence, apes invent signs, read others' intentions, gaze intentionally, and point spontaneously, both with and without human teaching (see references and discussion in Miles 1990; Mitchell 1993b). If Donald had actually read Crawford's study of chimpanzee cooperation, he would have found clear evidence of spontaneous gesturing: one chimpanzee solicited another's help in pulling an object by touching her on the shoulder and then illustrated what she needed: she "took her rope, braced herself ready to pull, and turned back to look at [the other chimp] as if expecting her to come up and help"; the shoulder touch soon became a conventional sign requesting assistance (Crawford 1937, p. 57). The sign-taught orangutan Chantek also invented signs based upon various types of resemblance: for example, his sign for a man who was missing a finger required Chantek to bend and touch that finger on his own hand (Miles 1991). Although Kanzi's signs almost always requested actions, most spontaneous self-initiated signs of two common chimpanzees did not, but rather named objects, described events or internal states, or described others' attributes (Miles 1976). Syntax is largely absent in ape sign-use, but some constituents of syntax occur (Gardner et al. 1989; Miles 1990).

Although Donald argues that *Homo erectus* was the first species to mime, in fact apes already have this ability, in both solo pretense and communication (the latter of which seems, contrary to Donald, psychologically more complex; see Mitchell 1993b). By contrast, labeling as "mimesis" some human activities from Eibl-Eibesfeldt's work seems inadequately supported. Although Donald argues that apes differ from humans more than linguistically, most differences Donald describes as nonlinguistic seem based on language. What would apes with language be like? A lot like humans.

Apes engage in all the things Donald says they cannot – "mime, play, games, skilled rehearsal, nonlinguistic gesticulation, toolmaking, other creative instrumental skills, many nonsymbolic expressive devices used in social control, and reproductive memory" (p. 193), as well as displaying many of the characteristics in his Table 6.1. The reason is that they have kinesthetic-visual matching, which provides apes with the "extended conscious map of the body and its patterns of action" (p. 189), which Donald presumes only humans have; it also allows for mirror-self-recognition (Mitchell 1993a). Mimetic culture is already there in apes.

Hunting memes

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Forty years ago, Kroeber (1953) remarked that

> man as a set of social phenomena, including his culture in all its aspects – along with values – not only is *in* nature, but is *wholly part* of nature. It is evidently going to be somebody's business to deal scientifically with these human phenomena; to work at more than aesthetic comprehension of them. (p. 358, emphasis in the original)

The notion that cultural change is the result of selectional evolutionary processes was proffered soon after by Murdock (1956); cultural change as cultural evolution has been a consistent theme of more recent theoretical treatments (e.g., Boyd &

Richerson 1985; Cavalli-Sforza & Feldman 1981). The bare bones of all such exercises are that cultural units, which we can call memes after Dawkins (1976), occur in variant forms; selectional processes then result in these units being differentially propagated by copying and transmission systems which move the units about in space and may conserve some of them in time. The differential survival of these units resulting from the action of such selection and transmission processes leads to changes in the frequencies of these units in the cultural pool over time; the culture shows descent with modification. In other words, cultural change is wrought by cultural evolution.

This form of analysis centres about the claim of the universality of selectional processes as the basis for many different forms of change in biological systems in time (for reviewing, see Campbell 1974; Plotkin 1993). Whether the transformations are of species, individual brains during development, immune system states or cultures, the processes are the same but embodied in wholly different mechanisms. Playing this particular game requires an abstract formulation of evolutionary processes and their instantiation in an appropriate set of mechanisms. My favoured formulation of processes comes from Hull's (1988) modification of Dawkins (1982), which posits the existence of replicators, interactors, and lineages in any system whose transformation in time is the result of the actions of evolutionary processes. But what might be the mechanisms when we are talking about cultural change? Here I take my cue from Kitcher's (1987) claim in a *BBS* article that a proper theory of cultural evolution must be rooted in a good psychological theory. I think this is exactly right. Culture is collective knowledge contained in the brains (and in the extension of our brains into paintings, books, computer discs, and the like) of all the individuals making up that culture. Changes in culture must be understood in terms of the processes and mechanisms of changing psychological states. This is one of the ways Donald's book assumes significance. A consistent theme of *Origins of the Modern Mind* is that cultures in humans and other animals (if they have such a thing at all) are to be understood "in terms of their predominant cognitive features" (p. 149) and that "a cognitive classification of culture could be built on any number of cognitive dimensions, but the most likely place to start would seem to be the area of *representational strategy*" (ibid., emphasis in the original).

There is a stark contrast between Donald's approach, which is firmly embedded in psychological theory, and that of most other theories of cultural evolution, which have been threadbare indeed when it comes to matters psychological. In the writings of other cultural evolutionists there has been a consensus on the importance of social learning as a transmission system; a small amount of attention has been paid to selectional mechanisms, usually couched in terms of social psychological constructs such as conformity; and some have noted the possible existence of internal selectional processes analogous to co-adapted gene complexes. And that is all. Social learning, however, is a complex, indirect, and unstable transmission system (Heyes & Plotkin 1989) which is little understood. It may well stand at the centre of the psychological mechanisms that drive cultural change, but not in the form in which it is currently conceived. If a transmission system is important, and it is, then one place to begin an understanding of that system is in terms of what exactly, psychologically, is being transmitted. What this means in the Hull-Dawkins replicator-interactor-lineage formulation is identifying the replicators. What exactly are the cultural memes?

Most previous analyses of cultural evolution have been based upon assumptions of unidimensional memes transmissible by social learning mechanisms appropriate to understanding the sharing of dietary preferences in rodents. What Donald offers us is a vision of the evolution of human culture that, whether correct or incorrect, complete or incomplete, is of a complexity that is intuitively credible and can serve as the beginning for understanding the psychological bases for cultural evolution. Donald gives us the possibility of meme complexes of at least three kinds, clustered within his three stages of cultural evolution – episodic, mimetic, and mythic memes with transmission devices appropriate to each form of replicator. Exactly what these memes might be, what the nature of their interactors are, and how these result in cultural lineages provides for rich theoretical and empirical pickings in the future. However the story ends, and whoever "makes it his business," will owe a great deal to Donald.

Memory, text and the Greek Revolution

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Merlin Donald, like others before him, suggests that it is the Greeks' ability to think theoretically that separated them from their predecessors and resulted in the Greek Revolution or Miracle. Donald differs, however, in making memory, not literacy, the alphabet, politics, or some other phenomenon, the cornerstone of his theory. Aristotle (Barnes 1984) would have agreed:

> Memory, experience, tact, good judgment, sagacity – each of these either arises from wisdom or accompanies it. Or possibly some of them are, as it were, subsidiary causes of wisdom (such as experience and memory), while others are, as it were, parts of it. e.g., good judgment and sagacity. (*On Virtues and Vices*, 1250a)

Like Donald, Plato (Hamilton & Cairns 1963) recognizes the division of memory into internal and external parts, but disagrees about the benefits of external symbolic storage (ESS), as Donald calls it:

> If men learn this [writing], it will implant forgetfulness in their souls; they will cease to exercise memory because they rely on that which is written, calling things to remembrance no longer from within themselves, but by means of external marks. What you have discovered is a recipe not for memory, but for reminder. (*Phaedrus*, 274e–275a)

To understand Plato's reaction to text, one has to consider the nature of text then compared to today. Donald (p. 341) explains that the Greek Revolution occurred when and where it did because "at least partly . . in the ancient Greeks, all of the essential symbolic inventions were in place for the first time. The evolution of writing was complete." Although I would agree that only comparatively minor changes occurred in the alphabet itself, the display, replication, and storage of text were still in their infancy. Here I can consider some of the issues only briefly.

Replication is, perhaps, the most obvious to us today. Everyone knows the virtues of print in distributing large numbers of exact copies of a text. What is frequently forgotten, however, is how print changed the way text was stored and how it looked. In the Athens of Plato:

> the use of jars [as document containers] . . . rather than shelves may have been more frequent than we would suppose. . . . part of the problem in consulting any document was knowing where to look. . . . It is after all comparatively easy to keep documents. It is a rather different step to use them again, find them and consult them. (Thomas 1989, pp. 80–81)

The situation in Rome was no different. Incidentally, the jars were not labeled, though sometimes tags were attached to the rolls.

Once you found the work you wanted, your real troubles began. I pass over the effects of different handwritings on the distribution of text to line and column to consider that it was not until the first century A.D. that the first codex, the ancestor of our book with "pages," appeared, even though wax tablets had been strung together since the time of Homer and could have provided a ready model. Without a concept of the "page," citations were mainly to the beginning of rolls, a particular book (our chapter) of a long work, or something like "Homer says." Pliny, in his *Natural History*, provides the great-great-grandparent of our table of contents – an invention without immediate descendants. Why? It takes an entire ancient roll, and in one English translation 71 pages of small type!

Now consider the display of Pliny's text. The roll with the contents is not only long, but it has no punctuation and no divisions within the text: none for paragraphs, sentences, or words. Think about the process of reading 71 such pages to learn which of the 36 remaining rolls contains your reference, then getting the right roll and reading through it until you come to the right spot. It took roughly from the time of Homer until the eleventh century for spaces between words to become standard and five centuries more before the paragraph was an accepted unit of division. Since the word as a unit was not isolated and handwritten pages and columns contained varying amounts of texts, most of our retrieval aids could not even be imagined. Absolute alphabetization remained a novelty as late as 1604, when Robert Cowdrey published his *Table Alphabetical* and still felt compelled to explain how to use his dictionary. In compensation, however, they did have shorthand, probably invented by a freedman of Cicero. In fact, any scholar who praises the economy of the alphabet as a better means of writing should be aware that by the end of the Middle Ages over 14,000 abbreviations existed.

As a result, an external symbolic store did not provide the great panacea, but instead forced the Greeks and Romans to improve the only method of retrieval they knew – biological memory. And that is why the teaching of memory techniques formed an essential part of ancient rhetorical training (cf. Donald, pp. 346–47). An interesting parallel occurs at the time when print first appears. Frances Yates (1966, p. xii) says: "Why, when the invention of printing seemed to have made the great Gothic artificial memories of the Middle Ages no longer necessary, was there this recrudescence of the interest in the art of memory?" So today the advent of the computer makes us reconsider human memory.

In conclusion, as it took millennia after the invention of writing for theoretic man to appear, so it took theoretic man millennia to figure out how to fully use that invention. Donald, in focusing on memory, has surely chosen one of the essential components of the Greek Revolution. A full explanation for why it was the Greeks at that time who first achieved Donald's third and final stage, theoretic culture, I believe lies in the application of the theory of complex adaptive systems. But that is another theory for another time.

NOTE

The translations are from the Bollingen Editions of Aristotle and Plato published by Princeton University Press.

Language, thought and consciousness in the modern mind

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Donald's book is one of the most provocative, well-written, and genuinely constructive works to have recently appeared in cognitive science. His main thesis – that each transition in human cognitive evolution depends on culturally mediated changes to the structure of memory – is exciting and well-argued. His proposal that the most recent transition involved the externalization of memory into nonbiological symbolic systems provides a compelling line of argument within the framework of contemporary cognitivism against the assumption that the boundaries of the human mind are coextensive with the skin of the human organism – an assumption also challenged by the cybernetic generation of cognitive scientists (Bateson 1972; 1979) and more recently by "externalists" in the philosophy of mind (Burge 1986; Davidson 1987). There are, to be sure, many differences among these scientists and philosophers, but all would agree that "the individual mind has long since ceased to be definable in a meaningful way within its confining biological membrane" (p. 359).

There are too many topics in Donald's book to discuss in this short commentary, so I shall concentrate on two that are especially relevant to philosophers engaged in cognitive science – Donald's reliance, when considering the relation between language and thought, on the fascinating case of Brother John (Lecours & Joanette 1980), and his discussion of consciousness in Chapter 9. In the first matter I think Donald is not as cautious as he should be, whereas in the second I think he is too cautious.

1. In recent years, a number of philosophers have held one or another version of the thesis that language is required for thought and consequently that nonlinguistic creatures do not have thoughts (Bennett 1988; Davidson 1975, 1982, 1984; Malcolm 1972). Donald presents one of the best cases that I have seen against this thesis. But although my sympathies are with Donald on this issue, I do not find his argument conclusive. Donald relies heavily on the case of Brother John, a paroxysmal aphasic. The case is a fascinating one because during his aphasic spells Brother John's linguistic abilities – both external and internal – appear to be completely impaired, yet he remains conscious and capable of intelligent, uniquely human, thoughtful activity. Donald says the case involves "the complete absence of language, internal or external" (p. 85), and claims that during his spells Brother John is an "archetype of the mind stripped of its words," a mind "that cannot think linguistically *at all*" (p. 253). Thus he takes the case to show that certain forms of intelligent thought do not depend on language; and he argues that it reveals a nonlinguistic layer of cognitive architecture that is vestigial from a prelinguistic phase in human cognitive evolution.

The problem, however, is that Brother John's mind-brain has already been shaped by language. It is linguistic at the phylogenetic level – he possesses the brain structures that subserve linguistic abilities – and at the ontogentic level – he has acquired language (French) during his development. As Donald notes, during his spells Brother John relies on many things he learned "via the language system"; Lecours and Joanette describe him as relying "on past experience. . . including language-mediated apprenticeships" (p. 20). It is therefore not obvious that during his spells Brother John provides an example of a nonlinguistic mind per se; rather, he seems to be an example of nonlinguistic cognition ***in a linguistic mind that has been temporarily stripped of its words***. The difference becomes important when we consider the possibility that language may affect nonlinguistic mind-brain processes at either the phylogenetic or ontogenetic levels. We therefore cannot assume that the apparent dissociation of thought and language in the Brother John case reveals a layer of cognition untouched by language. This point, combined with what Donald calls the "principle of singularity" – that each individual human brain develops its own functional organization at the cognitive level (Edelman 1987) – makes the neuropsychological quest for cognitive evolutionary vestiges far more perilous than Donald's use of the Brother John case implies.

2. In his final chapter, Donald raises the issue of consciousness, which, he says, "is *the* mainstream problem, the principal phenomenon under investigation" (p. 365). Ten years ago such an assertion would have been met with disdain by cognitive scientists, but not any more (though notice that Donald makes the remark at the end of the book, not the beginning!). Donald is

skeptical, however, about the contributions his own cognitive evolutionary approach can make to this problem. He carefully attends to some of the reasons against positing a central processor in the mind,[1] but finds it "difficult to accept" that replacing a central homunculus with disunified assemblies of agents (Minsky 1985) "is a significant improvement in the state of our metaphysics" (p. 365). Here I think Donald is not only being a little too cautious, but – to put it somewhat bluntly – actually selling himself short. For shortly after making this remark, Donald goes on to give a very nice sketch of just how consciousness in its various forms could reside in a "hybrid mental architecture" without any unified central executive (pp. 368–73). Moreover, because Donald's model of the evolution of our cognitive architecture contains not only linguistic ("symbolic"), but also "episodic" and "mimetic" layers, it is able to do greater justice to our "quite subtle and complex states of consciousness" (p. 370) than do accounts that emphasize language almost exclusively (Dennett 1969; 1991). This *is* an improvement in the state of our metaphysics and for it Donald should be commended.

NOTE

1. For other arguments against this idea, see Varela et al. (1991), Dennett (1991), and Dennett and Kinsbourne (1992).

It's imitation, not mimesis

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There is much to be admired in Donald's ambitious book, most especially his cogent arguments for the importance of the evolutionary and cultural dimensions of human cognition. Donald begins his book by pointing out that mainstream cognitive science has been peculiarly uninterested in the evolutionary origins of human cognition and the comparison of human cognition with the cognition of other animal species. This has resulted in an inappropriate reification of the "mind as computer" metaphor, with many cognitive scientists more interested in computer programs a colleague has written than in the rich variety of cognitive mechanisms on display daily in the natural world. Donald goes on to point out in his opening chapter the further curiosity that mainstream cognitive science is also for the most part not interested in culture – either as something human beings have created, as something they must adapt to, or as something that scaffolds and extends the cognitive abilities of individual human beings. The major exception is of course language, but language is mostly characterized by cognitive scientists as a bloodless representational system, with no reference to its roots in human social and cultural life (Tomasello 1992). The highlighting of evolution and culture in the study of cognition by itself makes this a valuable book.

My problem with Donald's account concerns some of his specific proposals, especially those dealing with the transition from an apelike form of culture based on episodic representation to a humanlike form of culture based on mimetic representation. Although it is inevitable that any account with a sweep this broad will leave out some important findings in specific research paradigms, in the current case I believe that Donald's lack of familiarity with current theory and research on ape (especially chimpanzee) social cognition and social learning has led him seriously astray. In his account of primate social cognition and learning he does not cite, for example, any of the papers in the volumes edited by Byrne and Whiten (1988) or Parker and Gibson (1990), the monograph of Cheney and Seyfarth (1990), or any of the work of pioneer chimpanzee researchers such as Menzel (e.g., 1971) and de Waal (e.g., 1982). This research now forms the core of thinking about nonhuman primate cognition and social cognition and how it relates to human cognition and social cognition. Donald's neglect of this literature (and almost exclusive reliance on Premack 1987) leads to a number of inaccuracies in his characterization of nonhuman primates and thus to a questionable theoretical proposal about the transition from episodic to mimetic culture.

The basis for the transition to the first humanlike culture is, in Donald's account, mimesis: the bodily reenactment of previously experienced events or episodes. Mimesis is very similar to imitation, but "adds a representational dimension to imitation" (p. 169), so that it may be used intentionally to represent both physical and social events. Thus, human beings use mimesis to engage in certain forms of dance, to communicate to others about past events, to practice social roles observed in others, to coordinate their behavior in group activities, and to teach youngsters certain kinds of skills. In all cases, imitation is a necessary but not a sufficient condition for mimesis. Mimesis reproduces a previous experiential episode, but for purposes of representation to others or to oneself.

Donald is led to this formulation, I would argue, because he believes that "Imitation is found especially in monkeys and apes" (pp. 168–69). To distinguish the human form of reproducing the behavior of others he therefore adds a "representational dimension" as the key difference. But current research does not support Donald's empirical assertion. With regard to imitation in monkeys, Visalberghi and Fragaszy (1990) report a number of studies demonstrating that monkeys do not learn new behaviors imitatively from conspecifics; and Whiten and Ham (1992), in a thorough review of all the recent literature, come to the same conclusion. Tomasello (1990) questions chimpanzee imitative learning abilities as well, although this is a bit more controversial. Although chimpanzees can in some instances reproduce outcomes that others have reproduced (e.g., in tool use), each individual does so in its own idiosyncratic way – what is called emulation learning (see Nagell et al., in press). Chimpanzees have also failed to show clear signs of imitative learning in their acquisition of communicatory gestures (Tomasello et al., in press a). Chimpanzees raised by humans in rich sociocultural contexts may have more humanlike imitative abilities (see Tomasello et al., in press b), but the question, as Donald emphasizes repeatedly, is whether in their natural habitats chimpanzees display the abilities required to create culture.

If Donald appreciated that humans differ profoundly from apes in their skills of imitation and imitative learning, he would not have to posit the added representational dimension in mimesis as the key differentiating factor. He would also not have to diminish the abilities of nonhuman primates unnecessarily in other areas. For example, there is no mention anywhere of the findings of researchers such as Cheney and Seyfarth (summarized in 1990) or of Gouzoules et al.'s (1984) finding that monkeys have systems of communication that seem to have a representational dimension. If these monkeys can also imitate, as Donald claims, then why do they not engage in mimesis? And Donald seems completely off-base when he claims that "apes in the wild do not possess even a rudimentary system of voluntary gestures or signs" (p. 126). The research in this case (summarized in Tomasello 1990) is very clear. Chimpanzees intentionally produce a number of communicative gestures that show all the signs of intentionality used by researchers studying human children. The most important of these are: (1) their gestures are accompanied by either response-waiting (indicating a specific expectation of a conspecific's response) or the alternation of gaze between goal and conspecific (indicating a knowledge that the conspecific is important in attaining the goal; Tomasello et al. 1985); and (2) across time they use the same gesture toward different ends and different gestures toward the same end, sometimes in repeated attempts in the same context, which only cease when the goal has been attained (indicating the kind of persistence and means-ends dissociation characteristic of intentional behavior of all types; Bruner 1981). These gestural signals are still not humanlike symbols, of course, but the difference is not in their use as intentional communication, nor in whether or not they serve to represent.

The point is this. Current research demonstrates that nonhuman primates show more intentionality (and possibly representational abilities) in their social behavior than Donald gives them credit for. If their imitative learning skills were as humanlike as he assumes, there would be little to differentiate humans and other primates in the cultural domain. My claim – which is in many ways simpler and more straightforward than Donald's – is that it is the imitative skills themselves that are the key differentiating factor. My further claim is that this difference is a direct function of the different social-cognitive capacities of the various primate species. The ability to learn novel behaviors or behavioral strategies by imitation of conspecifics, especially social-conventional behaviors, depends on the ability to understand their intentions (Tomasello et al. 1993). This seems to be a natural, indeed an essential, function of human cognition from at least the second year of life, but it may not be a natural function for nonhuman animal species (Cheney & Seyfarth 1990). Although it would take me far afield to make the argument in full, I also believe that the same ability is at the heart of the human ability to create and learn conventional symbols from others: the ability necessary for symbol learning and use is the ability to understand and reproduce the intentions of others when they intentionally manipulate your attention through an act of reference. The way this happens in the evolution of human language comes out clearly in Donald's own account of how human linguistic conventions are created and adopted by others in social interaction – "Users of a language vote for each new invention by adoption" (pp. 235–36) – an account that contrasts sharply with the way chimpanzee gestures are individually acquired (Tomasello et al., in press a).

Finally, I would like to register my surprise and dismay that throughout the book Donald refers to animals with terms such as "lower animals" or "advanced primates," and that he looks across animal species and notes "progress" and "cognitive gains." Although this kind of terminology may be perfectly appropriate within the evolutionary lineage of one species – where the modern form is considered more "advanced" than the earlier forms – using these terms across species suggests some lack of familiarity with evolutionary thinking.

Can a Saussurian ape be endowed with episodic memory only?

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Donald's brilliant book offers a provocative scenario for capturing the main evolutionary aspects of the culture and cognition leading from apes to modern *Homo sapiens*. Because we are comparative psychologists and primatologists we will not address the entire theory put forward by the author, focusing instead on the characterization of the "ape stage." According to the taxonomy proposed, this stage takes the form of an "episodic culture," namely, unreflective, concrete, and situation-bound representations (p. 149). This depiction of ape and nonhuman primate cognition sometimes seems too narrow to cover the cognitive skills of these animals and sometimes too generous when some forms of "Saussurian-style signs and signifiers" (p. 160) are uncritically attributed to these species. These two points will be considered in turn.

1. The nonhuman primate representational system is more than episodic. In Donald's terminology, the lives of nonhuman primates are "lived entirely in the present" (p. 149). This amounts to accepting that primate species use a basically perceptual memory that retains only episodes, that is, events within a specific time-space locus (p. 150). This approach, though it has the advantage of simplicity in relying on a single explanatory schema, is unable to encompass a number of cognitive capacities that have been found in nonhuman primates as well as other animal species (e.g., birds). We would like to single out some of these phenomena, such as the ability to form concepts or the capacity to build amodal representations across different sensory modalities ("crossmodal integration").

Several studies (see Roitblat 1987 for a review) have demonstrated that different animal species can indeed store memories of unitary events, for example, in the visual and auditory domains. Some species (pigeons, Herrnstein et al. 1976; monkeys, Schrier et al. 1984), however, can readily integrate visual percepts into broader entities. Even though the basis of this ability has not yet been clarified (Roberts & Mazmanian 1988), these experiments have shown that these species can conceptualize at a level beyond the immediate event (e.g., the capacity shown by laboratory pigeons to classify pictures into two categories, "trees" and "non-trees," Herrnstein et al. 1976). Crossmodal integration is the capacity to recognize stimuli-objects with one sense modality when they have previously been experienced through another. A fairly large body of data shows that monkeys and apes transfer information from, say, visual to haptic modes and vice versa (Davenport & Rogers 1970; Malone et al. 1980). Moreover, chimpanzees who have received symbol training are able to translate that information from a representational input into the relevant perceptual features of the output object (Savage-Rumbaugh et al. 1988). A similar ability underlies transitive inference, in which the knowledge of relationships between items is combined to infer novel relationships. Such a capacity has been exhibited by pigeons (Von Fersen et al. 1991), rats (Davis 1992), monkeys (McGonigle & Chalmers 1977), and apes (Gillan 1981). The available data thus strongly suggest that the nonhuman primate processes information at a level quite beyond that of perceptual episodes.

2. The nature of apes' signs. Donald writes that apes "are able to use symbols, in the critical sense that they can use them as substitutes for their referents" (p. 160), and that "they are obviously able to learn to use Saussurian-style signs and signifiers" (p. 160; see also p. 218). We think it worth critically evaluating such statements with respect to the capacities exhibited by apes trained in sign language (e.g., Gardner & Gardner 1969). The reference to De Saussure is relevant to the real nature of the apes' productions: according to De Saussure (1916), a linguistic sign is more than a relation between a thing and a symbol. Such signs express, at the individual level, a relation between two mental images, one of the sound (acoustical image) and one of the content (conceptual image). The process of designation requires, at the level of society, the selection, by shared convention, of a particular sequence of sounds to stand for a particular concept. It is this two-level activity that characterizes the creation of a Saussurian sign. Clearly, such a chain of individual and conventional processing has never been exhibited by apes or other animals (Vauclair 1990; 1992; Wallman 1992).

On several occasions (e.g., p. 159), Donald refers to the chimpanzee as the "pinnacle" of the episodic mind. This suggests two observations: first, most higher cognitive functions such as "language" skills have simply not been investigated in primate groups other than apes. Second, the failure to demonstrate a given capacity might simply reflect the use of an inappropriate method. For example, in a recent study using computerized techniques, we (Hopkins et al. 1993) were able to show that baboons can discriminate mirror images, an ability reputed to be difficult even for apes.

In conclusion, the three-stage scale in the evolution and cognition proposed by Donald is indeed appealing in its plausibility and its consideration of the intermediate steps leading to the highest cognitive capacities. Nevertheless, the cognitive processes so far identified in nonhuman primates and in other animals are not so easily reducible to a single conceptual framework. It might thus be useful in further comparisons to conceive of cognitive functions and their evolution not only in terms of their integration into progressive internal and external memory systems but to view animal adaptations as unique on their own terms, rather than relay stations on the road to man. This might better accommodate both animals' humanlike cognitive processing in perception and memory and their limitations when it comes to the unique features of linguistic signs.

Stages versus continuity

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Donald's book is a very readable and interesting exploration of one of the great evolutionary puzzles, the emergence of that remarkably and (at first sight) unnecessarily large and clever organ, the human brain. At no point does Donald fall into the jargon that afflicts the field, and his attempts to wrestle with such ineffabilities as consciousness are founded sensibly on concrete observation. He rarely attempts to go further than the data can take him.

There is, however, a conflict that runs through the book, one that is sometimes explicitly addressed and sometimes shunted aside. It has to do with a fundamental dichotomy in evolutionary thought – whether evolution proceeds in the gradualistic way that Darwin originally envisioned, or through a series of rapid and dramatic changes. Although there is ample evidence for sudden punctuations in many evolutionary lineages, such evidence is sparse in our own ancestry and becoming sparser as more information becomes available. And even in lineages in which sudden punctuations do occur, it tends to be forgotten by paleontologists and evolutionary geneticists alike that such rapid changes can only take place if most of the genetic variation needed to fuel them is already present in the population. It is well to remember that in a genetically polymorphic species such as our own, the reshuffling of preexisting variation and shifts in the frequencies of preexisting alleles are likely to have a much larger impact on the gene pool during periods of rapid environmental change than will the appearance of and selection for advantageous new alleles.

Donald envisions a series of transitions in the evolution of the mind, from primate "episodic" culture through mimetic, mythic, and finally theoretic culture. For some of these transitions, he presents evidence both from the fossil record and from what can be inferred about the cultures of our hominid and more distant ancestors. His categorization of these stages, particularly his emphasis on the important role of mimesis, is a very valuable and clarifying exercise. However, though the process of grouping things into categories can be useful, it becomes less useful when it masks the nature of the underlying processes. The question is: Have such transitions been the result of specific short-term events, consisting of either genetic or cultural alterations in our past, or were the biological and cultural changes that have taken place in our minds the result of more continuous processes?

Attempts to tie episodic advances in our ancestors' culture to the fossil record are uncertain at best. On p. 162, Donald suggests that the later australopithecines might have somehow become more advanced in their brain development even though they retained a very primitive toolmaking culture. Possibly, the appearance of *H. erectus* some 1.6 million years ago (Brown et al. 1985), with the roughly coincident appearance of more advanced Acheulian tools, might mark a watershed event. Yet the distinction is blurring as more evidence comes in. At Konso-Gardula in southern Ethiopia, numerous advanced Acheulian tools have recently been dated at 1.9 million years ago, well before the earliest known *H. erectus* (Asfaw et al. 1992). On the other hand, the earliest *H. erectus* inhabitants of Europe of a million years ago had very primitive tools (Rightmire 1991). The *H. erectus* of Asia, living at least a million and a half years after the first appearance of this group in Africa, seem to have had remarkably little toolmaking capability and (aside from the use of fire) little evidence of a complex culture (Lanpo & Weiwen 1990).

This blurring of the correlation between hominid fossils and the artifacts associated with them is also apparent much closer to the present. Neanderthals, once relegated to the status of distant and rather dim relatives of ours, seem (at least during the later stages of their occupation of Europe) to have acquired advanced Old Stone Age technology (Mercier et al. 1991). And even the ability to speak, once assumed to be entirely the province of our most recent ancestors, may have been pushed back in time by the discovery of a Neanderthal hyoid bone indistinguishable from our own (Arensburg et al. 1989). Though this conclusion of evolutionary continuity will still be fiercely contested in some quarters (Eldredge & Tattersall 1982), as more evidence accumulates the demarking of our history into clear-cut stages seems to be at least as much a function of gaps in our own knowledge as it is of the actual existence of clear stages into which our ancestors can be classified.

Donald is careful to point out that many of his transitions must involve very little genetic change – the acquisition of the ability to read must surely be primarily a cultural one (though there is growing evidence that there is a genetic component to certain aspects of that ability; Smith et al. 1990). One of the remarkable things about the evolution of the mind is that a brain's ability can often far outstrip the physiological and cultural boundaries imposed on it by circumstances. For example, chimpanzees, particularly bonobo or pygmy chimpanzees, can understand a remarkable variety of sentences even though they cannot easily reproduce them (Savage-Rumbaugh et al. 1993). Donald gives a number of examples of cases of such outstripping, and others abound. He points out (p. 254) that among humans, "In a multilingual polymath . . . one could have, in one brain, a dozen lexicons or more . . . with different rule systems governing their use." Such lexicons can only be learned if there is access to a great variety of knowledge from many different cultures, something that has only happened recently in human history. But the brain is up to such a remarkable task because it has, as I have pointed out elsewhere, evolved to become a sponge for knowledge (Wills 1993). Its growing capacity has undoubtedly been the driving force behind the evolution of many of the recent physiological and anatomical changes in the rest of our bodies, and behind the rapid recent elaboration of our culture. And these changes have acted in a feedback loop to accelerate the evolution of the brain. It is this runaway process that has, I suspect, given our recent evolution such apparent continuity.

It will be fascinating to see the extent to which the potential reach of the minds of other organisms exceeds their grasp. Possible experiments suggest themselves. Although it is not quite technically possible to do the following experiment, it soon should be. Suppose a clever and noninvasive device were attached to the neck of a bonobo chimpanzee, a device which would allow small vibrations and changes in the position of the larynx to be translated into a humanlike range of sounds far beyond the chimpanzee's current repertoire. The chimpanzee would soon learn to produce those sounds with little effort. How quickly would it learn to speak, and what would it say? Although it is unlikely that anything truly profound would emerge from the bonobo's speaking device, the results might still surprise and humble us. Donald's stages in the evolution of the mind, useful anchors though they may be to our thinking, should not be allowed to obscure the remarkable underlying evolutionary processes that have taken place.

Archaeological evidence for mimetic mind and culture

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Origins of the Modern Mind is a brilliant synthesis. Marshalling the evidence from diverse fields, Merlin Donald has given us a set of rich hypotheses for the stages of human cognitive evolution. While it would be possible to write lengthy comments on each of his proposed stages, I will concentrate on mimetic mind and culture. *Homo erectus* occupied a unique position in human evolution. The earlier australopithecines were, arguably, bipedal apes who adapted to life on tropical savannas. The later archaic *Homo sapiens* were, also arguably, early examples of true humans. But *Homo erectus* was neither ape nor human. Its brain size falls between the ranges of ape and human brains, and its brain shape appears to have been equally intermediate. Archaeological evidence of *Homo erectus* presents several enigmas, not the least of which is the incredibly slow rate of technological change. *Homo erectus* appears to have occupied the key link between our apelike ancestors and ourselves, making its understanding an essential element to any discussion of human evolution.

Donald presents an original and truly fascinating hypothesis for the culture of *Homo erectus*. He proposes an intermediate stage of mind and culture that falls logically between the episodic mind/culture of apes and the symbol based culture of modern humans. Mimetic mind and culture is characterized by conscious, self-initiated, representational acts that are intentional but not linguistic and would include re-enactment of events, reciprocal games, group mimetic acts, prosody, and other referential but nonsemantic skills. Donald supports his hypothesis with three kinds of evidence. The first is interpolation. What should an intermediate condition be like, based on our understanding of modern apes and humans? The second and most compelling is the evidence of vestiges; illiterate deaf-mutes, prelinguistic children, and pathological cases like that of Brother John all indicate that representation and communication are possible without language. As a third kind of evidence, Donald invokes the archaeological record. There are important reasons for doing so. Mimetic culture is the only one of Donald's proposed stages for which there are no modern examples. Apes have episodic culture and modern humans have mythic culture and theoretic culture, but no living community has mimetic culture. Although it is a reasonable hypothesis based on interpolation and cognitive vestiges, it cannot be directly observed. This is where archaeology comes in. Even though the archaeological record is extremely impoverished, it is a direct record of past behavior and can be used to check hypotheses about prehistoric minds. It is Donald's use of the archaeological evidence that I would like to examine further.

The archaeological evidence cited by Donald is not entirely accurate. Long distance migration, use of fire, and seasonal and cooperative hunting are all incorporated into Donald's characterization of *Homo erectus* culture, yet the archaeological record provides convincing evidence for none of these until very late in the time range of *Homo erectus*; indeed, so late that the hominid responsible was probably early *Homo sapiens* (Donald runs into similar problems in his documentation of prehistoric mythic culture). To be fair, Donald cannot be faulted for including such archaeological evidence. Such descriptions do appear in many of the just-so stories common in the archaeological literature and it has not been until recently that the work of such archaeologists as Lewis Binford (1985), Philip Chase (1991), Iain Davidson (1991), and Harold Dibble (Chase & Dibble 1987), has forced archaeology to take a more critical approach to the available data. The current picture of *Homo erectus* archaeology is not as rich as that supplied by Donald and this does have implications for his interpretation. Evidence for hunting is minimal, for example, and there is certainly no evidence for the cooperative hunting and game drives that would have used mimetic skills.

In other respects, however, the archaeological record is perhaps more helpful than Donald appreciates. In several places he states that sophisticated and systematic tool-making would require mimetic skill, but he does not detail why. Let me try to build on this assertion by checking what archaeologists know about *Homo erectus* tools against the features of mimetic mind and culture provided by Donald (intentionality, generativity, and so on). The target of my discussion is the biface, an artifact apparently invented by early *Homo erectus*. This is a relatively large stone tool, often extensively modified, that has an almond-like or pointed oval shape.

Two features of the biface are especially informative, its shape and its standardization. A biface is often bilaterally symmetrical, and this symmetry was almost certainly intended by the maker. Imposed bilateral symmetry is not known for ape tools and is not a quality of ape "art" produced in captivity, though compositional balance apparently is (Boysen et al. 1987). Cognitively, such rudimentary symmetry (reversal without congruency) is more complex than the topological qualities required for ape and Oldowan tools and, in Piagetian terms, requires greater "decentration" of thought or (to put it another way) spatial abilities that are less egocentric (Wynn 1989). *Homo erectus* toolmakers clearly shared an idea of appropriate shape; many, if not all, could make a biface. Sharing an idea about imposed shape is certainly well beyond anything apes do and has important implications for cognition. Conceptually, toolmakers do not just need to coordinate the motor sequences of toolmaking and an idea of final product, they must also have considered what other *Homo erectus* knew and considered appropriate. This required a sophisticated ability to construct an idea of not just what another saw but what that other understood (Wynn, in press). Learning to make a biface would not have required language. Observation, modelling, repetition, and the kind of intersubjectivity discussed recently by Tomasello et al. (1993) in this journal would have been sufficient, just as they are sufficient for the learning of most modern tool use through apprenticeship.

Several of the characteristics of mimetic culture can be recognized in the preceding discussion of biface technology. Intentionality and autocueing were required for the production of symmetrical shapes; and communicativity, in the guise of public acts, is implied in the repetition of standard form. In addition, at a more basic cognitive level, *Homo erectus* used spatial concepts that were less egocentric than those of apes and could construct some understanding of another individual's perspective. These are all in keeping with Donald's characterization of mimetic mind and culture. What is missing from the archaeological record is any evidence for representation, which is, of course, the key ingredient to mimetic culture. Nevertheless, because we cannot ever realistically expect to find such evidence, I think it fair to conclude that the archaeological record supports Donald's hypothesis for the culture of *Homo erectus*. If we then enrich this framework with the evidence of cognitive vestiges, as Donald does, the result is the most detailed and convincing picture of the mind and culture of *Homo erectus* yet proposed.

External representation: An issue for cognition

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In *Origins of the Modern Mind*, Merlin Donald has offered a provocative, compelling, and radically different view of cognition. It was a great pleasure to follow his convincing arguments on the evolution of the modern mind. I entirely agree on the nonbiological (external, social, cultural, and artificial) nature of cognition. In the first part of this commentary, I elaborate on some of Donald's ideas of external representations. My disagreement with him, which is minor, is on some specific properties of external representations. The second part describes the disagreement.

External representations are an indispensable part of cognition. According to Donald, the essence of cognitive evolution is the emergence of new representational systems that has taken place at three transitional stages. The first transition was from the episodic to the mimetic culture, marked by the emergence of the most basic representation – the ability to mime events. The second transition was from the mimetic to the mythic culture, marked by the emergence of the human speech system that is capable of representing narratives. The third transition was from mythic to theoretic culture, marked by the emergence of visuosymbolic representations and external memory storage. The changes in the first two transitions were in the internal biological hardware, whereas those in the third transition were in the external technological hardware. Donald's radical departure from the traditional view of cognition concerns mainly the third transition: the changes in cognitive architecture mediated by external memory technology were no less fundamental than those mediated by biological changes in the brain. The external symbolic storage is the most important representational system, responsible for much of the virtually unlimited cognitive capacity of the modern mind. And the modern human mind can be considered a mosaic structure of the biologically based representational systems that emerged during the early transitions and the external symbolic devices that emerged during the most recent transition.

Though I have some reservations about Donald's demarcation of the three transition stages, I entirely agree that external representations have not only played crucial roles in the evolution of the modern mind, but are also an indispensable part of cognition. People perform in an information rich environment filled with natural and artificial objects extended across space and time, surrounded by other people, and grounded in complex cultural and social structures. A variety of cognitive tasks, whether in everyday activity, scientific practice, or professional life, require the processing of information distributed across the internal mind and the external environment. Internal representations cannot be the whole story of cognition. The traditional approach to cognition, however, often assumes that cognition is exclusively the activity of the internal mind. External representations, if they have anything to do with cognition at all, are at most peripheral aids.

There is no doubt that internal representations are important for cognition. However, without taking external representations into consideration, one must sometimes postulate nonexistent internal representations to account for structure in behavior, much of which is merely a reflection of the structure in the environment (e.g., Kirlik 1989; Simon 1981; Suchman 1987). Thus, to study cognition, especially high-level cognitive phenomena, we need to consider internal and external representations as an integrated representational system – a system of distributed representations. Recently, the role of the environment in cognition has become the central concern in several areas of cognitive science. For example, according to the "situated cognition" approach, the activities of individuals are situated in their social and physical environment, and knowledge is considered a relation between the individuals and the situation (e.g., Barwise & Perry 1983; Greeno 1989; Lewis 1991; Suchman 1987). For the distributed cognition approach, cognition is distributed across internal human minds, external cognitive artifacts, groups of people, and space and time, and it is the interwoven processing of internal and external information that generates much of a person's intelligent behavior (e.g., Hutchins 1990; in preparation; Norman 1988; 1991; 1993; Zhang 1992). External representations, then, are an indispensable part of cognition and must be treated seriously in its study.

External memory is only one of the aspects of external representations. Donald defines external memory as "the *exact* external analog of internal, or biological memory, namely, a storage and retrieval system that allows humans to accumulate experience and knowledge" (p. 309). Though this is a useful functional definition, it does not capture the distributed nature of external memory. For example, a written Arabic numeral is not simply an exact external analog of the internal representation. It is a distributed representation: the arbitrary shapes of the symbols and their values must be memorized (internal information), and the spatial relations of the symbols are available in the environment (external information). In this case, it is the integration of internal and external representations that characterizes the nature of external memory (Zhang & Norman 1993).

In his discussion of external representations, Donald focuses on external symbolic storage (mainly on writing systems) but says little about other types of external representations, especially those cognitive artifacts (Norman 1991) that people use in everyday life to aid and organize their cognitive activities (e.g., calculating, navigational, and communicative devices). External representations not only represent information; they also constrain, anchor, structure, and change people's cognitive behavior. In addition, external representations can be nonsymbolic as well as symbolic, that is, they can provide information that can be directly perceived and used without being interpreted and formulated explicitly (e.g., Gibson 1979).

Another aspect of cognition that deserves more discussion is the dynamic, interactive nature of human activity. Human beings are not only the product of the environment, but also active agents in creating the environment. For example, the sociohistorical approach to cognition argues that it is the continuous internalization of the information and structure in the environment and the externalization of the representations in the mind that produce high level psychological functions (e.g., Vygotsgy 1978; 1986).

Author's Response

On the evolution of representational capacities

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I hope I can do justice to the rich lode of ideas in this collection of commentaries. I am pleased that no commentator has questioned the inherent value of trying to assemble this cross-disciplinary perspective; I assume this is because they felt the exercise has been basically worthwhile. Certainly my own view of human cognitive neuroscience will never be the same again. Coverage has been thorough; most of my proposals have been placed under the reviewing microscope. I have clustered my responses to the various commentaries under the major headings.

R1. The fossil record. I tried to tiptoe through the various war zones of archaeology to avoid being drawn into disputes that I felt were not central to my objectives as a psychologist. Evidently this high-wire act was not entirely successful; various generals working down in the fossil battleground have replied "Not so fast." **Brace, Chase, Dunbar, Halverson, Marshack, Wills,** and **Wynn** have all raised significant questions about the fossil database itself, as well as my interpretation of it. Chase in particular has presented a very strong case for caution in linking cultural artifacts with specific species of hominids, and indeed there does appear to be a great deal of slippage in this regard. I have always accepted this limitation in principle, and in fact went so far as to point out that my specific hypotheses about the capacities of *Homo erectus* and archaic *Homo sapiens* were likely to be the least durable of my proposals, since our view of these species is changing so fast. Certainly any current dating of the "transition periods" to which I refer must be taken with a grain of salt; this is especially true of the second transition, in view of raging disputes over replacement theory, the "Eve" theory, the competencies of late *erectus*, the place of Neanderthals in the line of descent, and so on.

Nevertheless, I don't think this caveat does serious damage to my theory of succession, which is rooted more in cognitive considerations – and in the logic of exaptation and preadaptation – than in the fossil record. It is not critical from a cognitive vantage point (although it obviously matters very much from an archaeological one) whether mimetic skill started to emerge in the late australopithecines, or appeared in germinal form in Miocene apes, as opposed to starting much later in *Homo habilis* or *Homo erectus*, provided that my proposed order of succession is not broken. Preadaptation is central to this logic: episodic capacity is a precondition for evolving a capacity for mimetic representations, since the complex motor models of the latter are generated (and evaluated) in linkage with event-perceptual models; and mimetic capacity is a precondition for evolving a capacity for lexical invention, because the morphological forms underlying lexical entries are ultimately assembled from retrievable mimetic schemata. In effect, the proposed succession is determined by an architectural proposal; to violate my specified sequence would require a different notion of cognitive architecture, and a different proposal about the way modern humans build their representations.

Serious slippages between the timelines of cultural and anatomical change might provide some problem, however, for my choice of a stage-type theory, as opposed to the gradualistic, mosaic theories favored by **Marshack** and **Wills,** among others. If it proves to be the case that cultural differences between habilines and early *Homo erectus* are minimal, and that the culture of *Homo erectus* blended very gradually into archaic sapient culture, then a gradualistic solution might prove more satisfactory. My proposed order of succession might still hold up in such a gradualistic theory; and in fact the gradualists are virtually certain to be right at least some of time, as I admitted in my book. But for the moment I still prefer to retain elements of a stage-type theory, since it fits the fossil record better than any other, but only time (i.e., better evidence) will tell.

I would like to reemphasize, as I did in my book, that my proposals do not in any way attribute teleology or purpose to evolution. **Cynx & Clark, Tomasello,** and **Jerison** raised the question of my implicit "*scala naturae*" and my covert Aristotelian/Piagetian progressivist tendencies. But I did not at any point intend to imply the existence of a progressive *scala naturae*. In making reference to "higher" and "lower" species, I was working mostly within the primate line, and the terms are probably appropriate there. But if I used those value-laden terms elsewhere, in an inappropriate context, *mea culpa;* heaven forfend that I should imply that humans are even slightly superior to beavers, budgie birds, or tree frogs; we are just, well, different. There is no need for value judgments here; but I insist that ethologists and comparative psychologists must realistically confront the richness of human cultures, and not underestimate the gulf between human cultures and those of other species. Teleology does not come into it, however.

R2. Linkages to other species. I chose to focus on the human case, and on our closest relatives, but several commentators (**Cynx & Clark, Jerison**) would have liked me to extend my proposals in the direction of a more general theory of cognitive evolution, and to integrate sociobiological theory into my scenario (or vice versa). There are at least two reasons why I did not. One was a practical reason, namely, the length of the manuscript and the limits of what might be achievable in that regard; I cut out a section that tried, among other things, to come to terms with some of Lumsden and Wilson's (1981; see also multiple book review of "*Genes, Mind, and Culture,*" *BBS* 5(1) 1982) sociobiological ideas. And the other reason was caution; extending the corpus of sociobiological theory to the human case would be an extremely speculative business (see especially Kitcher 1985; see also multiple book review of "*Vaulting Ambition,*" *BBS* 10(1) 1987), made all the more so for reasons outlined in the last chapter of my book, where I discussed the problems inherent in extrapolating any animal (episodic) model of cognition to humans. In a sense, one could say that the solutions emerging from my cognition-centered approach simply couldn't have emerged from the sociobiological literature. For example, I believe I have proposed a quite credible *nongenetic* solution to the problem of accelerated cognitive change during the recent past. Of course, this does not rule out the possibility, indeed the likelihood, of a synthesis between these two very different approaches at some point in the future.

I have been accused of mammalocentrism (**Cynx & Clark**) and anthropocentrism (Cynx & Clark, **Deregowski**); but surely these biases are inevitable in any book aimed exclusively at the special case of human cognitive evolution. Perhaps my anthropocentrism is also rooted in my early immersion in human culture as an undergraduate in philosophy; and I have never ceased to be amazed at the airy confidence with which some biologists are willing to equate, say, birdsong dialects with human language dialects [cf. Bickerton: "The Language Bioprogram Hypothesis" *BBS* 7(2) 1984; Johnston: "Developmental Explanation and the Ontogeny of Birdsong" *BBS* 11(4) 1988; Baker: "The Biology of Bird-Song Dialects" *BBS* 8(1) 1985]; or the mating behavior of fish and the courtship rituals of humans. Such grand, sweeping comparisons can only be taken seriously if we isolate these phenomena from the rich cognitive and cultural detail presented by the human species, an approach that these same investigators would probably reject in studying other species. I will grant that I am biased. But ethologists have to be on guard against the opposite of the anthropocentric bias; that is, a tendency to underestimate and even trivialize the enormity of the evolutionary gap humans have crossed, and the cognitive complexities that characterize human cultures. To return to Cynx & Clark's little allegory (ethologists are clever foxes, psychologists are tunnel-vision hedgehogs, and we never get to hear the end of the story): perhaps it is the ethologists who are really the hedgehogs, armed with one simple set of descriptors (not to mention rather limited allegory-construction skills). Cynx & Clark are, after all, just two more humans, embedded in their subculture, trying to construct a few moderately original representations, like the rest of us. There is no truly "objective" solution, no Olympian perspective outside of human culture.

Regarding the specific issue of avian cognition (**Cynx & Clark**), I accept the evidence that birds have something analogous to mammalian episodic memory storage; in fact I was greatly influenced by Sherry and Schacter's (1987) review of memory evolution, which relied heavily on avian data, and like them, I do not restrict this label to primates or even to mammals. I agree that the final word has not been written on the avian memory data; in fact, the theoretical situation is even more complex than Cynx & Clark have suggested, because the term "episodic" has acquired an unfortunate association with consciousness and voluntary retrieval. I would argue that mammals, and possibly birds, have varying degrees of episodic *storage*, but lack the voluntary *retrieval* (or "recall") skills of humans, which are a direct result of our newly evolved representational architecture.

R3. The episodic-mimetic boundary. No one has questioned the basic notion of episodic cognition as a governing mode, but one major complaint with my presentation was that I have underestimated the capabilities of apes. **Jerison, Mitchell & Miles, Marshack, Cynx & Clark, Tomasello,** and **Vauclair & Fagot** have all claimed this, in different ways. Unfortunately, many of the points I made in Chapter 3 (Darwin's Thesis) seem to have been lost on these reviewers, who understandably focused on Chapter 5 (Episodic Culture). In the former chapter I explicitly agreed with Darwin's observation that apes are capable of deception; and I also accepted Goodall's and **Dunbar**'s observation that they have complex social event-sensitivities, and Köhler's view that they are capable of very complex instrumental event-perceptions. It seems that these admissions were not enough for some researchers, who understandably wanted more of the recent literature to be cited, in particular Cheney and Seyfarth's (1990; see also multiple review of "*How Monkeys See the World*" *BBS* 15(1) 1992) work, and the large literature on tactical deception and theories of mind [see Whiten & Byrne: "Tactical Deception in Primates" *BBS* 11(2) 1988; Gopnik: "How We Know our Minds" and Goldman: "The Psychology of Folk Psychology" *BBS* 16(1) 1993]. Since this recent work is undoubtedly seminal research, I regret having overlooked its importance.

But do any of these new observations force me to change my basic proposals? On the contrary, I think these findings greatly strengthen my theory of succession rather than weakening it, since they serve to smooth out the rough edges of the first transition. I may have overstated the case as far as the gap between ape communicative skills and mimetic culture is concerned. I found **Marshack**'s commentary especially persuasive; his example of chimpanzee mother-infant interaction in the transmission of tool-making skill suggests the germs of both intentional gesticulation and autocuing, in a natural social context. Even allowing for the possibility that the authors (Boesch & Boesch-Ackermann 1991) were overinterpreting the mother's actions to some degree (their explanation might lie as much in the infant's imitative skills as in maternal "pedagogy"), these interactions appear to be "protomimetic" at the very least. They are evidently not enough, however, to create a larger set of shared cultural representations.

We might argue endlessly about how to interpret a single isolated gesture-like act; but surely the acid test for a species-wide representational skill is its *general adoption in culture*. If apes were sufficiently endowed with a capacity for mimetic invention they should quickly form a community pattern of invented gestural and mimetic signals, and the resulting expressive customs would gradually become highly variable across different social groups. This does not happen; evidently apes do not read as much into one another's "gestures" as do their human keepers. I think the Hayeses (1951) and the Kelloggs (1933) got it right long ago; they rejected the notion of intentional gesturing in their subjects. I concede that some of the elements of mimetic skill might be present in the social communications of modern apes, and thus might conceivably have been present in Miocene apes; this capacity evidently never reached the point where it led to a shared representational culture. This strengthens the argument that there was a credible basis in primate cognition from which generalized mimetic capacity might have evolved, and adds weight to arguments that this capacity could have emerged somewhat earlier in the hominid line than I have suggested.

I see no evidence supporting the stronger claim, made by **Mitchell & Miles**, that there is a fully developed mimetic capacity in modern apes. Where is this supposed capacity for mimetic invention, and the accompanying evidence of mimetic culture? Ape culture lacks virtually all of the behavioral elements of my definition. They do not systematically invent, rehearse, refine, and transmit movement patterns. They lack the ability to improve their praxic skills in a culturally cumulative manner; they do not systematically model or reenact significant events in their motor acts as a way of communicating with each other; and their gestural signaling appears to be limited and largely stereotyped. With regard to the latter point, even Cheney and Seyfarth's (1990) studies of vervet vocalization fail to suggest any significant expansion of ape signaling capacity into the mimetic realm. Again we might emphasize the general lack of cultural variation within species: vervets have a different vocal repertoire from other apes, but the total number of signals they produce is still very small and stereotyped. Moreover, Mitchell & Miles significantly misread a basic premise of my theory: they missed the notion that sophisticated symbol use is based on a series of changes in basic cognitive *capacity;* they seem to think I simply extrapolated from ontogeny to phylogeny. Quite the contrary: when I speak of symbolic "invention" I am referring first and foremost to the evolution of a *capacity* that underlies the ontogenesis of symbol use in any individual.

Tomasello challenged my theory from another direction, suggesting that a "capacity for imitation" alone might have provided the link from "episodic" cognition to language. I gather he thinks (wrongly) that I put the imitative horse before the event-perceptual cart. If I understand him correctly, Tomasello proposed that apes already have such highly developed social event-perceptions and intentional gestural capacity that the addition of what he calls "imitative ability" would produce a human-like culture. I certainly agree with him on the first point: apes (and some other mammals) have very highly developed social event-perceptions; I have said this over and over. Episodic competence in apes is integral to my theory of the first transition. But on the second point I disagree with him. The nub of our disagreement, or misunderstanding, seems to be in the definition of mimesis. Mimesis is, by my definition at least, both a replicative motor skill, and an inventive capacity. It must have emerged from existing primate imitative ability by mapping action onto a more generalized event-perceptual apparatus, much the way Tomasello's putative imitative mechanism presumably would. But it does much more than replicate; implemented in groups, mimetic capacity will play, invent, and create a new communicative environment. In principle, mimesis operates by perceptual metaphor; this is both a strength and a limitation. [See also Tomasello et al. "Cultural Learning" *BBS* 16(3) 1993.]

As broadly as I may have defined it, mimesis seems to encompass only a small part of Tomasello's very broad idea of "imitation," which apparently extends even to imitation of language learning and symbol use. His concept of imitation implies a modeling ability so broad that it reminds me of Goldstein's (1948) notion of "abstract attitude." I don't question the fact that humans can "imitate" one another's representations; but to misquote Gertrude Stein, an imitation is not an imitation is not an imitation. In my framework, imitating Cicero's rhetorical structure or the "logic" of Chinese writing would require a set of cognitive skills very different from those for imitating diving techniques or expressions of grief. I have tried to identify more specific underlying skill structures, and I have limited mimetic skill per se to modeling acts that are governed by the principle of action-metaphor.

One common error running through many commentaries is the assumption that if apes can be shown to *understand* some feature of the social environment, they must have the essential feature of intentionality I specified as central to mimetic culture. There are many demonstrations of the social sensitivities of apes: for example, in the tactical deception literature (Whiten 1991; see also Whiten & Byrne: "Tactical Deception in Primates" 11(2) 1988). Some researchers attribute "theories of mind" to apes on the basis of their ability to intuit what conspecifics (and sometimes human trainers) want or intend, and act accordingly. I do not like the term "theory of mind" in this context: tactical deception only implies a capacity to predict the behavior of others from a few salient cues, and these are strictly event-perceptual skills: *they do not necessarily imply any capacity at the output end of the system to represent that knowledge.* What I am referring to is the latter kind of intentionality – the ability to capture and express meaning in action.

Vauclair & Fagot have a different set of concerns: they point out the presence of abstract cognitive abilities – concept-formation, cross-modal integration, cross-modal transfer, and transitive inference in various animals, including not only apes and mammals but also birds. They ask, does this not show that these animals go "beyond the episode"? I reply that these demonstrations in no way suggest going beyond my definition of episodic cognition. Episodic cognition can be quite abstract; for example, perceiving and "parsing" a complex social event – identifying the agents, actions, and relevant objects and evaluating their significance – may require all of the abstract skills listed by Vauclair & Fagot and some that are even more abstract. Imagine the complexity involved in the following scenario: a lion surveys a herd of gazelles, singling out its candidate for a kill, and evaluates whether it can run fast enough to catch it. No one would claim that this is a simple task, yet this kind of event-complexity is the stock-in-trade of most mammals. This complexity is nevertheless restricted to the episodic level; the animal cannot re-present or "replay" the event to itself; it might recognize a later event as similar, but it cannot reflect on the event, since it does not have a retrievable representation of it.

Vauclair & Fagot also question my assertion that apes can use "Saussurian" symbols, pointing out that Saussure required not only the internal association of signifier and signified (which Washoe and Sarah, among others, certainly achieved) but also the social convention by which they could be understood. Although I am the first to agree that apes have never achieved this second plateau as a society, I would claim that in all language-trained apes the experimenters have provided the second Saussurian element: they have defined the social consensus by which meanings can be conveyed with the symbols. With this provided, some apes can become fairly good Saussurian performers; yet they have never invented such a consensus in the wild. In this I am in complete agreement with these commentators. In fact, I have proposed that the cognitive capacities whose absence prevented apes from creating such a symbolic environment were two productive, or output, skills: first and foremost, mimetic skill, and second, a capacity for lexical invention.

R4. Mimesis as the "missing link." My proposal that mimesis is the "missing link" in human cognitive evolution and a necessary preadaptation for language has triggered a number of interesting comments, of which **Katz**'s is perhaps unique. Katz found my basic proposal plausible, but suggested that motivation might be an important key to human cognitive evolution. Apes might have a capacity for mime, he suggests, but they just don't *care* enough to invent representations. Early hominids had to become more "obsessive-compulsive" in their emotional makeup before investing in such innovations. This is a fascinating idea, but it assumes that the appropriate cognitive capacity must be already in place; it is hard to imagine how traits like hyper-curiosity or an obsessive need to represent (call it protophilosophy) could emerge unless the required capacity was already there. But generally the presence of "talent" always involves a motivational element and Katz must be at least partly right: in a rivalry between a group of incurious, impulsive hominids, and a neighboring group of obsessive-compulsive hominids, I have no doubt that the latter would eventually win out in most survival-related skills.

Katz also questioned the need for a distinct "mimetic controller," especially since it looked (to him) like just another brain map, albeit a more abstract one. I do not see the mimetic controller in such mechanical terms, however; if it involves "mapping" (in the conventional sense) at all, it is very abstract mapping indeed, especially since it must be at base an *active modeling device*. We really do not have the slightest idea how the mammalian brain goes about the business of social event-perception; whatever that process is, however, it came to govern mimetic action by means of some sort of metaphoric approximation. The evolution of such a capacity would have required a fundamental change in the way the brain operates, since the new (mimetic) process required a much more sophisticated implementable self-image, which could be integrated with perceptions of external events and modified accordingly. A detailed hypothesis about how such a device might be implemented in neural tissue will depend on a very considerable advance in our understanding of how the brain stores episodic knowledge.

Bridgeman & Azmitia have presented some intriguing ideas about mimetic culture by pursuing the analogy of modern sport. Why is modern spectator sport so popular throughout the human world, cutting across so many otherwise impermeable cultural boundaries? I agree with most of what these authors said; spectator sport is one of the aspects of modern culture that I regard as essentially mimetic in its origins. And the emotive and motivational aspect of sport – particularly the mass-ritual aspect – seems impossible to understand without postulating some sort of deeply rooted cognitive and cultural universal. This phenomenon is extremely persuasive; I find it impossible to accept the notion that a uniquely human social spectacle so focused on motor skill and nonverbal performance could have its primary origins in language or symbolic processing. In addition, the effectiveness of the kinematic imagery-based training technique called "mental practice" is certainly at least a partial validation of the need for a "mimetic controller." [See also Jeannerod: "The Representing Brain" *BBS* 17(2) 1994.]

Wynn has provided important corroborating evidence that mimetic skill might have been both necessary and sufficient to manage the kinds of toolmaking achievements that have been attributed to late *Homo erectus*. The biface, a stone tool associated with *Homo erectus*, has a standardized symmetrical shape, whereas ape tools and even Oldowan tools lack these features. Wynn argues that any arbitrary standardized shape, and the adoption of symmetry in particular, implies that the shape of the artifact was intended and that there was some form of social consensus on this toolmaking "custom." I agree with his interpretation, as far as it goes. Wynn is also concerned that we have no direct evidence of "representation" in *Homo erectus;* I am not sure what he means by this, but I have argued that evidence for any kind of mimetic culture is a priori evidence of action-metaphoric representation. There could be no consensual, arbitrary "custom" such as he has described without an underlying mimetic representation of the act of toolmaking itself, and of the idealized objective of that act, the standardized biface.

Csányi makes an interesting distinction between genetically constrained cooperative schemata, such as might be found in lions, and the *individualized* schemata used by humans in cooperative behavior. The ultimate tool in constructing such schemata is language, but Csányi suggests that a mimetic capability might serve as the basis of a social super-model. He suggests that the roots of this may be found in animal "rituals" that are driven by genetically constrained schemata but become individualized by means of mimetic capability. This fits in nicely with my own perspective; in fact, I wrote that mimetic custom forms the basis of a shared model of society.

I disagree with **Csányi,** however, on the question of whether genetic factors might play a role in recent changes characterizing the Third Transition, as suggested by Lumsden and Wilson (1981). Although I have not yet developed a firm position of this issue, this kind of evolution now seems quite unlikely both because the rate of cultural change has been too fast and because fixing specific cultural content in genetic "concrete" would now appear highly maladaptive. Flexibility is the name of the game in the modern world, and I think current selection pressures would favor increased plasticity. It seems to me that fixing specific cultural content in the genes, even at a very abstract level, would militate against plasticity, which would be unlikely at this stage in our evolution.

Laakso has suggested that mimesis might perhaps be regarded as the *only* major representational breakthrough, after which speech was merely an add-on, without its own level of representation. Although this is an intriguing suggestion, I cannot agree with it. The philosophical distinction between natural and conventional signs does not go far enough in specifying the differences between mimesis and language. Language capacity goes well beyond mimesis not only in the arbitrariness (and number) of its referents, but also in *what* it can represent, and the *principles* by which it achieves representation. Lexical invention is the product of a second level of representation does not provide us with a broad enough "theory of mind"; fair enough. But he then suggests that Peirce's (1897) theory of signs might provide cognitive evolutionists with a more adequate framework within which to discuss human cognitive evolution. This sounds a nostalgic note for me, since I was very interested in Peirce's work thirty years ago; but why should we return to a nineteenth-century notion of mind as "symbol-crunching"? If we like such theories, we have a much richer smorgasbord now to choose from: Newell (1992), Simon (1981), Johnson-Laird (1983), Anderson (1983), and so on. Peirce anticipated the comparative laboratory ap-

Lutz has raised the question of how artificial intelligence might model a complex process like mimesis. I agree with his insight that in order to model mimesis in a robot the programmer would have to place that robot in a *society* of similar robots, observing the social-expressive consequences of introducing various degrees of mimetic capacity into the group. The most unpredictable (and perhaps most important) unknown is the degree to which relatively small increments in individual mimetic capacity might be potentiated into distributed representational invention. Until computational models of this sort are possible we will not have any way of knowing where the critical "threshold" was – that crossing-over point at which ape expressive skills started to create cultures with a capacity for innovation and distinctness.

R5. The mimetic-linguistic boundary. I am encouraged that **Katz, Laakso,** and **Lutz** all seem to accept my central notion that an archaic adaptation like mimesis is logically prior to language and that language would not have been necessary to create a society with shared representations, customs, conventions, and a limited degree of cultural accumulation. **Hampson** has pointed out some recent behavioral research on equivalence and transfer which seems to have identified certain mental operations that are prior to language in the same sense that I think mimesis is. Although I am not fully familiar with that field, it appears to me that it is focused on what I would call episodic (event-perceptual) skills such as the perception of subtle interstimulus relations. This makes Hampson's claims very interesting, because he implies that humans have evolved not only on the mimetic and linguistic plane but that they have continued to evolve on the episodic plane as well. This idea is quite credible; as collective mimetic capacity emerged, it would have brought selection pressures to bear on episodic cognition as well, since the emerging mimetic expressive environment could only be as good, or as complex, as the event-perceptions that drove it.

I have argued that mimesis is basically independent of language in the modern mind; in fact, I go even further: pure mimesis does not operate on the same principle as language and does not depend on the existence of lexical entries, grammars, and the kinds of metagrammatical operations that drive language use. I have been challenged on these critical points by **Bickerton,** who thinks I misunderstand language. [See also Bickerton: "The Language Bioprogram Hypothesis" *BBS* 7(2) 1984.] Bickerton comes from a long line of thinkers (including many in the AI tradition) who cannot conceive of a human-like mind without some form of language, whether a protolanguage, or a form of mentalese, or some sort of symbol-like conceptual elements that must reside in mind at some unconscious level. Dennett (1992) has raised a somewhat similar point, although not in as strong a form as Bickerton, who wants to attribute all of humanity's advances to the evolution of language and who, in denying the possibility of a nonlinguistic representational preadaptation like mimesis, must base the earliest form of that knowledge in some sort of protolexicon. Bickerton would thus root the lexicon in a vague, protolinguistic, semiconscious realm of mind that might have sputtered along for millions of years before exploding as it has recently. The trouble is, as we have just discussed in the context of Saussure, the lexicon is (1) collective by nature; (2) something that has to be invented and explicitly tested in the linguistic market; and (3) built upon an autocuable motor capacity. Moreover, by its very nature the lexicon is the product of linguistic *modeling* of reality; that is, lexical invention stems from an intellectual need to label whatever thing or relation the mind wishes to "capture" linguistically.

Put differently, the modeling process comes first, and the words follow, having been invented by that process (both individually and collectively). Gradually the burgeoning lexicon allows the modeling linguistic intellect to construct better and better shared linguistic models of reality. If a species has the collective cognitive capacity to evolve the beginnings of a lexicon, it must necessarily have passed through a nonverbal representational revolution similar to what I label as "mimetic," otherwise there could be no basis at the output end of the system for constructing lexical morphologies. These very practical, very concrete preconditions for lexical invention set minimum standards for the kind of motor adaptation needed before protolanguage could emerge, even in the limited form suggested by **Bickerton.** And given such an adaptation, I believe a mimetic culture would have been the inevitable (and mostly serendipitous) result.

Bickerton accuses me of putting the cart before the horse, but where does he think words and grammars come from? Does he really still believe in innate categories and some form of genetically determined, built-in set of grammatical rules? The cultural reality appears to be that invention – lexical as well as grammatical – is a normal, everyday property of language, and that inventive process is mostly driven from without, in the sense that new words and grammars are rarely simple combinations or permutations of existing ones. Bickerton counters that grammatical inventions might come from "hitherto unused spaces within the linguistic envelope made available by biology," and that language's two sides – an abstract conceptual base and a word-store – are sufficient to account for all of our distinctly human properties of mind. In effect, mimetic representations were mere by-products of linguistic evolution.

At the same time, **Bickerton** wonders how the thought process that underlies human language could be so different from that of animals. Precisely. He has put his finger on *the* key question which motivated my concept of an archaic mimetic adaptation. But the problem of the animal-human gap is even more difficult than he thinks it is, because there appears to be not one, but two distinctively human forms of thought, one verbal-analytic, the other nonverbal-holistic (see, for example, Paivio 1986). Intelligence tests, aptitude tests, and a variety of performance paradigms also split along similar lines (Gardner 1983). Anyone who wishes to "account for" all distinctly human representations on the basis of language (even a two-sided model of language) will have some difficulty dealing with this point. How do we account for the unique power, and distinctive nature, of human nonverbal cognitions, if language is, in Dennett's (1991) words, our only distinctive "good trick"? This reflects the danger of holding too close to one literature. Linguists ignore the psychological literature to their peril. They may accuse psychologists (justly) of the same behavior; but it works both ways.

Bickerton, Thompson, and **Mitchell & Miles** raised some objections to my interpretations of the case of Brother John (Lecours & Joanette 1980). The main problem, which I discussed in my book, is that Brother John has profited throughout his life from fully developed language skills. Thus, even while his language system is temporarily dysfunctional, his cognitions may somehow have benefited indirectly from his history of language use; his perceptual categories, for example, may have been shaped by language, and he may have a number of cognitive habits that could not have been learned without language. In addition, the "semantic" base of language might somehow continue to function even when words are not available. I have conceded all of this, but it still does not nullify my claim of mimetic independence. The bottom line is that Brother John must not have relied upon the explicit or conscious use of language in any of his new experiences during his seizures, yet he remained conscious, used and understood gesture, and was later able to construct linguistic descriptions of his episodic memories of the seizure period. Bickerton asked how Brother John could possibly have been conscious without symbolic thought; indeed, that was exactly the question I asked myself when I first heard of the case. And the answer must be: there is more to consciousness than linguistic representation.

R6. Oral-mythic culture. Although **Dunbar** agrees with the line of attack I have taken with regard to the basic cultural functions of speech and language, he was disappointed that I did not place more emphasis on the social function of language. [See also Dunbar: "Coevolution of Neocortical Size, Group Size and Language in Humans," this issue.] I find this a little puzzling, since I had the impression that I placed great value on that particular dimension. In fact, I called language the "governing" representational level in oral-mythic culture, because oral language is the means by which social disputes are arbitrated, group decisions are made, and cooperative ventures are coordinated. But Dunbar's emphasis is on a more basic level; he would place the exchange of social information in the very forefront of linguistic evolution, prior to the emergence of language "in the symbolic sense." I agree with him on this point, although I did not say it in so many words. Mimesis was (and remains) quintessentially social in its expressive function, even though it also does other things, such as allowing better toolmaking. It follows that, as humans moved from a mimetic style of group thought and expression toward language, the earliest emerging lexical inventions must have been plugged directly into the intimate culture-building functions that were first served by mimetic representations. I agree that abstract linguistic thinking probably piggy-backed on top of what was primarily a massive social-cultural shift. In fact, this intuition was what drove me to label the three cognitive transitions as changes in the mode of *cultural* governance.

When I first drafted this theory, the three transitions were not labeled in these terms; my theory was, after all, primarily a cognitive one. Initially, the transitions were simply labeled "mimetic" and "linguistic." Then, as the importance of external memory came to the fore, the latter split into "internal semiotic" and "external semiotic" before it became clear to me that representations were always shared, and thus the representational "locus" and "style" of cultural governance was a central issue. Some representations dominate a society, others are secondary, and others are fleeting and less important in terms of their impact on mind and behavior. My concept of "mythic" culture expresses the style of thinking I think governs such cultures.

My use of the term "mythic" might be traced back to psychoanalysis and structural literary criticism, and perhaps even to French anthropology. **Halverson** has criticized my elevation of myth to a governing role, and has made the point, which I have heard more than once from English-speaking anthropologists, that myths are not all that significant, because most people regard them merely as storytelling or entertainment. This criticism misses my point altogether, and imposes a definition of myth (as mere storytelling) that I find extraordinarily narrow. The mythic content of a culture is not to be found in casual storytelling, nor is it found just by transcribing all the formal oral traditions of a people. If we have learned anything at all from our long experience with both psychoanalysis and structuralism, this is only part of the surface database. Mythic ideas may take a predominantly narrative form, but they are also full of imagery and archetypes, reflecting and transmitting cultural norms and ideas of cultural identity. They have a great impact on the individual's self-image, and reflect and reinforce custom and belief. The larger mythic content of Western culture – an envelope of largely unconscious governing assumptions – can be extended back thousands of years, and the same may be said for all human cultures. Preliterate cultures are particularly dominated by mythic ideas defined in this sense; the aboriginal form of human culture might hence be called oral-mythic. But most cultures that have adopted external symbols remain predominantly mythic; that is, they remain under the governing influence of mythic thought.

With regard to **Brace**'s accusation that I betrayed a degree of ethnocentric bias in my descriptions of aboriginal cultures, I acknowledge that I am of course somewhat ethnocentric in my viewpoint, and so, inevitably, is he; but neither of us, one hopes, is malign in intent. Brace's main concern seems to stem from a misunderstanding of what I mean by the mimetic dimension of culture. I don't regard mimesis as "primitive," although it is primary in my hypothetical succession. In fact, mimesis is the mainstay of two of our most powerful modern art-forms, opera and cinema; and mimesis is still the core representational skill and basic cultural currency of human society. Aboriginals mime; so did Chaplin and Nijinsky: this dimension is an important cultural universal. There is surely no shame in this. Mythic thought, not mimetic, predominates in all aboriginal cultures. Mythic conceptions ultimately govern and attempt to justify mimetic ritual and custom.

Halverson has also raised questions about whether my concept of the second transition is based on sound interpretations of Upper Paleolithic and Mesolithic archaeology. Although the Upper Paleolithic period is admittedly hard to pin down, I think Halverson is quoting me out of context and not acknowledging the many caveats and reservations I sprinkled throughout my book. He is basically right, however, in saying that I have "retrojected" the Stone Age onto the Upper Paleolithic, much the way I and most other thinkers in this field have also retrojected modern ape capacities onto the Miocene apes. We don't yet have strong enough evidential confirmation of our right to do this, but so far there have been no serious disconfirmations either. More important, there is good convergence with evidence from other sources. My theory of the primary function of language was based, as Halverson must by now realize, on much more than our rather weak archaeological record: it was based on various converging lines of argument from ethology, philosophy, psychology, and neurolinguistics.

Fetzer has suggested that my approach to language would profit from a "speech act" conception; I agree with this in principle, although in many ways I have already taken this into account by blending language onto a larger mimetic behavioral framework. Besides, I think it is not entirely accurate to label my approach as a strictly "propositional" theory of language. Fetzer has also argued that my distinction between mimetic and linguistic modes of representation does not provide us with a broad enough "theory of mind"; fair enough. But he then suggests that Peirce's (1897) theory of signs might provide cognitive evolutionists with a more adequate framework within which to discuss human cognitive evolution. This sounds a nostalgic note for me, since I was very interested in Peirce's work thirty years ago; but why should we return to a nineteenth-century notion of mind as "symbol-crunching"? If we like such theories, we have a much richer smorgasbord now to choose from: Newell (1992), Simon (1981), Johnson-Laird (1983), Anderson (1983), and so on. Peirce anticipated the comparative laboratory approach to mind, typified by the research listed by **Hampson,** which has long searched for that simple chain of intellectual being, a simple formula by which the evolutionary progression of cognition might be made clear. Bitterman's (1965) attempts at establishing a progressivist "hierarchy" of animal cognition come to mind in this context: fish can do X, birds can do $X + Y$, mammals $X + Y + Z$, and so on. (If those biologist-commentators [**Cynx & Clark,** etc.] who accused me of being covertly progressivist want more clearly delineated targets for their righteous thunderbolts, here they are!)

Fetzer falls squarely within the progressivist tradition; he is looking for a fixed logical hierarchy, a system within which we can place all "orders" of mind – rather like a representational table of elements. In contrast, I have suggested a best-guess sequence of what might actually have happened in the human case, based on as much relevant empirical data as I could find. My complaint with Fetzer's approach is the same one I have with old-line AI theorists who proclaimed grandly that the human mind is merely a particular instantiation of various "possible" minds (all of which looked uncannily like digital computers): its origins are not empirical, and its axioms are very far from self-evident. I would turn the tables on such proposals. Symbol-driven systems are a metaphor by which the modern human mind contemplates itself; but in fact, truly denotative symbols (such as those used in mathematics and computing, and those postulated by both Peirce and Saussure) seem to have arrived only yesterday (in evolutionary time) and represent a very small subset of human representations. Computing languages and denotative semiotic systems in general are the *inventions* of minds that have managed to operate without such devices until very recently, and if Wittgenstein (1922) is to be believed, still manage without them most of the time.

I am enough of a neurobiologist in my bones to be suspicious of any nonempirical attempt to establish an abstract "theory of the mind," especially when it is built primarily on an analysis of input structure, rather than representational (output) skills. Does a simple neural net become an "iconic" knower when it generalizes a new stimuli? Are all successfully conditioned reinforcers thereby "indexical"? Not at all; these minds are still locked into an essentially passive, episodic mode of knowing. As I have said, many animals can use symbols in various ways; but unless they can *invent* them (which they cannot) they cannot be classified as having a capacity for symbolic representation. Symbols, even of the most primitive type, are different in principle from event-perceptions in that they are retrievable outputs encapsulating knowledge, and the result of the *active modeling* of that knowledge. Thus, any "order" of mind based simply on a logical hierarchy of increasingly abstract stimulus properties or stimulus relations cannot succeed in capturing the essence of the representational intellect.

Moreover, the slow evolutionary process that took humans out of episodic passivity into an actively representational mental universe should not be expected to be tidy. Evolution is generally kludge-prone and messy, and deals with terrifying degrees of complexity; we have no reason to expect cognitive evolution to be any simpler or neater. Anyone who wishes to construct a useful modern theory of the mind needs to move away from the traditional philosophical obsession with the structure of knowledge itself, instead focusing squarely on how representations came to be invented, by what principles, and for what biological purposes. Representations do not exist in some neo-Platonic ether waiting quietly to be described. Representations are ultimately the manifest outputs of individual human beings, distributed in culture; and the structure of our mental universe is constrained by the neurobiological nature of the beast that is generating those outputs.

R7. The boundary zone between internal and external memory. The idea that concepts exist in and move through a shared information-space has been around for a long time; the Greeks had already noted the independence of ideas from their human "hosts." In our modern era the most famous version of this notion has been Dawkins's (1976; 1982) idea of the "meme," a semantic element that can replicate itself directly in minds, or in symbols invented by and for minds. Memes exist only on the cultural surface; they are defined by semantic content, in terms of the size of the smallest replicating units: thus, complex ideas like "backgammon" or "breakdancing" or "Byzantium" are memes, because they replicate as units. As **Arbib** has pointed out, the notion of a "schema" might serve a similar purpose, presumably defined in terms of function rather than semantic content; schemas, like memes, might "evolve" on the basis of their reproductive success. [See also Arbib: "Levels of Modeling of Mechanisms of Visually Guided Behavior" *BBS* 10(3) 1987.] My preference in this regard has been for the more general word "representations." Like their physical counterparts in the genes, the fittest representations survive, the weak die out, and their selfish "interests" may or may not coincide with those of their human hosts (Dennett 1991). [See also Dennett: "Intentional Systems in Cognitive Ethology" *BBS* 6(3) 1983 and multiple book review of "*The Intentional Stance*," *BBS* 11(3) 1988.]

Plotkin has suggested that I have (perhaps inadvertently) provided a way of integrating Dawkins's meme theory into a cognitive context. [See also Plotkin & Odling-Smee: "A Multiple-Level Model of Evolution and Its Implications for Sociobiology" *BBS* 4(2) 1981.] In effect, I may have identified the "replicators," as well as the transmission devices, for three different levels of meme-evolution (I have presumably gone one step further than this, and specified the generators of memetic variation as well). Plotkin's suggestion points to the intriguing possibility of constructing a more detailed and testable synthesis of cognitive science and this aspect of evolutionary theory. I think this is a constructive approach; it is certainly reasonable to expect the perspective afforded by Dawkins to interact profitably with my own. On the other hand, there are some problems in the way of this exercise. I think Dawkins tends to anthropomorphize his memes; or it is perhaps more accurate to say that he attributes life-like properties to memes, as if they had a degree of autonomy. Once released into the "memosphere," they supposedly parasitize their human hosts. In **Gabora**'s even stronger terminology, they are "idea-parasites," and as the brain evolved, they took the opportunity to "travel new evolutionary trajectories," just as seeds, given new opportunities, can spread to new and different ecologies.

This idea has an almost fatal fascination for students: it rivets their attention because it hints at dark uncontrollable mind-gobbling forces, much the way Freud's notion of unconscious forces riveted the attention of his generation. The much more mundane reality is that each "meme" is the painstaking creation of a group of humans interacting in a shared knowledge environment and that the "replication" of memes is generally possible only in a culture that already contains most of the elements inherent in that meme. Memes have no independent existence; each is a small increment, a slight variation, on some aspect of existing culture. And they are interconnected and mutually interactive: unlike genes, memes are not really discrete units. They are part of a larger, tangled semantic fabric, more like the fuzzy blobs of paint on a Jackson Pollock canvas than the discrete squares on a chessboard. The work of deconstructionism has suggested, at least to me, that semantic space is more relativistic than natural scientists might prefer it to be; it follows that the definition, and thus the isolability, of most memes is subjective and rather questionable.

Gabora acknowledges that the gene-meme analogy breaks down, particularly in the way variants are generated. Where genetic variation is random, cultures evolve by systematically varying an internalized model of the world. She then tries to salvage the analogy by proposing that complex "fitness landscapes" are internalized, and the selection process is thus speeded up and, if I understand the implications of her proposal correctly, must therefore become at least potentially purposive. I find these parallels interesting but potentially misleading because Gabora remains very abstract and theoretical in her stance, and is thus spared the messy business of actually identifying and isolating memes. This leads to a chastening thought: Where would modern genetics be if geneticists had no way of agreeing on how to identify or isolate specific genes?

Arbib raised the important question of why theoretic culture appears to be so unstable and explosive, whereas mimetic and mythic cultures appear to be much more stable. This seems to be a function of the increased availability of external representations, the spread of externally driven networks, and a greatly increased epistemic turnover. We shouldn't underestimate oral-mythic culture. In relative terms, oral-mythic culture was undoubtedly explosive when compared with mimetic. The rate of oral-linguistic evolution over the past twenty thousand years can only be described as fantastic. The rate at which oral language capacity generated phonological, semantic, and grammatical variation across the inhabited human world would have been completely stupefying to poor old *Homo erectus*. But we should not underestimate our archaic ancestors, either; the rate and extent of mimetic cultural innovation in late *Homo erectus* would probably have baffled any pongids that came into contact with them; if there is one constant in the history of hominid cultural change, it is that there has been a steady increase in the rate of cultural variation.

Clark is unclear on my distinction between the problem of symbolic invention and the problem of reference; the latter addresses a specific question, vital to the computational view of language, namely, that words cannot in general be defined in terms of symbols or other words; their semantic content remains therefore out of the reach of current theoretical tools. Symbolic invention presents us with a different challenge; Clark tries a Dennettian deflationary tactic, shifting the onus from the generative mechanism back onto the existing linguistic environment. This has a certain face validity, provided it isn't pushed too far: symbolic invention is always embedded in a cultural context, and feral (i.e., socially isolated) children have not generally fared well in terms of symbol skills, except possibly some mimetic skills. This reversal of my strategy quickly runs into a brick wall, however, as Clark realizes. Something must prime the language-invention process; and, I would argue, something must also keep it going, given the documented rate of linguistic change. I agree with Clark that symbolic invention extends to more than external symbols, and to all kinds of representations; in fact, that was the meaning I intended to convey in my book.

Clarke refers to a change in the way humans are self-aware that occurred between the *Iliad* and *Odyssey*, and implicitly attributes this change to writing. In his highly original and elegant book, Jaynes (1977) had earlier traced the origins of modern self-consciousness to this period, and described a similar shift from the oldest parts of the Old Testament to the *Book of Job*, in which Job was presented as a fully self-aware modern human. I would be hesitant to attribute this introspective mien to writing, however; it is ironic that the very absence of writing makes it impossible to say whether most preliterate societies were similarly self-aware. It is at least equally probable, I would think, that what changed between the *Iliad* and *Odyssey* was a poetic convention. Not all things that are thought end up being written down, and writing has generally been reserved for certain kinds of thoughts, just as there have been very strong conventions about what an artist is permitted to display publicly in a play or a painting. One could counter that explicit cultural awareness only develops when an idea is publicly debated and displayed, but it is possible that oral cultures developed many ideas, at least in germinal form, long before any of them were written down. I should be the last to diminish the revolutionary effect of external symbols, but we should also be careful not to underestimate preliterate cultures, just as cultural relativists should try not to underestimate the effects of technological change. I agree with Clarke's speculation that our current electronic technology might be pushing us even farther down the road toward distributed cognition, and perhaps new ways of seeing reality.

Costall pushes the idea of distributed cognition much farther than I do, exploring some of the radical implications suggested by such an exercise. If I follow his meaning correctly, Costall is saying that psychology should abandon the internalist (or monadic) model altogether, always situating cognition instead in its cultural context. If this idea is pushed to its limit, the individual presumably disappears as a viable unit, reduced instead to some sort of node in a social environment. Costall overinterprets some of my own use of network jargon, however; I don't really reduce human social context to codes and external storage systems, except inasmuch as such a structure is imposed. An analogy to industrialization is apt here; when workers were coopted into mass production (a trend that started at least as far back as the first pyramids), they became parts of "man-machine systems," but that is not *all* that they were. Similarly, when modern humans plug into the electronic highway, they temporarily become nodes in a network; but that is not *all* that they are, one hopes.

Deregowski has focused on what I would call visuosymbolic invention, alluding to Kennedy's (1975) idea that drawings were "discovered." [See also Deregowski: "Real Space and Represented Space" *BBS* 12(1) 1989.] If this was the case, he asks, why should drawing skills have appeared so late in our evolution? Wouldn't they be properly placed at the mimetic phase? Deregowski might have noticed that in those curious little diagrams of representational architecture that I regrettably included in Chapter 8 of my book I linked the "pictorial path" directly to episodic representations, that is, to the perceptual systems. This was done on evidence that apes can understand line-drawings; thus, pictures as *input* seem to be processed, at least initially, through standard perceptual channels. But their *production* is another matter; the act of drawing closely parallels iconic and metaphoric gesturing; when we make an iconic gesture in mud, sand, or snow, we have a simple drawing. I agree that these are essentially mimetic skills; then why not place them further back in time? The reason is simple; I know of no good archaeological evidence for crafted pictorial or sculptural representations until well after the arrival of modern *Homo sapiens*. There is no a priori reason, however, why primitive drawings might not have shown up earlier than this, especially in impermanent form. However, I think it is unlikely that a permanent drawing medium, which involves combining two or more complex technologies, could have been developed without language.

I am less inclined to agree with **Deregowski**'s other point, about an extra path in the history of drawing. Although the detailed depiction of individuals may be different from making "primitive" figures, it does not seem to involve a different underlying principle. The operative criterion is still pictorial, rather than ideographic. I have deliberately left a great deal of latitude in my definition of this term, so that it may be extended to include a wide range of graphic images that are prevalent in the modern world. As to my blind spot about Adam Smith's Glaswegian connection, I can only say that my great-grandfather's family came ultimately from the Western Isles; there may have been unconscious forces at work here.

Feldman suggests that I overestimate the power of distributed computing systems, pointing out that there are tremendous difficulties involved in coordinating a network in any kind of problem-solving; I presume this implies that until those problems have been solved my kind of model will not be testable. I agree, but only in part. I depend on the network metaphor only in a loose sense, and I certainly do not suggest that current computational networks are a good model for the whole of human culture. In fact, I did suggest, in several places, that social coordination was one of the prime movers of both mimetic and linguistic evolution. Networks are becoming more sophisticated, and computer-coordinated cooperative work is already a reality in the marketplace. As we are drawn more into coordinated networks, we will become subject to their possibilities and constraints. For the moment, the analogy is used in a much more restricted sense, more as a metaphor than as a testable model. [See also Feldman: "Four Frames Suffice" *BBS* 8(2) 1985.]

Gilhooly suggests that I may have overestimated the limitations of biological working memory, which can be extended without relying on external memory. He cites Ericsson and Kintsch's (1991) theory that experts can overcome the limits of working memory by encoding more efficiently for rapid retrieval. There is some question in my mind whether this is truly a case of extending working memory, or rather devising highly efficient and automated search-and-retrieval strategies for a specific subregion of long-term memory. In either case, it allows the expert to work in certain circumscribed domains much more efficiently than the novice. Gilhooly does not diminish the importance of external memory devices, however. I would add that experts are also better than average at using external memory devices; they are obviously faster at locating, retrieving, and processing the encoded material they use. Experts are prime users of electronic retrieval devices like financial market reports and scientific databases; they are also the prime users of scholarly libraries, archives, and other external devices such as computer-assisted design programs. In all these cases, the expert user must use both internal and external memory in an integrated manner. This is what I meant by the memory-management "baggage" imposed by high levels of literacy; these skills take up space and involve thousands of hours of intensive training.

Halverson downplays the role of external storage, pointing out that the Greeks probably were not as literate as we popularly believe. I have acknowledged this elsewhere (Donald 1993), but this does not affect the point I was making when I reviewed the early history of writing. I have specifically refuted the claim that writing itself was the main cause of the Greeks' breakthroughs in science, mathematics, and philosophy. Their achievement was in the way they used writing; they were the first to expose the *process* of thought to critical public formulation and review. It is revealing that even their manuals for teaching oral rhetoric, a central skill in their political system, were written down. The point is that external memory can play a crucial role in the germination of an idea, especially in the critical review of ideas and their gradual improvement. Once those ideas have been formulated, however, they can be disseminated and discussed orally. Indeed, it is normal and useful even in our high-tech environment to alternate between oral discussion and written text. It is extremely doubtful, however, that an exchange such as this one could be anywhere near as complex and disciplined without carefully crafted written expressions of the various ideas being discussed. Transcriptions of oral discussions are usually astonishingly disconnected and imprecise (as I can testify, having recently tried to edit one of my tape-recorded colloquia). External memory has been at the center of governance, from early agricultural theocracies to the modern postindustrial state.

Small made some very pertinent points about the difficulties of using external memory devices in ancient society. Manuscripts were stored in awkward formats, lacked tables of contents, alphabetic indexes, and paragraph structure; moreover, even the alphabet didn't simplify writing all that much, given the thousands of abbreviations in use. Hence these manuscripts imposed a tremendous memory load on the reader and made both the location and specific search of any manuscript very difficult. This in turn required an improvement in *biological* memory – a very good point. However, the importance of external memory is not necessarily diminished by these considerations. Reading was an elite scholarly activity; ancient experts, like modern ones, undoubtedly became extremely adept at the skills needed to use their particular set of symbolic artifacts. Perhaps, given their familiarity with a fairly limited number of manuscripts, they did not see the need for indexes, paragraph structure, and the like. Moreover, like modern experts, they were becoming more and more efficient at combining internal and external memory sources in their work.

Zhang questioned whether external memory really is an exact analog of internal memory, correctly pointing out that an external symbol only works as a distributed representation, integrating external storage and internal interpretation, and thus does not really reside independently outside the brain of the observer who uses the symbol. This is an astute objection and forces me to explain precisely what I meant. First, I was not speaking of all engrams, only of those that store symbolic representations. Engrams, probably in some form of synaptic facilitation pattern within neural networks, are themselves simply physical media of storage, just like blobs of ink on paper or logic levels in a computing device. When an engram or exogram *of a representation* is "retrieved," it is usually located on the basis of its internal morphology or external form. Both internal and external representations map form onto meaning; and morphology alone, whether internal or external, remains meaningless. Thus, both engrams and exograms must be mapped onto a semantic network that resides "elsewhere." In this sense the analogy seems to hold up. But there is a flaw in it: engrams can also be addressed directly via their semantic dimension, whereas exograms cannot. Thus I have to concede that the analogy is somewhat inexact.

R8. Consciousness. My account of consciousness in a hybrid mental architecture was judged by **Thompson** to be more satisfying than accounts that hold too close to language. In a sense, humans have gone through a series of "displacements" of the central processor, so that there are several integrative "processors" arranged in an intransitive hierarchy, for want of a better term. This allows the control functions of consciousness to be located in a variety of different places in the overall architecture of mind, and accounts for many of the strange neurological dissociations commonly observed in the clinic. It also allows the quality of consciousness to differ radically, depending on which style of representation is temporarily dominant. I think I have avoided some of the pitfalls of the Cartesian Theater assumption that Dennett (1991) so ably destroyed [see also Dennett & Kinsbourne: "Time and the Observer" *BBS* 15(2) 1992], and I have also tried to avoid invoking the "platoons of homunculi" that typify some AI accounts (including, I suspect, his). Dennett might want to accuse me of installing several Cartesian Theaters, each with a differently equipped viewer, in the head. I wouldn't necessarily reject that suggestion out of hand; the need to account for ideational integration is there, and I have difficulty accepting the epiphenomenalism inherent in his Multiple Drafts theory. Ultimately, we seem to agree that conscious experience is held together by representations, rather than by a specific "place" in the brain; but those representations must reside in appropriate physical systems with special properties, and at least one of those systems is nonlinguistic.

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Letters *a* and *r* appearing before authors' initials refer to target article and response respectively.

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