

# Précis of *Origins of the modern mind: Three stages in the evolution of culture and cognition*

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**Abstract:** This book proposes a theory of human cognitive evolution, drawing from paleontology, linguistics, anthropology, cognitive science, and especially neuropsychology. The properties of humankind's brain, culture, and cognition have coevolved in a tight iterative loop; the main event in human evolution has occurred at the cognitive level, however, mediating change at the anatomical and cultural levels. During the past two million years humans have passed through three major cognitive transitions, each of which has left the human mind with a new way of representing reality and a new form of culture. Modern humans consequently have three systems of memory representation that were not available to our closest primate relatives: mimetic skill, language, and external symbols. These three systems are supported by new types of "hard" storage devices, two of which (mimetic and linguistic) are biological, one technological. Full symbolic literacy consists of a complex of skills for interacting with the external memory system. The independence of these three uniquely human ways of representing knowledge is suggested in the way the mind breaks down after brain injury and confirmed by various other lines of evidence. Each of the three systems is based on an *inventive* capacity, and the products of those capacities – such as languages, symbols, gestures, social rituals, and images – continue to be invented and vetted in the social arena. Cognitive evolution is not yet complete: the externalization of memory has altered the actual memory architecture within which humans think. This is changing the role of biological memory and the way in which the human brain deploys its resources; it is also changing the form of modern culture.

**Keywords:** cognition; cultural evolution; culture; distributed representations; external memory; human evolution; knowledge; language origins; mimesis; motor skill; neuropsychology; symbols; working memory

This book (Donald 1991) was an attempt to synthesize various sources of information – neurobiological, psychological, archaeological, and anthropological, among others – about our cognitive origins, in the belief that the human mind coevolved in close interaction with both brain and culture. I should make clear from the start that I have no illusions about my ability to become expert in all of the disciplines touched on by this enterprise; accordingly, my effort should be regarded with suspicion by all; at best, it will probably prove to be no more than a guide to some of the important questions that remain to be settled. This Précis focuses on my core theory and disregards most of the background material reviewed at length in the book itself.

My central hypothesis is that there were three major cognitive transformations by which the modern human mind emerged over several million years, starting with a complex of skills presumably resembling those of the chimpanzee. These transformations left, on the one hand, three new, uniquely human systems of memory representation, and on the other, three interwoven layers of human culture, each supported by its corresponding set of representations. I agree with multilevel evolutionary theorists like Plotkin (1988), who believe that selection pressures at this stage of human evolution were ultimately expressed and tested on the sociocultural level; hence I have described the evolutionary scenario as a series of

cultural adaptations, even though individual cognition was really where the main event was taking place, since it provides the link between physical and cultural evolution. [See also Plotkin & Odling-Smee: "A Multiple Level Model of Evolution and its Implications for Sociobiology" *BBS* 4(2) 1981.]

In one sense the proposed evolutionary sequence is an exercise in interpolation not unlike many other efforts to construct a credible case for the emergence of particular morphological and behavioral features in various species. But in another sense it is a structural theory that confronts the question of how many processing levels must be interposed between the nonsymbolic cognitions of animals and the fully symbolic representations of humans. Symbolic representation is the principal cognitive signature of humans and the main phenomenon whose arrival on the scene has to be accounted for in any scenario of human evolution.

The theory posits a series of radical evolutionary changes – the punctuations, as it were, in punctuated equilibrium – rather than a continuous or unitary process. I do not rule out the possibility, indeed the likelihood, of smaller graduated changes that might also have occurred during the long period of human emergence; but judging from the anatomical and cultural remains left by hominids and early humans, the most important evolutionary steps were concentrated into a few transition

periods when the process of change was greatly accelerated, and these major transitions introduced fundamentally new capacities.

I have made certain hard choices – for instance, I have opted for a late-language model, placing language near the end of the human evolutionary story rather than much earlier, as Parker and Gibson (1979) and Bickerton (1990) have. [See also Bickerton: “The Language Bioprogram Hypothesis” *BBS* 7(2) 1984.] For another, I have opted for a lexically driven model of language evolution, rather than placing the main emphasis on phonology, as Lieberman (1984) has, or on grammar, as Bickerton (1990) has. In fact, I have portrayed our capacity for lexical invention as a single pivotal adaptation capable of evolving into an instrument of sufficient power to support all of the higher aspects of language.

Moreover, I have postulated an early motor adaptation, intermediate between ape cognition and language, that gives primacy to the unique motor and nonverbal cognitive skills of humans. In this I am in basic agreement with Kimura (1976) and Corballis (1989; 1991), who have also argued for an early motor adaptation that preceded speech. However, I differ from these two authors in that I am much less concerned with the issue of cerebral laterality, and more focused on the representational possibilities inherent in an early motor adaptation. Moreover, I do not agree with them about the close qualitative linkage between language and serial motor skill; I see the two as qualitatively different, albeit interdependent, adaptations.

I have also tried to build a theory in such a way that specific details of chronology are not crucial to its central hypothesis, which is essentially concerned with cognitive *succession*, and consequent modern cognitive *structure*. Finally, I have chosen to construct my succession hypothesis around a fairly simple unifying theme, that of evolving cognitive architecture. This is based on my belief that brains store memories in and around their functional processors rather than somewhere else, as most computers do; and therefore that radically new representational strategies signal the likelihood of a change in the underlying neuropsychological architecture (keeping in mind that such changes may be anatomically complex).

**Chronology, succession and transition.** Chronology is important in that it helps us establish an order of succession and determines how many major cognitive steps were taken, and roughly when. This issue was not as difficult to resolve as one might have expected, given the controversy that seems to pursue archaeological finds. There is considerable stability in the basic number of hominid species that are currently interposed between humans and Miocene apes and in their order of appearance. Moreover, there is agreement that although australopithecines undoubtedly underwent massive anatomical and cultural change in adopting erect posture, they did not leave any evidence suggesting major cognitive evolution. There appear to be only two strong candidates for a major breakpoint in hominid cognitive evolution and these coincide with the transition periods leading to the speciation of *Homo erectus* (about –1.5 M years) and archaic *Homo sapiens* (roughly –0.3 M years), respectively. Four recent books on this subject (Bickerton 1990;

Corballis 1991; Donald 1991; Lieberman 1991) have all agreed on this basic point.

The relative brain size of *Homo erectus* was much larger than that of previous hominids, eventually exceeding 70% of the modern human brain. The upward linear trend of hominid brain size accelerated sharply during the transition to *Homo erectus* and was sustained until *Homo sapiens* emerged (Lieberman 1991). This rapid increase in cerebral volume was concentrated in the association cortex, hippocampus, and cerebellum. Even taken in isolation of cultural artifacts, these anatomical facts would suggest a significant cognitive change. But the cultural evidence left by *Homo erectus* strongly confirms the presence of major cognitive evolution: *Homo erectus* manufactured quite sophisticated stone tools, devised long-distance hunting strategies, and migrated out of Africa over much of the Eurasian landmass.

A second major transition period preceded the speciation of *Homo sapiens* and was marked by another large brain expansion and the descent of the larynx. As Lieberman (1984) has argued, the latter probably coincided with the emergence of spoken language as we know it, that is, with the arrival of a high-speed vocal communication system driven by a large lexicon containing thousands of entries. Our exact lineage is still not known, but modern humans appear to have reached our present form some time prior to 45,000 years ago. All modern humans have a fully developed speech capacity, as well as complex oral cultures that incorporate myth, religion, and social ritual. This would suggest that our final period of major biological change extended over the late Middle and early Upper Paleolithic periods.

My decision to postulate a third transition in human cognitive evolution takes this scenario out of the realm of purely biological evolution toward a definition of evolution that is at once broader and more purely cognitive. If the descriptive criteria for major cognitive transitions are held constant throughout human history and prehistory, it is obvious that there have been some very major changes since the Upper Paleolithic. The likelihood that the specific mechanism of such recent change is nongenetic should not distract us from making that observation and exploring it to the fullest. Recent cognitive change is evident primarily in cultural artifacts and might have been classified along many different continua; I have singled out the development of external memory as the critical issue. The third transition seems to have started in the late Upper Paleolithic with the invention of the first permanent visual symbols; and it is still under way.

**Structural issues.** Here the main structural questions are: What new cognitive features were introduced at each of these three stages? And how do these three developments coalesce in the modern brain and mind and express themselves in culture? In my proposal all three stages introduced new memory features into the human cognitive system. One important consequence has been greatly improved voluntary access to memory representations; in effect, humans have evolved the architecture needed to support what Graf and Schacter (1985) have called “explicit” memory retrieval.

The first transition introduced two fundamentally new cognitive features: a supramodal, motor-modeling capac-

ity called *mimesis*, which created representations that had the critical property of *voluntary retrievability*. The second transition added two more features: a capacity for *lexical invention* and a high-speed *phonological apparatus*, the latter being a specialized mimetic subsystem. The third transition introduced *external memory* storage and retrieval and a *new working memory architecture*.

The structural arrangement of these uniquely human representational systems is hierarchical, with mimetic skill serving as a necessary but not sufficient condition for language, while language capacity is a necessary condition for the invention of external memory devices. All of these representational systems are at the high end of the system and are aspects of what is sometimes vaguely called the "central processor" (see especially Fodor 1983; see also multiple book review: *BBS* 8(1) 1985). This proposal therefore implies that the human version of the central processor has evolved through a series of major changes and is now complex and quasimodular in its internal structure.

### 1. The starting point: The abilities of apes

Apes are brilliant event perceivers; as I have acknowledged in my book, they have a significant capacity for social attribution, insight, and deception, and great sensitivity to the significance of environmental events. In the latter category, I include the signing systems provided by human trainers; these are best treated as complex environmental events or challenges to which apes respond with their usual perspicuity (see, e.g., Savage-Rumbaugh 1980).

I agree with Olton's (1984) suggestion that apes have episodic memory, that is, the ability to store their perceptions of specific episodes (a position that Tulving [1984] evidently agrees with). However, they have very poor episodic recall, because they cannot self-trigger their memories: that is, they have great difficulty in gaining voluntary access to the contents of their own episodic memories *independent of environmental cues*. Thus they are largely environmentally driven, or conditioned, in their behavior, and show very little independent thought that is not directly related to specific episodes. I have called their style of thought and culture "episodic."

The limits of ape intelligence seem to be especially evident on the production side of the cognitive system. Bright as their event perceptions reveal them to be, they cannot express that knowledge. This limitation stems from their inability either to actively shape and modify their own actions or to voluntarily access their own stored representations. This might be why they cannot seem to invent gestures or mimes to communicate even the simplest intention (see, for instance, Crawford, cited in Munn 1971). They can learn signs made available by human trainers but they do not invent them on their own; nor do they seem to consciously "model" their patterns of movement, in the sense of reflecting on them, experimenting with them, and pushing them to the limits, the way humans do. This seems to indicate that they are far less developed than humans in at least two areas of motor control: the construction of conscious action models, and the voluntary independent retrieval of such models.

Without easy independent access to voluntary motor memories, even simple operations like self-cued rehearsal and purposive refinement of one's own skill are impossible, because the cognitive system remains primarily reactive, designed to react to real-world situations as they occur, and not to represent or reflect on them. Thus apes are not good at improving their skills through systematic rehearsal. The contrast with human children in this regard is striking: some apes might throw projectiles in a fight, but they do not systematically practice and improve their throwing skill the way human children do. The same applies to other kinds of voluntary action; children actively and routinely rehearse and refine all kinds of action, including facial expressions, vocalizations, climbing, balancing, building things, and so on. Although apes may have the same basic repertoire of acts, they do not rehearse and refine them, at least not on their own. In fact, it takes an incredible amount of training – on the order of thousands of trials – just to establish a single reliable naming response in chimps, and even those very context-specific responses remain reactive and episodic: for example, 97% of Kanzi's signing consists of direct requests (Greenfield & Savage-Rumbaugh 1990). Until hominids were able to model their actions and their episodic event perceptions and access those representations independently of environmental stimulation, they, like all higher mammals before them, were locked into an episodic lifestyle, no matter how sophisticated their event perceptions had become.

### 2. First transition: Mimetic skill and autocueing

The rationale for the first transition is based on several premises: (a) the first truly human cognitive breakthrough was a revolution in motor skill – mimetic skill – which enabled hominids to use the whole body as a representational device; (b) this mimetic adaptation had two critical features: it was a *multimodal* modeling system and it had a *self-triggered* rehearsal loop (that is, it could voluntarily access and retrieve its own outputs); (c) the sociocultural implications of mimetic skill are considerable and could explain the documented achievements of *Homo erectus*; (d) in modern humans, mimetic skill in its broadest definition is dissociable from language-based skills and retains its own realm of cultural usefulness; and (e) the mimetic motor adaptation set the stage for the later evolution of language.

**(a) The primacy of motor evolution.** The first really major cognitive breakthrough and the appearance of the first truly human-like species seem to have occurred with *Homo erectus*. The question commonly asked is: Do we need to attribute some language capacity to this species? This is inherently a structural question, since it asks whether language – rather, symbolic thought – is primary in the human cognitive hierarchy. Placing language this early in evolution, giving it this primacy, is a vote for a symbol-based, computational model of all human thinking.

The evidence supporting the premise that *Homo erectus* was unlikely to have had language has been reviewed in several places (most extensively by Lieber-

man 1984; 1991; but also by Corballis 1991; and Donald 1991). No modern investigator has argued that *Homo erectus* had speech or anything like it. However, to explain achievements like toolmaking and social coordination, several authors have attributed to *Homo erectus* a limited degree of linguistic capacity, usually labeled "proto-language" (Bickerton 1990; Parker & Gibson 1979). The current form of this notion is that *Homo erectus* had the linguistic capabilities of a two-year-old child, namely, one- and two-word utterances and intentional gesturing, but no grammar (Bickerton 1990). This seems feasible, since there have been claims that apes are very close to achieving this (see, e.g., Greenfield & Savage-Rumbaugh 1990).

There are serious problems with this position, however. For one thing, it puts the cart before the horse; it leaves out a prior motor adaptation without which language could never have evolved. In reality, apes are not even close to two-year-old children in the way they use symbols, except perhaps in their perceptions of the utility of symbols. On the motor side, they cannot even match what infants achieve during the babbling phase, let alone later on when children acquire reference, because apes cannot rehearse and refine movement *on their own*, or create models of reality *on their own*. Early language theories of evolution are seeking a "quick fix" solution, a rapid leap to some form of language without attending to the more fundamental motor changes that must have preceded it. (In the process, of course, these theories also sustain the AI agenda that attributes all higher cognition to a symbolizing process; Dennett 1992; Donald 1993.)

The primacy of motor evolution is central to any credible phylogenetic account of language. Before they could invent a lexicon, hominids first had to acquire a capacity for the voluntary retrieval of stored motor memories, and this retrieval had to become independent of environmental cueing. Second, they had to acquire a capacity for actively modeling and modifying *their own movement*. Without these two features, the motor production system could not break the stranglehold of the environment. Any language system assumes the ability of the speaker both to actively construct communicative acts and to retrieve them on demand. In other words, the system must first gain a degree of control over its own outputs before it can create a lexicon or construct a grammatical framework governing the use of such a lexicon.

This is critical from the viewpoint of cognitive theory, but one might still ask, given that an early motor adaptation was a logical necessity, couldn't it have occurred much earlier, perhaps in the australopithecines, and don't we still need language in some form to account for *Homo erectus*? I argue that a revolution in nonverbal motor skill would have had immediate and very major consequences in the realms of representation and social expression. These alone, without any further evolution, can account for the kinds of skills that have been documented in the culture of *Homo erectus*; they can also account for many of the nonverbal skills of modern humans.

There is another good reason for asserting the primacy of motor evolution: language is not the only uniquely human attribute that must be explained in an account of cognitive evolution (cf. Premack 1987). A good theory of the first cognitive evolutionary steps of humans should try

to account for as many human nonverbal skills as possible. This leads to the first proposal of my theory: the first major cognitive transition broke the hold of the environment on hominid motor behavior and provided hominids with a new means of representing reality. The form of the adaptation was a revolutionary, supramodal improvement in motor control called "mimetic skill."

**(b) Mimetic action as a unified supramodal system.**

Mimetic action is basically a talent for using the whole body as a communication device, for translating event perceptions into action. Its underlying modeling principle is perceptual metaphor; thus, it might also be called action-metaphor. It is the most basic human thought-skill, and remains fundamentally independent of our truly linguistic modes of representation. Mimesis is based in a memory system that can rehearse and refine movement voluntarily and systematically, guided by a perceptual model of the body in its surrounding environment, and it can store and retrieve the products of that rehearsal. It is based on an abstract "model of models" that allows any voluntary action of the body to be stopped, replayed, and edited under conscious control. This is inherently a voluntary access route to memory, since the product of the model is an *implementable self-image*.

The principle of voluntary retrievability, which might be called "autocueing," was thus established at the top end of the motor system. Autocueing is perhaps the most critical unifying feature of mimetic skill. Only humans can recall memories at will; and the most basic form of human recall is the self-triggered rehearsal of action, the refinement of action by purposive repetition. Purposive rehearsal reveals the presence of a unified self-modeling process, and most important, the whole body becomes a potential source of conscious representation. Retrievable body-memories were thus the first true representations, and also the most basic form of reflection, since the mimetic motor act itself represents something: systematic rehearsal "refers" to the rehearsed act itself, comparing each exemplar with a sort of idealized version of itself.

The human mimetic mechanism is supramodal at the output: that is, it can employ any part of the skeletal musculature in constructing a representation. It is supramodal at the input as well, since it can also utilize input from any major sense modality or perceptual system for its modeling purposes. A mimetic reenactment of an event – say a toolmaking sequence – might use the eyes, hands, feet, posture, locomotion, voice, or any combination of these, and the event itself might have been experienced through a variety of sensory modalities. Moreover, a given event can be mimetically represented in various acted-out versions. It follows that a mimetic act is a manifestation of a highly abstract modeling process.

The existence of a unified central "controller" for body mimesis is revealed most clearly in the unique human propensity for rhythm. Humans seem unable to resist rhythm; and even very young children spontaneously imitate, rehearse, and modify the rhythmic sounds and actions around them, with varying degrees of sophistication. The transferability of rhythm to virtually any skeleto-muscular system in the body reveals the abstractness of human mimetic action-modeling: rhythms can be transferred from one muscle group to another, singly or in combination. For example, a sound rhythm initially mod-

eled by the fingers can be transferred to the feet, or to the axial locomotor systems (as it is in dance), or to the head, face, eyes, tongue, voice; or to any subset of these in combination. Rhythm is thus an excellent paradigm for mimetic skill, in which an abstract perceptual event (usually a temporal pattern of sound) is "modeled" by the motor system.

Note that this modeling process relies on a principle of resemblance by which some property abstracted from sound is reproduced in motion; but these "resemblances" can be very indirect and elaborate, and innovation and mimetic "wit" are evident in more sophisticated human rhythmic games. Thus the modeling process is metaphorical or holistic: many variants of the basic rhythm may meet the criterion of resemblance, and the rhythm itself is not easily reduced to digital or discrete units combined according to "rules"; rather, it is the Gestalt, or overall pattern, that is primary.

Human mimetic capacity extends to larger time scales; it extends to the purposive sequencing of larger chunks of body movement over much longer periods of time. This assumes an extended mimetic imagination capable of imagining a series of actions in environmental context. If hominids could visually track and "parse" a complex event, as well as apes, say, then given the location of the mimetic controller at the top end of the event-perception system they should have been able to reenact complex events once large-scale action-modeling was within the capacity of the motor system.

**(c) Sociocultural implications of mimetic action.** An improvement of this magnitude in primate motor skill would inevitably have resulted in changes to hominid patterns of social expression. Existing repertoires of expressions would have become raw material for this new motor-modeling mechanism. By "parachuting" a supramodal device like mimesis on top of the primate motor hierarchy, previously stereotyped emotional expressions would become rehearsable, refinable, and employable in intentional communication. This would allow a dramatic increase in the variability of facial, vocal, and whole-body expressions as well as in the range of potential interactive scenarios between pairs of individuals or within larger groups of hominids. It is important to note that since a supramodal mimetic capacity would have extended to the existing vocal repertoire, it would have increased selection pressure for the early improvement of mimetic vocalization, a skill whose modern residue in speech is known as prosody.

Given a mechanism for intentional rehearsal and refinement, constructional and instrumental skills would also have moved to another plane of complexity through sharing and cultural diffusion. Improved toolmaking was in many ways the most notable achievement of *Homo erectus*, but it is important to realize that the manufacture of a new kind of tool implies a perceived need for that tool and corresponding advances in both tool use and pedagogy. Mimetic skill would have enabled widespread diffusion of new applications as well as supporting the underlying praxic innovations that led to new applications.

In addition to toolmaking and emotional expression, motor mimesis would have allowed some degree of quasi-symbolic communication, in that it would have allowed hominids to create a very simple shared semantic envi-

ronment. The "meaning" of mimed versions of perceptual events is transparent to anyone possessing the same event-perception capabilities as the actor; thus, mimetic representations can be shared and constitute a cognitive mechanism for creating unique communal sets of representations. The shared expressive and social ramifications of mimetic capacity thus follow with the same inevitability as improved constructive skill. As the whole body becomes a potential tool for expression, a variety of new possibilities enters the social arena: complex games, extended competition, pedagogy through directed imitation (with a concomitant differentiation of social roles), a subtler and more complex array of facial and vocal expressions, and public action-metaphor such as intentional group displays of aggression, solidarity, joy, fear, and sorrow. These would perhaps have constituted the first social "customs," and the basis of the first truly distinctive hominid cultures. This kind of mimetically transmitted custom still forms the background social "theater" that supports and structures group behavior in modern humans.

Greater differentiation of social roles would also have been made possible by mimetic skill. The emergence of mimetic skill would have amplified the existing range of differences between individuals (and groups) in realms such as social manipulation, fighting and physical dominance in general, toolmaking, tool use, group bonding and loyalty, pedagogical skill, mating behavior, and emotional control. This would have complicated social life, placing increased memory demands on the individual; but these communication tools would also have created a much-increased capacity for social coordination, which was probably necessary for a culture capable of moving a seasonal base camp or pursuing a long hunt.

It is important to consider the question of the durability of a hominid society equipped with mimetic skill: adaptations would not endure if they did not result in a stable survival strategy for a species over the long run. Mimesis would have provided obvious benefits, allowing hominids to expand their territory, extend their potential sources of food, and respond more effectively as a group to dangers and threats. But it may also have introduced some destabilizing elements, especially by amplifying both the opportunities for competition and the potential social rewards of competitive success in hominid culture.

**(d) The dissociability of mimetic skill in modern humans.** The neuropsychological dissociability of mimesis can be demonstrated from neuropsychological studies of modern humans with brain injury. Certain paroxysmal aphasics manifest a unified, coherent strategy for dealing with reality that has the properties of a purely mimetic strategy. Their cognitions have a style that is often (I believe simplistically) termed a "right-hemisphere" strategy; it shows a degree of unity and a complete independence from language that must be explained.

One well-documented case, Brother John (Lecours & Joannette 1980), suffered from seizures lasting as long as ten or eleven hours, during which all aspects of language – including inner speech – were "excised" from his mind. Nevertheless, he remained fully conscious, able to find his way around, able to operate an elevator or a radio (he used the news station on the radio to test whether he was regaining speech comprehension), and capable of com-

municating with gesture and mime. Most important, he retained perfect episodic recall for most of these seizures; he could remember what went on during the spell, including who entered and left the room and what he had done with his time. This implies that neither his formation of retrievable episodic memories nor his subsequent retrieval of them could have depended upon having a functional language system at the time of storage. Nor could his functioning mimetic skills have depended on language. There are other neurological syndromes that produce a somewhat similar profile – some cases of global aphasia, for instance – but most patients suffer from other disabilities as well as permanent impairments and this makes clear distinctions between language and non-language symptoms difficult to derive. The uniqueness of paroxysmal cases lies in their lack of nonlanguage symptoms such as apraxia, agnosia, amnesia or dementia, and their ability to return to normal after the seizure.

Further evidence comes from documented histories of deaf-mute people raised in hearing communities without formal sign language training. Such individuals could have had none of the lexical, syntactic, or morphological features normally associated with language. They obviously lacked a sound-based lexicon of words; they couldn't read or write, and had no access to a community of other deaf individuals who signed, and thus also lacked a visually based lexicon. Yet, by some accounts (e.g., Lane 1984) such individuals retained a capacity for all aspects of what I have identified as mimetic cognition: a full range of human emotional expressivity, gesture, mime, dance, athletic and constructional skills, and an ability to participate in reciprocal mimetic games.

The persistence of mimetic skill is evident in modern society. In fact, the realm of mimetic representation is still relatively autonomous from that of language and remains essential to the training of those who work with the body, such as actors or athletes, as well as to those who practice traditional constructional skills, such as arts and crafts. It is central to human social effectiveness and to the practice and teaching of games, competitive skills, and many group expressive customs, as for instance in the intentional use of group laughter as punishment, or the signaling of deference, affection, manliness, celebration, and grief; or the maintenance of group solidarity (see, for instance Argyle 1975; Ekman et al. 1969; Eibl-Eibesfeldt 1989). [See also Eibes-Eibesfeldt: "Human Ethology" *BBS* 2(1) 1979.]

**(e) Mimesis as a preadaptation for language.** Mimetic skill was, fortuitously, an important preadaptation for the later evolution of language. It allowed hominid tool technology and social organization and the shared realm of custom and expression to become more complex. Given the inherent fuzziness and ambiguity of mimetic representation, it would eventually have reached a level of complexity where a method of disambiguating intended mimetic messages would have had immediate adaptive benefits. Thus it created conditions that would have favored a communication device of greater speed and power.

On a more fundamental level, however, the principle of self-triggered voluntary retrieval of representations had to be established in the brain before the highly complex motor acts of speech would have been possible. Phonetic

skill has been called "articulatory gesture" by various investigators (Brownman & Goldstein 1989); the whole higher apparatus of speech depends on the basically mimetic ability of individuals to create rehearable and retrievable vocal acts, usually in close connection with other mimetic acts. In a word, language per se is layered on top of a mimetically skilled phonological system.

Language is not confined to the phonological system, however, because mimesis is inherently supramodal; thus, when phonology malfunctions, other mimetic subsystems may be harnessed by the language system. This is particularly visible in Petitto and Marentette's (1991) elegant documentation of infant babbling in sign-language environments, which occurs at exactly the same time as phonological babbling and has the same properties. Deaf infants growing up in deaf-signing households showed themselves to be very good at miming the motor *principle* behind signing, if not the signs themselves; that is, their manual "babbling" reflected the conditional probabilities of their expressive environments on a purely mimetic level. This is exactly what babbling infants do in hearing households: they model, in their actions, one of the most obvious dimensions of motor behavior to be observed in their families: repetitive, and to the infant, apparently random production of phonological acts.

Babbling, whether oral or manual, is reference-free in the linguistic sense – that is, it has no linguistic meaning – but it is nevertheless truly representational, in that babbling patterns are (eventually) excellent motor models of the expressive patterns the infants observe around them. They reproduce not only the elementary units of language, but also the larger mimetic envelope of expression as well: for example, prosody, and the habit of alternation, or "waiting one's turn" in expressive exchanges. Since babbling is free of linguistic reference, the brain mechanism that supports it does not have to be linked to language per se; rather, these eight- to ten-month-old infants look very much like good supramodal mime artists. And the supramodal nature of their babbling is very revealing: the fact that babbling isn't confined to phonology suggests that a supramodal mimetic adaptation evolved first, with phonology developing later as a specialized subsystem of mimetic capacity.

There are other theories that view early advances in praxic skill as preadaptations for language (Corballis & Beale 1976; Kimura 1976). Kimura observed that oral and manual apraxia and aphasia often result from the same left-sided lesions; from this she inferred that language and voluntary movement control are linked, possibly to a common processor. However, the neuropsychological case for linking pantomime and language to the same left-sided serial processor has since disintegrated (see Poizner et al. 1987; also Square-Storer et al. 1990).

These authors did not provide any theoretical justification of why praxis should have been an essential preadaptation for language, but Corballis's more recent (1989; 1991) hypothesis faces this problem squarely. His idea is that the left hemisphere acquired a general-purpose capacity for "generativity" that served as the common substrate for image generation and praxis and later for language. Generativity requires categorical perception (Harnad 1987), or the decomposition of the object world into elementary units; and it also requires the ability to recombine these units, as in both phonology and image

generation (Kosslyn 1988). In Corballis's view, these two aspects of generativity evolved for improved praxis, forming a preadaptation for the later emergence of language. A closely related theory has been proposed by Greenfield (1991), who argues for a common left-sided mechanism for combinatory praxis and phonology, at a prelinguistic level.

The concept of mimetic skill proposed here differs fundamentally from both Corballis's idea of generativity and Greenfield's left-sided praxic "module" in its reduced emphasis on cerebral laterality; as pointed out at some length in my book, mimetic skill is probably bilateral (which is not to say that it is symmetrical) in distribution. More important, it differs in the nature of the proposed underlying mechanism. Generative praxis is conceived of as categorical, rule-governed, and serial in its manner of operation, whereas mimesis is basically a holistic or analog system that can model over time as well as space. A capacity for serially recombining categorical units would not easily account for the complex, fuzzy, holistic process of comparing movements against their idealized versions (cf. Moerck 1989), or of producing event reenactments, as in charades or pantomime. The generative modeling of the mimetic action-patterns that humans create and refine (including phonology) seems far too metaphorical and analog in principle to fit easily into this kind of quasisymbolic computational framework.

### 3. Second transition: Lexical invention

The rationale for the second transition is, briefly, as follows: (a) since no linguistic environment yet existed, a move toward language would have depended primarily on developing a capacity for *lexical invention*; (b) phonological evolution was accelerated by the emergence of this general capacity for lexical invention, and included a whole complex of special neuronal and anatomical modifications for speech; (c) the language system evolved as an extension of lexical skill and gradually extended to the labeling of relationships between words and also to the imposition of more and more complex metalinguistic skills that govern the uses of words; and (d) the natural collective product of language was narrative thought (essentially, storytelling), which evolved for specific social purposes and serves basically similar purposes in modern society.

**(a) Lexical invention.** Lexical invention is not yet understood in terms of mechanism. There is no viable computational model of this process and neural network models have not yet reached the point where anything so complex could be simulated. The process mapping the "lemma" or meaning-based side of the lexicon onto the form of the symbol – whether it is phonological or manual – involves much more than the association of a discrete signifier, or form, with a discrete meaning. The previous section argues that phonology, like any mimetic system, works according to a metaphorical principle; but so does lexical invention, if Wittgenstein (1992) or Johnson-Laird (1983) are to be believed. In other words, both word forms and meanings tend to be fuzzy, and neither side in the lexical entry is cleanly defined or discrete. Nevertheless, the tension between word form and meaning is a creative one

that greatly increases the range of things that can be represented.

As discussed at some length in the book, the invention of a symbol is a complex process that involves labeling and differentiating our perceptions and conceptions of the world, including other symbols as parts of that world. Form is mapped onto meaning, but meaning is *defined* by that same process, in a reciprocal tension. This reciprocal tension is evident even now, after at least 45,000 years of lexical invention. Languages are constantly changing their particular mappings of form onto meaning; for instance, all of the tremendously diverse aboriginal American languages derived from three root Asiatic languages within the past 15,000 years (Greenberg & Ruhlen 1992); and the entire Indo-European group of languages, including language groups as diverse as Sanskrit, Gaelic, Latin, and Greek, have all evolved from a common ancestor within the past 7,000 years (Renfrew 1989). This incessant pattern of change suggests that the driving force behind lexical invention – the need to define and redefine our maps of meaning onto word forms – is more fundamental and considerably less rigid than the specific forms and rules of language at any given moment.

**(b) The phonological adaptation.** Phonology was not *the* primary language adaptation, but rather a specialized mimetic subsystem that supported the primary adaptation, lexical invention. The specialized anatomical subsystem that supports phonology evolved after the evolution of a supramodal lexical capacity, or more properly perhaps, concurrently with it, in a mutually reinforcing manner. As a mimetic subsystem, phonology has the same basic properties as all mimetic action, such as rehearsability, autocueing, and purposive refinement. The fact that language can be offloaded to other motor modalities, as it is in the sign languages of the deaf, is evidence of the secondary position of phonology in the evolutionary chronology. Phonology by itself could not have created a lexicon, and without lexical invention it is doubtful whether humans would have been subjected to selection pressures favoring such a powerful phonological system.

Nevertheless, it was a very complex and important adaptation, and without it, archaic sapient humans may not have been able to sustain the expansion of lexical capacity that they subsequently did. The great survival value of phonology to archaic humans is evident in the fact that it evolved despite the great respiratory dangers associated with a descended larynx, and in the sheer anatomical complexity of the adaptation. Included in the phonological adaptation were (as a minimum): the descent of the human larynx and the redesign of the supralaryngeal vocal tract, with corresponding central motor programming devices; a specialized auditory device for achieving improved auditory object constancy, which feeds back directly onto speech production; the articulatory loop, for immediate literal recall of articulated messages; and a specialized, large-capacity auditory memory system of word forms (see Levelt 1989; Lieberman 1975; 1984).

The importance of phonology should not be underestimated. There is little alternative to the notion that the original form of language is spoken language. There is an easy relationship between vocalizing and language, per-

haps because phonology is fast, highly portable, and less likely to interfere with locomotion or praxis: in this sense it is a special "channel" of communication that can float freely above the largely visuomotor world of events, constructing commentaries unimpeded. Moreover, it works at a distance and in the dark, two features that have great adaptive value.

Phonology also has the special virtue of being able to generate a virtually infinite number of easily retrievable sound patterns for symbolic use. Human retrieval capacity for oral words is extraordinary; we carry around tens of thousands, and in the case of some multilinguals, hundreds of thousands of words; most other species, from bees to the Great Apes, seem to be limited to a few dozen expressions at most in the wild; and this limitation even applies to Cheney and Seyfarth's (1990; see also multiple review: *BBS* 15(1) 1992) vervet monkeys.

The natural dominance of phonology over manual signing is evident in experimental settings where subjects are encouraged to tell stories about specific experiences and their gestures are videotaped. In such experiments (cf. McNeill 1985), facial and manual gesture fall into a secondary support role, their timing ruled to the millisecond by spoken words; even the mimetic dimension of voice, prosody, remains secondary to the expressed meaning. Phonology is thus clearly the medium of choice for language itself. It should be added, however, that this pattern of dominance is often broken, especially in humor (including the humor of children), where the semantic counterpoint between what is said and what is gestured or done can become a powerful means of expression in itself. The ease with which humans can parallel-process two contradictory messages – mimetic and linguistic – has been long known to playwrights.

It is important to note that these new representational acts – speech and mimesis – can be performed covertly as well as overtly. Covert speech has been called "inner speech" by Baddeley (1986), who considers it to be equivalent to the activation of the central aspects of articulation, without actual motor execution. The mental operation we call "imagination" can similarly be seen as mimesis without motor execution of the imagined acts. The control of mimetic imagination (probably even of visual generative imagery, which is facilitated by imagined self-movement) presumably lies in a special form of kinematic imagery. Autoretrievability is just as crucial for covert imaginative or linguistic thought as it is for the overt, or acted-out equivalent. Thus, given a lexicon, the human mind became able to self-trigger recall from memory in two ways: by means of mimetic imagination and by the use of word symbols, either of which could be overt or covert.

**(c) Grammar and metalinguistic skill.** I have opted for a lexically driven, rather diffuse model of language evolution partly because it fits in well with the preceding evolutionary scenario and partly because I judge this to be a scientific "best bet" on the basis of an extensive review of neurolinguistic research. The main issue here is whether grammar and metalinguistic skills such as those which operate at the level of discourse and logic require a separate adaptation in addition to phonology and lexical invention. If one were to try to envisage language in such a way as to meet all of Fodor's (1983) requirements for a

true linguistic "module," a separate grammar module would surely fail on a number of counts (as Fodor has acknowledged), especially inasmuch as it appears to be completely interpenetrable with the rest of language, and closely tied to semantics.

Moreover, the neurological case for a separate grammar module is weakened by recent cross-linguistic studies of aphasia, which strongly suggest that there is no specific brain lesion, nor any specific pattern of grammatical deficit, that is universally found in agrammatics of all languages. According to the "competition" model proposed by Bates and MacWhinney (1987), the whole perisylvian region of the left hemisphere is diffusely dedicated to language, function words and grammatical rules being stored in the same tissue as other kinds and aspects of lexical entries. However, I readily admit that this issue, like many others in this field, is still not conclusively resolved; there is electrophysiological evidence that function words – those most relevant to grammar – might have a different cerebral representation from open-class words (Neville 1992).

A related issue is whether it seems necessary a priori to posit a separate adaptation for the invention and transmission of grammar and the metalinguistic skills that support extended discourse. I have cited some biographical accounts of symbolic invention in mathematics as examples of how difficult it is to invent any new word or symbol. Once a concept has been "captured" lexically for the first time, it seems to become much easier to transmit it to others, but its original invention is generally difficult; and every new invention must then be subjected to the shared linguistic market for acceptance or rejection. All original lexical invention is difficult and collective. This applies to all classes of words, and there is no compelling reason why closed-class, or function, words might not be viewed as a product of the same skill that enabled the invention of nouns and adjectives. If the lexical inventor can isolate and define an abstract concept like "run" (including the exclusion rules for its correct use), it is not clear why that same mind could not isolate and define a function word like "with." Grammatical concepts do not seem to demand special treatment in their invention and, presumably, their transmission.

It would certainly make good evolutionary sense to attribute language to one core adaptation whose further evolution could account for all the attributes of language and language-based thought. Part of the reason is, simply, time; there wasn't enough time in the human story for more radical cognitive adaptations, and a capacity for lexical invention is the obvious *sine qua non* of language, and hence must be put first. If we put lexical invention earlier in the scenario, as Bickerton (1990) did, there might have been time for a separate adaptation for grammar; but then we must explain why a powerful capacity like lexical invention would have evolved when it was apparently not needed 1.5 million years ago, and then failed to develop further for over a million years. If we put lexical invention late, as I and many others do, there doesn't seem to be enough time to allow for a second adaptation for grammar.

**(d) Sociocultural ramifications.** Spoken language increased the number and complexity of available words and grammars and altered human culture by introducing



a new level of shared representation. My hypothesis is that mimetic skill continued (and continues) to serve its traditional social purposes perfectly well: it still provides the cognitive foundation for institutions like dance, athletics, craft, ritual, and theater. Oral language initially carved out its own sphere of influence within mimetic culture, eventually assuming a dominant and governing role in human culture, but never eliminating our basic dependence on mimetic expression. Oral language remains focused on the human world, particularly on relationships (Dunbar 1993), and this pattern extends to a wide range of cultures, from technologically primitive hunters and gatherers to highly urbanized modern European societies.

The natural product of language is narrative thought; in this sense, language, like mimesis, evolved primarily as a method of modeling reality. Dunbar (1993) has argued that the normal social use of language is storytelling about other people – gossip – and he has produced observational data to prove this. But day-to-day storytelling in a shared oral culture eventually produces collective, standardized narrative versions of reality, particularly of past events; and these become what we call the dominant “myths” of a society. It is interesting that all documented human societies, even the most technologically primitive ones, have elaborate systems of myth, which appears to reflect the earliest form of integrative thought. These socially pervasive constructs continue to exert a major influence on the way oral societies – and indeed most modern societies – are run: thus I have suggested that many cultures might be labeled “mythic,” after their governing representations.

#### 4. Third transition: The externalization of memory

The case for a third cognitive transition is based on arguments, partly structural and partly chronological, that are similar in principle to those used for the first two; but the physical factors that supported the third transition are a little different, inasmuch as the latter was driven primarily by technological rather than biological developments. The chronological evidence is based on the rapid emergence of whole new classes of memory representations – external memory records – as well as a major change in the types of symbolic artifacts produced by humans. The structural argument is based partly on neurophysiological and neuropsychological evidence bearing on localization and plasticity and partly on an analysis of cognitive architecture in the context of our new relationship with external memory.

The historical case for a third transition rests on evidence that since the Upper Paleolithic humans have gradually developed three new representational devices. The first was *visuosymbolic invention*, which advanced through various well-documented stages, culminating in a variety of complex graphic and numerical conventions and writing systems. The second was *external memory*, which evolved to the point where external memory records, mediated by a “literate” class, started to play a governing role. The third was the emergence of very large, externally nested cultural products called *theories*.

I will not reiterate the voluminous historical evidence for this, partly for reasons of space, but mostly because my chronology is neither original nor in dispute. The real argument for grouping together these historical trends into a so-called third transition is a structural one.

The structural case can be stated as follows: (a) external memory has introduced radical new properties into the collective storage and retrieval systems of humans; (b) the use of these external storage systems is difficult and requires a major redeployment of cerebral resources toward establishing literacy-related “modules” in the brain; (c) the physiological basis for this reorganization probably lies in neuronal epigenesis and plasticity; and (d) the role of biological working memory has been changed by the heavy use of external memory.

**(a) New properties.** Early humans, like their primate predecessors, depended heavily on their natural or biological memory capacities. Even though mimetic skill and language enabled humans to create a shared representational culture, the actual physical storage of that collective knowledge depended on individual memory. Thought was dependent on biological working memory, and whatever was seen or heard had to be remembered and rehearsed either in imagination or in speech. The contents of our long-term store were accessible only by means of the limited associative strategies available to biological memory, such as similarity and contiguity; thus, the need for oral mnemonics, extensive literal oral recitation, and a dependence on specialized individuals, like shamans, to preserve particularly important memory material.

The advantages of external memory are easily documented. External symbolic storage systems allow humans to circumvent, at least partially, the limitations of biological working memory, while creating a wide range of new storage, retrieval, and processing possibilities. By changing the physical medium of storage, human memory systems have acquired new properties, especially retrieval properties. I have suggested the term “exogram” to complement the notion of a biological “engram.” As shown in Table 1, exograms introduced new possibilities into the human representational universe.

Exographic storage constitutes a hardware change just as real as the biological hardware changes that mediated the first two transitions; and its effect on overall memory structure may have been even greater. The exportation of memory storage has literally meant that the human race, as a collectivity, can now evolve new memory systems at the accelerated rate of technological change, as opposed to the relatively slow rate of genetic change. Perhaps the most important new features introduced by external storage are radically different options in memory retrieval, and the fact that exograms are easily reformattable. Extensive reformatting can modify the kinds of ideas and images that are available to store in biological memory, and so on, in iterative loops. This iterative crafting of complex memory records has produced completely new types of symbolic representations that had no equivalents in preliterate oral cultures – examples might include the servicing manuals for a rocket engine, the equations proving the Pythagorean theorem, a corporate income tax handbook, a heat-map of the troposphere, or the libretto and score for *Eugene Onegin*.

Table 1. *Some properties of engrams and exograms*

Engrams	Exograms
internal memory record fixed physical medium constrained format	external memory record virtually unlimited media unconstrained and reformat- table
impermanent large but limited capacity limited size of single entries	may be permanent virtually unlimited virtually unlimited
not easily refined	unlimited iterative refine- ment
retrieval paths constrained	retrieval paths uncon- strained
limited perceptual access in audition, virtually none in vision	unlimited perceptual access, especially in vision; spa- tial structure useful as an organizational device

Source: From Donald, M. (1991, p. 315).

**(b) Cognitive reorganization.** External memory has introduced new cognitive skill-clusters that are generally referred to as "literacy" skills, but full symbolic literacy extends well beyond the traditional Western definition of literacy, that is, alphabetic reading competence. The neuropsychology of various acquired dyslexias, dysgraphias, and acalculias has revealed a cluster of functionally dissociable cognitive "modules" in the brain that are necessary to support these skills (see, for instance, Morton 1984; Shallice 1988 [see also multiple book review: *BBS* 14(3) 1991]; Shallice & Warrington 1980).

The localization of these neural modules seems to vary across individuals, as might be expected, since the whole structure must have been imposed by cultural programming rather than by any specific genetic predisposition built into the nervous system. There is a great deal of evidence in single-case neurological histories that these "literacy support networks" are anatomically and functionally distinct from those that support oral-linguistic skills, as well as from those brain regions that support basic perceptual and motor functions (see especially Shallice 1988).

There are at least three dissociable high-level visual interpretative paths involved in symbolic literacy. The most basic is "pictorial," and is needed to interpret pictorial symbols such as pictograms and visual metaphors; even at this level there are numerous interpretative (mostly metaphoric) conventions to master. The second is "ideographic," and is sometimes called the direct visual-semantic path in studies of reading (see Coltheart et al. 1980; Paradis et al. 1985); it maps visual symbols directly onto ideas, as in the case of Chinese ideographic writing, most systems of counting, or many street signs and analog graphic devices like maps, histograms, and charts. The third is "phonetic," and serves to map graphemes onto phonemes, as in alphabetic print. These three paths emerged at different historical phases of visuosymbolic evolution and remain functionally independent of one

another; moreover, each path feeds into a distinct visual "lexicon" of thousands of recognizable symbols.

**(c) Physical basis.** How could the highly complex functional subsystems necessary for reading, writing, and other visuosymbolic processing skills be accommodated by the human brain without genetic change? The answer seems to lie in the increased neocortical plasticity that came with the final expansion of the human brain. This increase in plasticity might be partly a function of greater cortical asymmetry, which allows nonredundancies of function between homologous association regions in the two hemispheres, in effect creating twice as much "extra" neuronal space as a comparable expansion in primary cortical regions, which tend to be more symmetrical in function. Moreover, the immense tertiary neocortical subregions of the human brain have so many competing input pathways that epigenetic factors such as those described by Changeux (1985) and Edelman (1987) could create a very great range of potential functional arrangements. In effect, it is probably because of the plasticity of this arrangement that the human brain can invest so heavily in the decoding baggage needed for using large numbers of novel external memory devices.

In addition, there is evidence that even in adults the cerebral cortex is constantly readjusting and fine-tuning its assignment of processing space, reflecting the constantly changing use patterns imposed by the environment. The somatosensory regions of neocortex may be reorganized by a prolonged increase in stimulation; in fact, the area dedicated to fine touch discrimination expands and contracts in response to imposed load changes (see, for instance, Merzenich et al. 1987). This sensitivity to use pattern may even extend to functions quite different from those that normally occupy a given region, as in the case of the auditory cortex of a congenitally deaf person, which in the absence of auditory stimulation will eventually assume visual functions (Neville 1990). If the relatively hard-wired primary sensory regions are this flexible then tertiary cortical regions ought to show even greater flexibility in their function, given the additional degrees of freedom added by moving two synapses or more from the many sensory, motor, and association regions that impinge upon them.

There appear to be tradeoffs inherent in this flexible arrangement – that is, "invasions" of a given region by an environmentally or culturally driven function will displace other functions that may potentially have depended on that region. This suggests that high levels of literacy skill may entail considerable costs, as indeed has been suggested by the literature comparing the cognitive competences of oral cultures with those of literate ones. Oral memory and visual imagery are often listed among the skills that may have been traded off against literacy (Cole et al. 1971; Richardson 1969).

**(d) Changed role of biological memory.** One of the most interesting effects of external memory devices is the way they alter human working memory. Working memory is generally conceived as a system centered on consciousness; and although there are many alternatives in the literature, Baddeley's (1986) model was adopted for the purposes of this discussion because it is fairly representative and maps very well onto a neurobiological model.

The tripartite working memory structure proposed by Baddeley includes two slave memory systems, the articulatory loop and the visual-spatial sketchpad, and a central executive. According to this model, when we think, we either imagine, via the sketchpad, or verbalize, via the articulatory loop (the latter may be covert, in the form of "inner speech"). The central executive handles the intermediate-term semantic context – for instance in a conversation it might keep track of what was said, by whom, and in what context. This working memory structure provides the basis for consciousness, although not everything held in working memory is consciously experienced; rather, it is easily available to consciousness.

In preliterate cultures this arrangement, or something close to it, was all that humans had to work with, and its limitations are well documented. A society that relies on this type of memory mechanism would accordingly have to depend upon a variety of social arrangements and mnemonic skills to maintain its accumulating knowledge base: rote verbal recitation, preferably in groups; specialized individuals whose task was to learn and retain knowledge (for instance, shamans and bards); formulaic recital by individuals in an undisturbed, special place; rigidly formal and repetitive group ritual; and various forms of visual imagination as a means of understanding and retaining quite complex memories.

This situation has changed with the increased use of external symbolic storage. The larger architecture within which the individual mind works has changed; in fact, the structure of internal memory is now reflected in the external environment: there is now an external memory field that serves as the real "working memory" for many mental operations, and there is also an external "long-term" store. The external equivalent of the long-term store has very different storage and retrieval properties from those of our internal long-term store. Similarly, the external working memory field has completely different properties from the internal working memory system. Whenever an individual is "plugged in" to some part of the external store, that interaction is mediated by certain items displayed in the external memory field; the latter may consist of a variety of display devices, including print, graphs, monitors, and so on, usually arranged in visual space. The conscious mind is thus juxtaposed between two memory structures, one internal and the other external.

The external display projects directly to the visual regions, which now become the locus of a new kind of internal working memory, one which utilizes the power of the perceptual systems. In effect, because the perceptual systems are displaying *representations* (as opposed to nonsymbolic objects), the user's brain can move through "information space" just as it has always moved through the natural environment, with the difference that processing occurs on two levels, instead of only one. The items displayed in the external memory field are treated first as natural objects and events and second as memory representations that can externally program the user's brain, that is, create specific states of knowledge that were intended by the creator of the particular external device on display.

This second level of analysis, which is the prerequisite for literacy, imposes a great load on visual as well as semantic processing. The process of reading a book,

where meanings literally pop out of the script (or the graphs, numbers, ideograms, or other types of symbols it uses) requires a tremendous amount of additional high-level processing. This second level of processing, wholly automated in the expert reader, requires rapid access to thousands of internally stored pictorial, ideographic, graphemic, and visual-lexical codes, along with various specialized grammars, scanning conventions, and a great deal of interpretative knowledge. In effect, this second-level visual system produces knowledge states that are directly driven from the external memory field; it thus becomes the internal display device for a very complex external memory trace. The literate brain thus becomes externally programmable.

But unlike the constantly moving and fading contents of biological working memory, the contents of this externally driven processor can be frozen in time, reviewed, refined, and reformatted. Moreover, all of this can be done intentionally, online, and in real time, in constant interaction with the external display mechanism. In biological working memory, the possibility of this kind of iterative refinement of mental representations is very limited. Neither of Baddeley's (1986) slave systems can support such reflection: the articulatory loop needs constant rehearsal and has a decay time of a few seconds, whereas the visual sketchpad is even more ephemeral, vaguely defined and vulnerable to interference. The central executive is able to hold quite a large amount of information, but in order to consciously modify that information, its contents apparently need to be displayed in one of the slave systems, usually in a covert manner (for instance, inner speech). This imposes a serious limitation on the amount of conscious reflection that can be done on any material that is stored exclusively in biological working memory.

Breaking out of this limited working memory arrangement in itself was a very major change. But it potentiated another important new development: new metalinguistic skills, which expressed themselves in the kinds of symbolic products and cognitive artifacts (Norman 1990) humans could produce and maintain. Producing a single new entry in the external storage system is not a trivial occupation; it never has been, from Ice Age cave paintings to modern science. As artifacts have become much more complex, and the knowledge environment itself has grown, the specific skills needed to become a serious "player" has also taken much more specific preparation, in the form of extended apprenticeships and higher education. There is a trend in the kinds of "metalinguistic" thought skills that have been taught in Western academies over the past few thousand years, moving from an early emphasis on oral and narrative skills, toward visuosymbolic and paradigmatic skills. Denny (1991) has suggested that the major new thought pattern attributable to literacy is a property called "decontextualization," and Olson (1991) has suggested that writing allowed the "objectification" of language, and consequently the development of formal thought skills. These proposals are compatible with my suggestion that literacy allowed the thought process itself to be subjected to iterative refinement through its stable display in the external memory field, and its subsequent incremental refinement, like any other external symbolic product.

The modern brain must accommodate not only these

new working memory arrangements and all the coding demands imposed by symbolic literacy, but also a number of metalinguistic skills that simply did not exist a few thousands of years ago. The latter are socially entrenched; for example, an enterprise like modern science is very much a collective endeavor, in which the individual mind is essentially a node in a larger networked structure supported by external memory. Humans have been part of a collective knowledge enterprise ever since mimetic skill permitted us to break with the limitations of episodic cognition, but external memory has amplified the number and variety of representations available in human culture and increased the degree to which our minds share representations and rely on external devices for the process of thought itself. Cognitive studies of the modern workplace (e.g., Hutchins 1990; Olson & Olson 1991; Suchman 1987) testify to the way that electronically distributed knowledge representation, and computer-coordinated planning and problem-solving, are affecting the relative roles of individual minds and external memory devices in this collective enterprise.

## Open Peer Commentary

*Commentary submitted by the qualified professional readership of this journal will be considered for publication in a later issue as Continuing Commentary on this article. Integrative overviews and syntheses are especially encouraged. All page references are to Donald's Origins of the Modern Mind unless otherwise indicated.*

### From cooperative computation to man/machine symbiosis

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In an early target article in *BBS*, Arbib and Caplan (1979) offered a view of neurolinguistics that is in many ways concordant with that offered by Donald in Chapter 3, "Wernicke's Machine." However, where Donald turns to Fodor's (1983) "modules" – which are so large that "language" occupies a single module – we offered a finer-grained analysis that uses the "cooperative computation" of interacting entities called schemas to reveal the salient patterns of interaction embedded in the neural network dynamics of the brain. This intermediate-grain approach to modularity (in the general sense that antedates Fodor) not only encourages computational modeling and neuropsychological analysis of such Fodorian modules as language and vision but it also allows one to embark upon a similarly insightful analysis of the "central processes" that Fodor argues are unanalyzable (Arbib 1987).

Donald, although he uses Fodor's large-scale notion of modules, does take the argument further by analyzing the "central processes" in terms of evolutionary stages. To the basic mechanisms for exploiting episodic memory he adds (1) the "mimetic controller" (Fig. 6.1, p. 190), (2) the "linguistic controller" at the peak of a vertically integrated speech system (Fig. 7.2, p. 260), and (3) the metalinguistic skills which coevolve with the development of increasingly sophisticated "external memory fields."

Such "large-module" language leads Donald to characterize the second transition as "one vertically integrated adaptation, ultimately unified under a 'linguistic controller,'" (p. 259) even though elsewhere he reminds us that: "Just as in the case of mimesis, the language adaptation had to involve many different parts of the brain. . . . Once again we are looking at mosaic evolution" (p. 261). (One may note, for example, that a key point in the use of language is to negate a statement and then gather evidence as to which of the alternatives is true. Another is the flexible expression of goals and the ability to analyze various paths to attain them.)

Donald's argument may be strengthened if "schemas" are seen as the units on which evolution may act. Fodor's "modules" then disappear, to be replaced by patterns of schemas which provide a coherent style. As Arbib and Hesse (1986, p. 50) note, "Though processes of schema change may certainly affect only a few schemas at a time, such changes may 'cohere' to yield a mental 'phase transition' into a pattern of holistic organization. [Such transitions may include] stage changes in the sense of Piaget [and] paradigm changes in the sense of Kuhn."

To these examples I would here add "evolutionary transitions in the sense of Merlin Donald." This forces us to spell out more carefully a view of evolution as a form of punctuated equilibrium that goes somewhat as follows: Existing species have reached a local quasi-optimum of fitness in relation to their ecological niche. By quasi-optimum, I mean that the expected effect of a random mutation is a decrease or negligible change in fitness. Thus the species can remain stable for long periods of time until there is either (1) a catastrophic change in the environment so that species fitness is no longer optimal or (2) a very rare mutation does occur that yields a heritable improvement in fitness. The key point is that a successful mutation does not yield a new quasi-optimum. Rather, many different mutations can now effectively yield changes that increase fitness. The biological changes in both bodily and neural structure and function are manifested in new schemas which provide the "mental phenotypes" on which selection acts. We can then seek to see how these changes might be small enough in the genetic metric to be the plausible result of mutations, yet large enough in their functional expression to yield adaptive improvements that can build upon one another to yield a coherent pattern of change. (I would appreciate pointers to scholarly treatments that either support or refute this approach.) Such incremental changes in brain and body are not "unidirectional." Arbib (1989, sect. 7.2) gives a concrete account of the "evolution" of schemas in a computational model for visual motion perception that has strong resonances with a Jacksonian view of brain evolution (Jackson 1874; 1878/1879). This example stresses that "older systems" are not fully encapsulated but can themselves further evolve to take advantage of changes in the "informational environment" afforded by the new brain regions.

Donald's work thus poses two challenges: to understand why the transitions to each of episodic, mimetic, and mythic culture yielded relative stability after a multitude of coadaptive changes (over 50 to 100 generations?) had cohered to yield a new "species style"; and to understand why what Donald calls theoretic culture (marked by man/machine symbiosis; cf. Arbib 1973; 1982) is a stage of explosive cultural change. I do not know the answer, but I think it worth recalling the extension of schema theory that Hesse and I introduced to place "schemas in the head" (cf. stage changes in the sense of Piaget) and "schemas in society" (cf. paradigm changes in the sense of Kuhn) in an integrated perspective.

An "ideology" is expressed within the whole structure of interactions among the individuals of a society, their institutions, and their artifacts; it can only be vicariously and imperfectly represented within the head of any individual within that society. . . . we use the term "social schema" to denote any such network, whether an ideology, a language, or a religion. . . . Such schemas are not external, like the physical world, but they shape the development of our

individual schemas at least as powerfully as the patterns of physical reality shape the development of our sensorimotor schemas. (Arbib & Hesse 1986, p. 129)

Such ideas find computational expression in a model of language acquisition (Hill 1983) in which the child actively forms the schemas of language whether the child's experience is based on one speaker or a whole community. What is more to Donald's point, however, is that we discuss the conditions under which one individual will assimilate such a social schema while another will "rebel."

The point of the above orgy of self-citation is not to claim that my work anticipates Donald's. It does not, and I have learned much from his fascinating book. It is rather to suggest that schema theory may provide a framework in which the implications of Donald's ideas may be fruitfully elaborated into a form testable by the methods of computational and cognitive neuroscience. These methods themselves must be augmented by the tools of evolutionary theory if they are to fully meet this challenge.

## Putting cognitive carts before linguistic horses

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*Origins of the Modern Mind* gets off on the right foot by taking two steps seldom conjoined in approaches to human cognition. Merlin Donald accepts that cognition can only be understood from an evolutionary perspective, and sees the representational function of language as marking an essential stage in the development of mind. It is all the more disappointing that his book should be seriously marred by misunderstandings of the nature of language.

Take Donald's treatment of Brother John (pp. 86-89), an epileptic monk whose severer seizures temporarily deprived him of all language functions, but who could still cope with a variety of social situations. According to Donald, this means that language, despite the significance he accords it, cannot play a central role in cognition - otherwise Brother John would have been "confused" or "disoriented" by those situations. But what does Donald think happened, in these fits, to the knowledge of language underlying Brother John's normal performance? Did it vanish, to be magically reinstated once the fit was over? Surely it remained present throughout. And if language is indeed central to human cognition, then a temporary blockage of its peripheral output-input mechanisms would have no consequence for any automatic (and quite unconscious) computational processes in which it might be implicated. Brother John in seizure is not, contra Donald, a man without language - but a man without full use of language. That he could remember and verbally describe his experiences should have alerted Donald to inconsistencies in the description given.

But Donald himself gives conflicting accounts of Brother John's status. On p. 86, Brother John, even in his severest seizures, is "fully aware, with no breakdown of basic conscious functioning." On p. 253, however, he has "no symbols, no symbolic thought." How he could be conscious and aware without symbolic thought, or whether (and if so, how and why) his consciousness differed between conditions of seizure and normalcy, apart from mere changes in the ability to give synchronous verbal descriptions, are just two of the things Donald fails to explain.

Without a clear understanding of language and how it functions, further confusions inevitably arise. Consider the following passage:

The apparently impossible complexity of linguistic constructs at the level of word and sentence might well be secondary phenomena. The primary objects of language and speech are thematic; their most salient achievements are discourse and symbolic thought. Words and sentences, lexicons and grammars, would have become necessary evils, tools that had to be invented to achieve this higher representational goal. . . . Above all, language was a public, collective invention. (p. 216)

If ever there was a case of putting the cart before the horse, this is it. The claim that language was invented so as to achieve higher representational goals entails that such goals must somehow have been envisaged in advance of language - first you think it, then you dress it in words. But in that case, where did this advanced thinking come from, and why does it seem so different from the thinking of even our closest relatives?

Donald is rightly suspicious of brain-inflation and high encephalization quotients (or at least seems to be: a persistent problem with his style is uncertainty about just what positions he holds on a variety of issues). But if neither brain size nor language bootstrapped us into higher cognitive realms, what is left? The mimetic stage he invokes as the sole phase between episodic apes and linguistic humans hardly bears the burden, well-adapted though it may be for "customs, games, skills" (p. 173) and the like.

As for language as invention, collective or otherwise - although Dante and Spenser may have added grammatical constructions to Italian and English (p. 235), they could do so only by exploiting hitherto-unused spaces within the linguistic envelope made available by biology. To make the most obvious point, a poet cannot invent a nothing, still less assign places for that nothing to be in, or create principles that give each nothing a semantic interpretation. And yet any language is full of such nothings, and needs no rare genius to establish them. Take the following sentences from Aucaans (Djuka), a language "invented" by the children of African slaves who had escaped into the South American jungle (the position of each referential "nothing," or empty category, standing for an unexpressed agent or recipient in the situations described is indicated by e):

- (1) Kofi kai Samo e kon e gi en fisi
- (2) Kofi kai Samo e kon e gi e fisi

(1) can mean either "Kofi called Samo to come and give him, Kofi, a fish" or "Kofi called Samo to come so that he, Kofi, could give Samo a fish." (2) can mean only "Kofi called Samo to come so that he, Kofi, could give Samo a fish." Why (1), with the overt pronoun *en* ("him"), should be more ambiguous than (2), with a gap in the same place, and how the different semantic references are assigned in each case, is a matter too long and technical to deal with in the confines of a *BBS* commentary, but I can assure Donald that these questions are fully answerable within the framework of a generative grammar (e.g., Chomsky 1986). My point is that although it is vanishingly unlikely that the processes underlying (1) and (2) could have been invented by individual or collective ingenuity - and equally unlikely that children could subsequently acquire constructions produced in this way, without long and explicit training - both creation and acquisition are unproblematic if the processes involved stem from biologically specified mechanisms.

One syntax is accepted as biological rather than cultural, one can hypothesize a two-stage evolution of language: first an unstructured protolanguage - in effect, lexicon without syntax - characterizing *Homo erectus*, then an autonomous syntax arising to create the complex structures characteristic of full human language. This way of accounting for the *habilis-erectus-sapiens* speciations, proposed in Bickerton (1990), satisfies the numerous boundary conditions of the problem far better than Donald's mimetic stage. There is no evidence, even of an indirect nature, that *erectus* possessed mimetic skills, and no evidence for selective pressures toward such skills; the mimetic hypothesis cannot help to explain, except in terms so vague as to

be vacuous (e.g., "enhanced co-operation"), how *erectus* made fire, coped with sabertooths and cave-bears, or spread through three continents; and although apes can be trained in primitive protolanguages, no one, to the best of my knowledge, has yet taught them mimesis in Donald's sense, even though their very name has become a byword for imitation!

It seems more probable that, like representational drawing (another species-specific characteristic), mimesis is a spin-off from language rather than a precursor of it. Alas, failure to appreciate the dual nature of language (here, a culture-driven word-store, there, a biologically driven system of abstract structures), when coupled with the wholly legitimate goal of distinguishing cognitively between hominid species, leaves Donald no alternative but to hypothesize this or some equally fictitious Rubicon between two of our species' ancestors.

### "Pop science" versus understanding the emergence of the modern mind

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The question of how the human mind came to achieve its extraordinary power is obviously of great interest to those sciences that take human beings as the focus of their attention – to say nothing of its interest to human beings in general. Nevertheless, fields such as anthropology have made only modest progress toward providing illumination. As an anthropologist, then, I looked to Merlin Donald's book with the hope and expectation of gaining major insights that had failed to come from the contributors to my own area of effort. My disappointment, then, may stem from the level of my expectations, but I still feel that a more considered treatment of the part of the picture about which I am qualified to comment would have given me more confidence in the treatment of those aspects of cognition that are beyond the scope of my professional expertise.

Although Donald makes a laudable if inept bow to the contributions of archaeological and anthropological data, he has elicited approval for his stress on the role played by the cognitive sciences in dealing with the question of how the human mind came to acquire its unique characteristics. On the other hand, a reviewer of his book noted that, despite the claims for novelty, his book does not represent a departure from "old-fashioned cognitive psychology" (Longuet-Higgins 1992, p. 20).

The nonspecialist, then, should be able to read *Origins of the Modern Mind* with the expectation of finding out something about the nature of the science of cognition. Everything proceeds as though that were going to happen until one gets into Chapter 3 and is suddenly confronted with a series of postulated "input modules" that are described as being "isolable" and characterized by defined "properties" coordinated by a "superordinate integrator." The definition of input modules is said to be "a subtle and intricate one, which cannot be done easy justice," so it is not attempted. A list of modules is provided with their "properties" indicated by almost incomprehensible juxtapositions of words. To be sure, "domain-specific (dedicated computational resource)," "mandatory (automatic, obligatory processing)," and "inaccessible to consciousness" are less objectionable than "amativeness," "philoprogenitiveness," and "concentrativeness"; and the modules are not associated with identifiable "bumps" on the skull. But the baffled noninitiate finds it a little hard to see how the typology differs from nineteenth century phrenology in much more than the nature of the jargon. So we are left with a whole series of black boxes and not a single clue concerning their actual locus or means of operation. The reader has to take on faith that somewhere there is a justification for this categorizing and labeling. In my own case, that faith is

seriously undermined by the cavalier misuse of information available from anatomy, anthropology, and archaeology.

I should hasten to say, however, that, beyond the sloppy scholarship, there are some major and laudable aspects to Donald's presentation. The broad Darwinian perspective is admirable indeed. The link between the development of language and the evolution of the human mind, although hardly original, also has everything in its favor. For me, the use of the available evidence to postulate a three-stage sequence of mental development – episodic, mimetic, and symbolic – seems like a very useful scheme. Similar things have been suggested by linguistic scholars in the past – Swadesh's *epiglottic*, *paleoglottic*, and *neoglottic* comes to mind (Swadesh 1971, pp. 182–83). Donald makes no mention of this and its relatives (see the discussion by Hockett 1978), but his own formulation has all of the useful implications of those others and more as well.

Although Donald's cognitive stages seem intuitively reasonable, he keeps shooting himself in the foot as he attempts to provide evidence to illustrate the time and nature of their emergence. His treatment of the evidence for the appearance of articulate speech is a case in point. Although he realizes that since cancer patients can learn to speak through a simple throat tube "it is the brain, not the vocal cords, that matter most," he then goes on to cite Lieberman's far-fetched reconstruction of the Neanderthal larynx as evidence for a difference in the cognitive capacities of Neanderthal and "modern" forms of *Homo sapiens* (Lieberman & Crelin 1971; Lieberman et al. 1972).

Curiously enough, Donald even cites the definitive demonstration that Lieberman's conclusions have no basis in anatomical fact (Falk 1975), but this does little to dampen his enthusiasm for those claims. The discovery that the completely preserved hyoid bone – the voice box – of a 60,000-year-old "classic" Neanderthal from Kebara in Israel is indistinguishable in form from that of living humans has been offered as evidence that the Neanderthals were just as capable of articulate speech as we are (Arensburg et al. 1989; 1990). To be sure, Lieberman has continued to deny the obvious implications from the anatomical evidence (Lieberman et al. 1992), but the most recent assessment of all the available data concludes that the Neanderthals were not prevented by their anatomy from speaking just like "modern" human beings (Houghton 1993).

Having accepted Lieberman's view that the Neanderthals' ostensible lack of vocal capabilities indicated cognitive limitations, Donald goes on to claim that "modern" humans and Neanderthals "co-existed" in Europe for some 5,000 to 7,000 years during which time Neanderthal culture remained stagnant while "Cro-Magnon culture was evolving at a steady rate." There is not a single citation to support this allegation and it is simply incorrect. It has long been recognized that the cultural tradition associated with the Neanderthals is the Mousterian (Bar-Yosef 1992a; Bordes 1961; Bordes & Bourgon 1951; Dibble & Mellars 1992). The late Neanderthals at Hortus in southern France were in fact identified as Neanderthals because the archaeological tradition was recognized as Mousterian (de Lumley-Woodyear 1973). On the other hand, the late Neanderthal specimen from Saint-Césaire in southwestern France was called "Neanderthal" because of nuances of brow-ridge form that are more robust than typical "moderns" in spite of the fact that the cultural tradition was a form of Upper Paleolithic called Châtelperronian.

The Châtelperronian, however, was identified by the late François Bordes as being an evolved derivative of the local Mousterian (see the treatment by Harrold 1983). Furthermore, the dentofacial dimensions of both Hortus and Saint-Césaire are identical with those of the early "moderns" from the Aurignacian site of Předmost in Czechoslovakia (Brace 1979; in press; Matiegka 1934). Since both the cultural transition from Mousterian to Upper Paleolithic and the morphological transition from Neanderthal to "modern" is so gradual that it is impossible

to say where one stops and the other begins, Donald's undocumented claim that "Neanderthals underwent a drastic, rapid extinction between 45,000 and 35,000 years ago" is just an unsupported assertion based on a kind of current "folk-wisdom" that has to be relegated to the realm of "pop science."

In instance after instance, Donald displays the same cavalier disregard for fact in the treatment of the human archaeological and fossil record. To cite one of the more obvious examples, he refers to the supposedly "modern features, such as a larger, rounder cranium" in the 200,000 to 350,000+-year-old skull from Petralona in Greece (Stringer 1988). I include a drawing of the Petralona specimen here (Figure 1) to show that there is no hint of the "features of . . . modern sapient humans" despite Donald's undocumented assertion (Brace et al. 1979, p. 81). I won't even go into Donald's unsupported misuse of mitochondrial DNA beyond noting that grave problems plague the conclusions on which he evidently based his claims (Spuhler 1988; 1989) and that the credibility of the whole model has been called into question by the most recent independent appraisals (Hedges et al. 1992; Maddison et al. 1992; Templeton 1992).

If Donald's narrative has assumed the character of pop science in his treatment of prehistory, the same is true when he turns his attention to recent anthropology. With condescending ethnocentrism, he refers to "the monotony and redundancy of the hunting-gathering lifestyle." An employee in a garment shop or on an automobile assembly line might offer a few choice words here. In similar fashion and without benefit of any documentation, he asserts that the cultures of the San of South Africa and the aboriginal Australians had "remained unchanged for tens of thousands of years." But the mortars and pestles, bows and arrows, poisons, nets and snares of the Kalahari hunters; and the fish hooks, spear-throwers, traps, netting techniques, and grinding and leaching procedures used in Australia are all recent acquisitions that are radically different from the "type of tool culture associated with the very earliest modern human remains" (Bowdler 1976; Davidson 1933; Colson 1974; Klein 1984; Lee 1979; Meggitt 1957; Sampson 1974; Wilmsen 1989).

Donald's denigration of "autochthonous totemic dance rituals" in Australia as "still essentially mimetic" as though they indicated a pre-*sapiens* level of cognitive development equivalent to that of *Homo erectus* in the Middle Pleistocene is particularly regrettable. I want to close my review with a recounting of how one Australian group managed to transcend the "monotony" of their humdrum existence and avoid the "redundancy" of starvation during the drought of 1943 in the outback of Western Australia.

As things got bad, one of the tribal elders led his band off toward a fall-back waterhole at the extreme northwest corner of the tribal territory. He had only been there once previously, over half a century earlier, but he remembered how to get back. Resources began to fail even there, however, and he led them off again westward through country that he had never visited. Their trek took over half a year and eventually brought them out to Mandora Station on the coast of Western Australia more than 600 kilometers from the point of their start. During its course, they had proceeded via a string of between 50 and 60 waterholes (the account was collected by Norman B. Tindale in 1953 and related by Birdsell 1979, pp. 147-48).

Even though the old man had never been to as many as half of those waterholes, it was neither superior bushcraft nor dumb luck that led him to them. He had learned of their existence and location via the account of the wanderings of the supposedly mythical totemic ancestors intoned in the song cycle that accompanied the "essentially mimetic" "totemic dance rituals." Donald evidently assumes that these are occasions when the quaintly primitive Australians hop around mindlessly for hours imitating kangaroos and other creatures of the bush.

The rigors and discipline associated with Australian initiation rituals and ceremonies are famous in anthropological circles, although the lore that is transmitted on those occasions is often

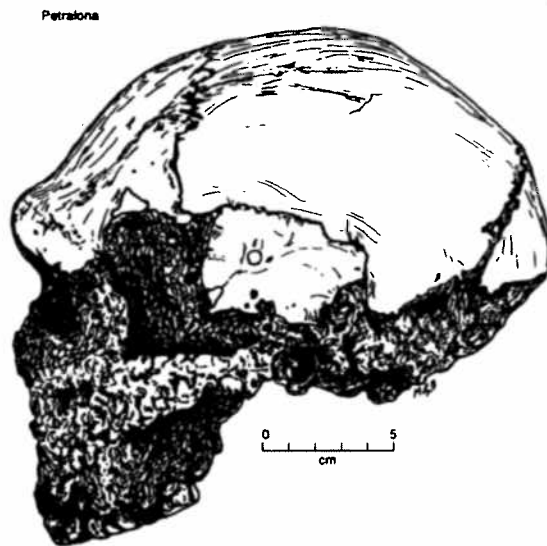


Figure 1 (Brace). The Petralona skull, an *erectus*-level specimen from the Middle Pleistocene of Greece, which is clearly not "modern" in form (Brace et al. 1979, p. 81).

treated as arcane and irrelevant. As the saga related above will indicate, however, there are times when that information is literally of vital importance. If the old man had forgotten a verse or gotten one wrong, the consequences could have been fatal for the whole group.

The cognitive sciences may indeed have a better handle on the emergence of mind than does anthropology. However, so long as they continue to treat anthropological data in the offhand and undocumented fashion of pop science, they are not going to promote much more understanding than the phrenology of 150 years ago.

### Mimetic culture and modern sports: A synthesis

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Merlin Donald's division of hominid evolution into three phases - australopithecine, *Homo erectus*, and *Homo sapiens* - and his correlation of these three biological stages with episodic, mimetic, and symbolic cultures, respectively, is a daring if not entirely unique step. His prediction that each of these stages should leave remnants in modern humans can stimulate a search for those remnants, increasing our understanding of how modern humans are put together. Our symbolic capabilities have been studied intensively in the context of language, logic, and problem-solving, and our episodic properties are the subject of most work in the psychology and neurophysiology of perception. The mimetic phase has received less attention.

We write this on the weekend of the Superbowl, the American professional football championship. A hundred million people will watch the game, and billions of dollars will change hands in tickets, promotions, advertising, and other commodities. And this is just the tip of a spectator sports iceberg that pays its stars more than professors, presidents, or prime ministers. How could such a phenomenon develop and why are similar activities so widespread around the world?

The roots of this phenomenon may lie in our mimetic past, when imitation was the most important method of transmitting

culture and acquiring the skills needed for survival. Imitation works best if it is experts who are imitated, rather than the less skilled. For imitation to work, one needs not only models to imitate, but a motivation to bother with the whole business. Thus, fitness would have been increased in those members of the population who were not only capable of imitating skilled performance in hunting, for instance, but who were also intrinsically motivated to observe the most skilled hunters in action. Observing performances of the less skilled would have been less productive.

Such an imitative culture would be facilitated if simply watching a skilled task being performed, as opposed to doing it oneself, could improve performance. This is indeed the case in modern humans, as revealed in the phenomenon of mental practice. The effect has been observed and replicated many times (reviewed by Decety & Ingvor 1990), and it works even in mentally retarded humans (Surburg 1991); retardedness, of course, is determined with symbolic rather than mimetic tests. Mental practice can increase the speed of acquiring a skill (Maring 1990), an important consideration when a dangerous task such as hunting is involved. And just imagining a skilled performance can improve motor skill once the activity has been modeled (Hall et al. 1992). Mental practice seems to work by improving the motor programs or plans, for it can lead to an increase in the apparent strength of a practiced movement even before muscle hypertrophy begins (Yue & Cole 1992).

A motivation to observe the most highly skilled physical activities seems to be built into us. In the modern world, we no longer imitate spear-throwers or bowmen, but spectator sports have exploited this motivation to witness peak physical performances and have formalized it into an elaborate cultural system. Only the very best players command top attention and top salaries. Combined with a mixture of tribal loyalty to the local team, the system is powerful and widespread. The fact that teams hire players from anywhere they can, and that whole teams occasionally move from one city to another, does not seem to affect the motivation and the loyalty very much, for only appearances are important.

The case of sports reveals an aspect of *Homo erectus* culture that remains in us, and implies that love of sports, or some precursor of them, is older than humanity itself. Is there any other evidence of Donald's three stages of phylogenetic development? Human development may provide a source of such evidence. Although contemporary developmental research challenges the biological dictum that ontology closely recapitulates phylogeny, there are several broad points of correspondence between them.

The episodic, mimetic, and symbolic stages proposed by Donald correspond loosely to Piaget's (1970) sensorimotor, preoperational, and operational (concrete and formal) stages respectively. This correspondence may stem, at least in part, from Piaget's early training as a biologist. Biological homeostasis is represented in Piaget's concept of equilibration, which is the mechanism through which humans adapt to environmental challenges. Each stage represents a particular form of adaptation that derives from the child's current cognitive competence. Like the apes that create episodic cultures, the sensorimotor infant knows the world through sensation, perception, and action but is unable to represent it symbolically. Because the frontal and parietal areas of the cortex are not fully functional, the infant is tied to the here and now. Sensorimotor development concludes with the beginnings of deferred imitation and language around two years of age. The preoperational child learns from imitation, and research shows that preoperational children are drawn to the behaviors of those who have slightly more advanced skills, and are more likely to imitate them (Morrison & Kuhn 1983). Finally, with operational thought comes logic, and with logic comes the ability to imagine possibilities, anticipate challenges, and isolate causal variables. Regardless of whether they are college professors or superbowl

impresarios, members of symbolic cultures can use these powerful mental algorithms either to conceptualize brilliant research or to make a buck.

The analogy is not a tight one, for even at the preoperational stage children are developing language far in excess of anything *Homo erectus* would have possessed, and some other symbolic capabilities are already developed. The direction and sequence of the development, however, clearly traces our Pleistocene and pre-Pleistocene past.

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### Archaeology and the cognitive sciences in the study of human evolution

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Donald has produced a model for the evolution of human cognition that is of considerable interest. It is beyond my own competence as an archaeologist to evaluate most of Donald's ideas. However, parts of his book (most of which are not reproduced in the accompanying Précis) do touch on the archaeological data, and given the relevance of his work to Paleolithic archaeology and vice versa, some comments from an archaeologist may be of interest.

Donald's model involves changes in cognition that in some cases result from or involve biological changes in the brain and in some cases do not. It would be nice to link the behavioral changes reflected in the archaeological record with the biological changes reflected in fossil hominid anatomy. Unfortunately, this is more difficult than one might expect. Archaeological assemblages are usually classified in terms of stone tool typologies, but the behavioral meaning of stone tool morphology is not always self-evident. Fossil hominids are classified in terms of their morphology, but usually neither the genetic, the functional, nor the behavioral meanings of these morphologies are clear and there is considerable disagreement even about how many species or subspecies there are (see, for example, articles in Delson 1985; Mellars & Stringer 1989). Beyond this, it is no longer possible to identify one archaeological tradition with one hominid form (*pace* Foley 1987). Lower Paleolithic stone tool industries traditionally associated with *Homo erectus* and Middle Paleolithic industries traditionally associated with Neanderthals are now known to overlap in time (Tuffreau 1982). Modern *Homo sapiens* appeared in Africa and the Near East before the Upper Paleolithic industries traditionally associated with them, and in neither location is their appearance accompanied by archaeological evidence of significant behavioral change (e.g., Bar Yosef 1992b, pp.196-99). Even in Europe it now appears that Neanderthals coexisted for millennia with modern *Homo sapiens*, producing an essentially Upper Paleolithic archaeological record (Lévêque 1989; Lévêque & Vandermeersch 1980).

As Donald correctly points out, at the Middle to Upper Paleolithic boundary (formerly considered to be coincidental with the Neanderthal to modern *Homo sapiens* boundary), certain rather striking changes in behavior (especially the origins of undeniable art) do take place in Europe (see Chase & Dibble 1987; 1992). Evidence for the same changes does not appear until much later in other parts of the world, however, despite the indisputable presence of fully modern humans. It remains to be determined whether this is due to the vagaries of preservation or to the fact that art, like written language, did not come automatically with biological change and is something even modern humans can do very well without. Thus, although



at a certain level links between biology and behavior undoubtedly exist, when one considers specific links, caution is necessary.

The behavioral implications of later Lower Paleolithic archaeology (the period Donald uses as his model of the behavior of *Homo erectus*) are also less clear than was generally believed until only recently. In particular, the characterization of Lower Paleolithic peoples as regular hunters of very large game, such as elephants, has come under attack (Binford 1987; Klein 1987, pp. 11–32), although in my opinion even elephants were probably hunted, at least on occasion (Adam 1951; Scott 1980 – also see Villa 1990). What is not clear is how much social complexity this hunting required. I doubt that the need for communication in hunting would have played a very big role in the evolution of mimesis. In fact, the one thing that would be most useful in cooperative hunting, the ability to discuss future and conditional events with precision, would probably not be possible without the syntactic structures provided by language.

Inferring sexual division of labor and cooperation on the basis of (1) hunting or (2) the clustering of stone tools and animal bones into the concentrations we call sites has also come under very serious attack over the last two decades, although the primary arena for this debate has been the basal Paleolithic sites of East Africa (usually attributed to *Homo habilis*; see Isaac 1983 and Klein 1989, pp. 170–80, for summaries). It is possible (but not demonstrated) that division of labor was common by the later Lower Paleolithic. By the Middle Paleolithic of Europe there is little doubt that Neanderthals were at the very least transporting meat from place to place on a regular basis (e.g., Chase 1986, pp. 46–57) and it may be that this reflected sharing (1) between hunting/foraging parties and those remaining at home, (2) between different hunting/foraging parties, or (3) between hunting parties and foraging parties.

Many scholars have drawn conclusions about intelligence or symbolism from Middle Pleistocene stone tools. However, their cognitive implications are not entirely clear. Donald may overestimate the difficulty of making stone tools. It is true that it takes practice, but a few months of practice should be seen in terms of a young hominid growing up doing what the surrounding grownups are doing. In fact, it is doubtful that pedagogy is necessary for Lower Paleolithic stone tool technology. After all, the making of Paleolithic-style stone tools was a lost art, reconstructed by archaeologists working without even the benefit of someone to observe. On the other hand, it is also true that the skills involved are apparently beyond the ability of chimpanzees to master. However, exactly what new cognitive abilities are required has not been analyzed in the kind of detail the subject deserves. The only in-depth study has been done by Wynn (1979; 1981; 1985; 1989), using a Piagetian perspective, and for the most part he considered secondary attributes of stone tools such as the relative placement of different flake scars rather than the fundamental problem of learning how to remove a flake from a stone core. It is thus difficult to evaluate the need for a new cognitive structure such as Donald's mimesis.

Another old archaeological belief coming under increasing attack is the idea that the stone tools of the Lower or Middle Paleolithic (or even, for that matter, many of the tools of the Upper Paleolithic) required a great deal of time to manufacture and were made for specific purposes well in advance of actual need. Some lithics specialists (Dibble 1987; 1988; Rolland & Dibble 1990, pp. 482–86, see also Chase 1990) feel that such tools were often if not usually *ad hoc* in nature, and even more elaborate tools such as bifaces were probably usually multipurpose tools not destined for a particular purpose.

Not all of these comments are critiques of Donald's book, and certainly none go to the heart of what he has to say. The most important point is that, in general, the meaning of archaeological data in psychological terms is either unclear or controversial or both. One reason is a lack of communication between archaeology and psychology. If more researchers follow Donald's

example in the future, there is every reason to hope that the dialogue between archaeology and psychology will benefit both disciplines.

## Symbolic invention: The missing (computational) link?

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There is much to applaud in Merlin Donald's careful and imaginative reworking of our collective cognitive history. The head-on confrontation of so many major puzzles concerning how any sequence of individually viable transitions might bridge the abyss between ape and man is a delight, as is the author's sensitive and balanced treatment of the powerful role of external symbol systems in reconfiguring human cognition. My purpose in this commentary is merely to draw attention to what I see as the major cognitive scientific problem which Donald's discussion isolates, and to make a suggestion concerning how best to view it.

The key unsolved mystery, if we accept the bulk of Donald's discussion, concerns what he calls "symbolic invention." The problem of symbolic invention (which may or may not be identical with what the author calls "the problem of reference," p. 368 – I found the latter usage puzzling) concerns how we achieve the spontaneous and repeated development of new symbols. It is this ability which both distinguishes our use of symbolic media from that of the apes (p. 160) and which the author depicts as the vital innovation of the so-called mimetic mind ("mimesis is fundamentally different . . . in that it involves the *invention* of intentional representations," p. 169). Again and again in the book Donald comments on the important difference between the spontaneous and repeated *invention* of symbols and the mere ability to exploit them once they are available (see, e.g., pp. 134, 160, 169, 368). Once symbolic invention is achieved, the organism is on the royal road to the third transition and genetic evolution can be replaced by cultural evolution grounded in the exploitation of a burgeoning series of external symbol systems and external memory systems. Symbolic invention thus seems to be the real "missing link." But what exactly is missing? How best to conceptualize this pivotal issue?

One possibility (which I think of as pretty much Daniel Dennett's view of the problem – see Dennett [1991] and especially Dennett [forthcoming]) is to try deflationary tactics. [See also Dennett: "Intentional Systems in Cognitive Ethology" *BBS* 6(3) 1983; "Précis of *The Intentional Stance*" *BBS* 11(3) 1988; and Dennett & Kinsbourne "Time and the Observer" *BBS* 15(2) 1992.] One key deflationary tactic is to reverse Donald's order of events. Instead of depicting some complex of biological adaptations as the root of a capacity for symbolic invention and public language as an *effect* of symbolic invention, the presence of public language is itself depicted as the root of symbolic invention! This sounds paradoxical. But a story can be told. A tortuous sequence of chance discoveries (e.g., of the usefulness of using some external items as labels for others) eventually puts a kind of protolanguage in place. Exposure to this new kind of input reconfigures the next generation's cognitive architecture in a way which promotes the development by them of a *little* more language. And so on, until we reach the present state of affairs in which the average child is exposed to the fantastically potent reconfiguring forces of the whole external symbolic apparatus of the "theoretic mind." Children's rich abilities of symbolic invention are, in this scenario, then *explained* by their experience with the symbols of public language. Public lan-

guage is seen as the software which reconfigures the mind so as to make symbolic invention possible.

The deflationary response is, I think, interesting and deserves some careful consideration. My own view is that Donald is right to opt for the other scenario, in which there is a computational "missing link" which puts a capacity for symbolic invention in place and thus *primes* the onset of public language. I close, however, with the suggestion that it may be fruitful to broaden the notion of symbolic invention, for, as Donald uses it, symbolic invention implies the spontaneous and repeated creation of new *external* symbols. But a prior question concerns the ability of a system already capable of episodic thought to recode those thoughts in a series of increasingly abstract ways which support more flexible (but still nonlinguistic) behaviors. This notion of progressive recoding is the heart of Karmiloff-Smith's (1979; 1992) theory of representational redescription. Its computational implications are further discussed in Clark and Karmiloff-Smith (forthcoming) and Clark (forthcoming).

The simple suggestion I want to make here is that instead of focussing directly on external symbolic invention as the central issue, we may see the creation of new external symbols as continuous with the progressive creation of new *internal* symbols and structures. Thus refocussed, what emerges as crucial is the ability to engage in forms of computation that construct higher and higher orders of representational content. And one fascinating feature of the recent connectionist work which Donald later mentions is that such systems *are* able spontaneously to construct new (higher order) internal representations, although they do so only in quite limited ways (see e.g., Rumelhart et al. 1986). Understanding the nature of these abilities and finding ways to overcome these limitations may provide a practical means of one day illuminating the much more opaque issue of how we invent and use new external symbols. The key issue thus construed is *representational* invention: symbolic invention is best seen as a special case.

## A natural history of the mind: A guide for cognitive science

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Cognitive science seeks to understand how the mind emerges from the brain. Artificial intelligence seeks to implant a mind within the machine. To their detriment, both have largely ignored work on the natural history of the mind. They would do well to pay close attention to Merlin Donald's account of mental evolution.

The archeology of mental evolution is of course a difficult project since the mind leaves no fossils. Only the artifacts produced by brains containing minds controlling bodies are available for examination. Not until the advent of writing do we apparently have direct access to the thoughts of others. Looking back to the dawn of history, Snell (1960), comparing the language of Homer's *Iliad* and *Odyssey*, concludes that mentality changed between the periods represented in the two works. In the *Iliad*, there is no word for the modern concept of mind. The stormers of Troy seem to be sleep walkers controlled by godly visions. But after the fall of Troy, clever Odysseus appears a modern man, fully self-aware and introspective.

The more controversial Jaynes (1977) also finds a phase change when comparing earlier written records with more modern. His evidence is Biblical; the book of Amos sounds alien to a modern ear, whereas the more modern book of Job is fully modern (it is even the basis for contemporary song). Jaynes seeks reasons for the change within the structures of the brain, but his

explanations involving the formation of laterality in the brain do not ring true.

Although ancient literature hints at a different style of thinking, ancient artifacts do not. Allowing for a more primitive technology, the equipment of the Neolithic hunter recently found frozen in the Alps does not seem strange to a modern backpacker. Donald's *Origins of the Modern Mind* provides a natural explanation for these observations.

Briefly, Donald divides the evolution of mind into four stages. First at about 2 million years B.P., ape becomes hominid. The hominid mental culture is an extension of the episodic culture characteristic of apes. The penultimate ape, the hominids are able to respond to very complex sets of stimulus scripts or episodes, but they do not plan ahead.

Around 700,000 years B.P., *Homo erectus* appears. The *Homo erectus* mental culture is mimetic; they think and plan ahead, but do so without language. As a modern example, Donald cites the case history of Brother John. When epileptic attacks deprive Br. John of language, he is nevertheless able to plan and carry out quite complex scenarios. *H. erectus* brings the ability to manipulate the environment through tool use to modern levels. Properly educated, a *H. erectus* could make a living through manual labor in the modern world.

Around 60,000 years B.P. modern man, *Homo sapiens*, appears. *H. sapiens* has spoken language and has a mental mythic or linguistic culture. Communication is oral and societal structure is maintained through ritual. Perhaps the peak of mythic culture was reached with the world-wide rule of Roman oratory. Nevertheless, the fully modern mind does not appear until after the advent of writing. Early systems of writing, cuneiform and so on, however, access the linguistic part of the brain only indirectly, through the earlier episodic and mimetic portions. As a result, these systems implemented linguistic storage and communication imperfectly, and were not widespread among *H. sapiens* culture.

The breakthrough into theoretic culture comes with the invention of the phonetic alphabet. The direct mapping of visual, physical symbols to phonemes enables the linguistic portion of the brain to begin directly processing writing. Phonetic writing provides an organizing center, linking external memory storage to all three portions of the brain - episodic, mimetic, and linguistic. The brain plus external storage is thus more capable than what came before; the modern mind has been born. A feel for the brain/phonetic writing synergism can be gotten from Donald's metaphor for reading. In reading, the contents of the book take control of the brain of the reader. The book is the source material and the brain is merely the player. The brain/written word is thus something more than the brain alone.

Thus, our modern manual skills date back to the era of *H. erectus*, and similarly, our rituals and icons originate in the Paleolithic. Only in historic time, however, does the synergistic combination of brain and environment occur that is the modern mind. As Snell and Jaynes argue, traces of this change can be found in written literature.

Donald's mental architecture is quite different from the computational paradigm much used in cognitive science. His architecture makes explicit allowance for the external environment through the central organizing principle of phonetic written language. The architecture is also vastly different from the low-level approaches advocated by connectionists. Although Donald does discuss neurophysiological features such as Broca's area, this is mostly to argue that natural selection has worked on the brain. Donald's work suggests new approaches based on the natural history of the mind. It deserves close attention by both cognitive scientists and AI researchers.

On a final speculative note, echoed by Donald himself in his final sentences, the ideas in *Origins of the Modern Mind* should be applied to the present. The current developments in interactive, networked, multi-media, and virtual means of communica-

tion based on computers and electronics may be pushing *H. sapiens* toward another shift of mental architecture. Participants in the old theoretical culture, reading these words, can only dimly guess what form the next culture will take.

## The place of cognition in human evolution

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Modern science remains deeply committed to the project of localization, be it the attempt to locate development in the genes, society within the individual, or cognition within the head (see Still & Costall 1991). Merlin Donald claims that cognition is "the engine, as well as the locus," of human evolution (p. 2). My purpose is to question the "locality" of this proposed locus. For although Donald himself takes us some way towards a distributed conception of cognition, he does not go far enough.

The idea that cognition must be "in the head" has been encouraged by the mythology of "cognitive mechanisms" and the promise that these would eventually be localized within the brain. One of the important messages of Donald's review of the neuropsychological literature is that many of the anatomical structures "underlying" human cognition have not merely been "coopted" to serve new functions (cf. Gould 1991), but arise – *post hoc* and *ad hoc* – along with those functions (see pp. 11–14). Cultures, as he puts it, "reconfigure" the brain (p. 14). Yet surely even when so reconfigured, cognition has not itself become lodged inside the head. To quote Bartlett's apt critique of Head's lesion studies:

Head gives away far too much to earlier investigators when he speaks of the cortex as "a storehouse of past impressions". All that his experiments show is that certain processes cannot be carried out unless the brain is playing its normal part. But equally those very reactions could be cut out by injuries to peripheral nerves or to muscular functions. One might almost as well say that because nobody who is suffering from raging toothache could calmly recite "Oh, my love's like a red, red rose," the teeth are a repository of lyric poetry. (Bartlett 1932, p. 200)

The point, of course, is more general. Performance would also be disrupted if the environment, too, were not playing "its normal part." So the consistent (if incorrigible) localizer will need also to "localize" the function in question *out there* as well.

Donald's own departure from an internalist approach to cognition is most clearly indicated by his emphasis upon "external memory technology" in human evolution. Despite his references to a multilevel approach to evolution (pp. 157 et seq.), he does not fully explore the radical implications of the distribution of intelligence. On the one hand, in his emphasis upon stages, he neglects the important place of traditions in many nonhuman groups; cognition is distributed in shared skills and in the very structure of the (structured) environment. Yet he also underplays the real difference that language and literacy make. Consider Donald's remarkable example of a monk, Brother John, who, whilst suffering a temporary attack of aphasia, nevertheless manages to book into a foreign hotel (pp. 82 et seq.). Donald presents this as an example of regression to a stage of prelinguistic if distinctly human intelligence. Yet the world in which Brother John exists does not itself regress. His success relies upon a world in which there remain passports, other people who understand their significance, and his own appreciation of these facts.

To theorize our human world requires more than to turn the internalist scheme of cognitivism inside out. Donald himself presents many astute criticisms of standard cognitive psychology. It is all the more surprising therefore that he allows his own

account of the distribution of cognition to become hijacked by the misleading metaphors of memory and storage. For in talking of the externalization of memory he commits himself to a "privatized" metaphor of memory where our shared practices of storage are transferred to the individual and stripped of their social dimension. When this conception of memory is transferred back out into the world, computer technology and much else besides appears as a purely "technical" affair: "Individuals in possession of reading, writing, and other visuographic skills thus become somewhat like computers with networking capabilities; they are equipped to interface, to plug into whatever network becomes available" (p. 311).

The technical metaphor of networking surely masks the fundamental issue raised by Donald's wider argument, that of the transmission of culture. Our relation to culture is reduced to finding the right password and accessing an appropriate store. The social dimension disappears. Given, however, that cognition does not (just) occur within any one of us, psychologists had better ensure that they once again make the issue of cultural transmission their business (cf. Bartlett 1929) and seek fresh metaphors that capture more fully the implications of the distribution of cognition.

## Human evolution: Emergence of the group-self

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The study of human evolution reminds me of the puzzles that are so popular nowadays. We have an undefined set of knowledge-pieces consisting of past changes of human morphology and behavior, and we try to assemble a coherent, meaningful, logically defensible pattern for the evolutionary process. It is an advantage in this game if somebody can minimize the number of pieces remaining in hand and Donald has certainly done this.

Not only has he used most of the known pieces, but he has designed a configuration that shows some strikingly interesting patterns. The mimetic culture of *Homo erectus* is an elegant idea, a "missing link" indeed (a similar train of thought appears in Csányi 1992a; 1992b). Mimesis fits well between the episodic culture of apes and the mythic culture of *Homo sapiens*. Puzzles differ, however, from studies of human evolution in that we sometimes cannot even be sure of the reality of the pieces used for construction; accordingly it is worth considering other candidate syntheses (Csányi 1989; 1990; 1992a; 1992b).

We can assume an evolutionary change in the human brain, be it a new solution for mimetic representation or the emergence of the language "organ" localized or distributed, only if we suppose that a pattern of selective forces provided by the environment was available to enforce the change. The evolutionary puzzle can be solved if we dissect the changes into smaller units and find a plausible explanation for the sequential emergence of these units in time. Donald emphasizes the individual changes, but in human evolution we have to account for the phenomena on at least two organizational levels. Simple group formation and cooperation can be explained satisfactorily by individual selection in animals. Many animals cooperate successfully at a very high level without having a language or any special cognitive mechanism. A wolf pack or the lioness's in a pride can catch prey by concerted action. Each member of the hunting group knows exactly his place and function during the joint action. This kind of cooperation occurs on the basis of a species-specific "schema" that is flexible but strongly constrained genetically. There is no need for explanation or reconciliation and there are neither roles to distribute nor actions to concert in time. On the contrary, among humans cooperation almost always occurs on the basis of

an individually designed schema or plan requiring prior elaboration (Rumelhart 1980). The key word here is the *individuality* of the schema. Individuality represents an enormous adaptive value whatever forms it takes during evolution (Sterrer 1992). Sexuality made genetic individuality possible, making available high variability to the changing conditions of the environment. A neural individuality was made possible by the various mechanisms of learning and also enhances individual variety. Both genetic and neural individuality are constrained in animals living in groups. Genetic individuality is constrained by the gene reshuffling in each generation. Neural individuality is constrained by the lack of language, because the complexity of the species-specific schemata of cooperation could not be increased beyond a certain limit by individual learning mechanisms. This is the key problem of human evolution.

Further evolution of animal cooperation would have been possible only by introducing individual group-schemata, which was not conceivable without a language of a human type. The necessity of individual group-schemata of cooperation forced the cognitive ability of the early *Homo* lines to reach a higher level, including the emergence of language.

From the study of primates and apes we know that individuals in groups constantly watch each other's activity, try to predict future actions of important individuals, and use their social skill to manipulate others (Byrne & Whiten 1988). [See also Whiten & Byrne: "Tactical Deception in Primates" 11(2) 1988.] They can interpret each other's actions in their modeling process (Csányi 1992b). The next big step could really be the mimetic culture, which is in essence the development of a *social super-model* as a cognitive device: in its most primitive forms this could be based on common learning processes (Csányi 1989). Such processes are the simple rituals in the form of gestural and vocal signaling of the subsequent common actions. Mimetic culture has its own animal roots. For example, primitive forms of ritual can be observed in present-day highly socialized predators such as the African wild dog. These gather before the hunt and the alpha male initiates a "ceremony" which involves various forms of social interactions such as "kissing," tail-wagging, and mutual licking of muzzles. These activities spread through the group and synchronize the mood of the individuals. Excitement builds up and the dogs are then ready to go off together (Chinery 1979).

Memory traces of such rites connected to the group's vital actions become part of the environmental model of each individual, but they also represent a *super-model*, a group entity, because this model can be activated and processed only during a group action (Csányi 1989). (Apart from rituals, other mechanisms serving to entrain action include the "comprehensive interactional synchrony" [CIS] observed during hypnosis; Bányai 1985; 1992; Csányi 1992b.)

The super-model, independent of its internal mechanisms, is an *individual structure*. Each of the small groups of early *Homo* confront its environment with a unique individual super-model. As communication developed among group members their super-models became more individualistic. This generated the high selection potential for language evolution. A group of apes with individual species-specific brain models could survive only in a very narrow niche. Group-based super-models made radiation possible and *Homo* groups spread over the whole planet because they could find the appropriate adaptive responses for any conceivable environment.

The emergence of the super-models corresponded to the appearance of the *group-self*. Animal group formation is possible because of dyadic interactions. In *Homo* groups the group-self and its group representation appear. Human personality and the group-self mutually define each other. The emergence of the group-self created a new organizational level. Further evolution depended on the structure and competition of these new group-individuals. Formation of the group-self needed several changes in the "hardware" of the *Homo* brain. We can understand human behavior only if we suppose that humans are

genetically able to recognize, accept, and represent group-selves. We have many reasons to assume this (Eibl-Eibesfeldt 1989; see also Eibl-Eibesfeldt: "Human Ethology" *BBS* 2(1) 1979).

A second problem on which I would be happy to have the reaction of the author concerns whether, with the emergence of language, those genetical changes which led to the emergence of group-self and culture at last come to a close. Is the formation of "theoretic culture" only a "software" problem, as Donald assumes, or do we have to consider the thousand-year rule of Lumsden and Wilson (1981; see also multiple book review: *Genes, Mind and Culture*, *BBS* 5(1) 1982), which involves formative genetical changes? This is a very important question; and the answer, whether positive or negative, calls for solid evidence.

## Ethological foxes and cognitive hedgehogs

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The cognitive sciences often suffer from their lack of ties to ethology and evolutionary biology. Questions of memory, attention, language, and learning are approached as if one were back-engineering a computer chip. There is no interest in where the chip came from or its relation to the creation of other chips. This may be well and good for silicon devices, but it seems wrong-headed in the biological sciences. Donald's book is a treatise concerned with making strong links between cognition and its evolutionary origins. He is asking this question in the face of the overwhelming tendency of cognitive science not to make links with anything in the direction of evolution. Many cognitive scientists are in a mad rush to go the other way, toward even more proximal causes. His sanguine indifference to this neural reductionism is commendable. Many cognitive scientists pay lip service to evolution with Just-so stories but fail to provide anything substantial. The question is whether Donald has provided links between cognition and evolution or has offered up another, extremely elaborate, Just-so story.

First, it should be noted that the difficulty of reconciling cognition with evolutionary theory has already both been commented on and studied. Perhaps the most acerbic comments came from B. F. Skinner, who likened cognitive psychologists to creationists who scorn Darwin's theory of evolution (Skinner 1978). [See also *BBS* special issue on the work of B. F. Skinner: Canonical Papers *BBS* 7(4) 1984.] In his view, cognitive science ignores the historical, contingency laden aspects of behavior – which means to him that it ignores just about everything. Donald is an apostate – but of a different heresy than cognitive scientists. In Skinner's view, Donald would correctly have identified a schism, but thinks incorrectly that the sects can be reconciled.

**Cognitive ethology.** Those who have sought some form of unification have, by and large, not been cognitive psychologists but ethologists. For example, Yoerg and Kamil (1991) provide an extended discussion of this issue. This may account for the curious fact that Donald seems largely unaware of the research. Ethologists are keenly aware of the difficulty in contemplating the evolution of cognitive structures. Donald Griffin and other cognitive ethologists, as they have been labeled, have struggled both with this question and the question of animal consciousness (Ristau 1991). Many of the issues Donald raises have experimental data. For example, Peter Marler (Marler et al. 1991) and many of his colleagues, including Cheney and Seyfarth (1990), have studied how animals represent and communicate such things as predators and food. Some animals may be more or less

truthful or deceitful depending on what is to be gained. These studies speak directly to Donald's notion of the evolution of mimetic representation. As ethologists, we find it very unclear whether one can safely say that this ability to re-represent, or mime events, can be phylogenetically localized in the way Donald wishes. This evolutionary question is dealt with below.

**Episodic memories.** Donald seems intent on preserving the idea of episodic memories largely for primates: "Episodic memory is apparently more evolved in apes than it is in many other species . . ." (p. 151). As far as we know, this notion is unfounded. He does cite studies on how some birds hide and relocate food as an episodic memory system. What he does not discuss is the complexity of this avian memory system. Some species of birds can remember the location of thousands of items. Likewise, pigeons can remember hundreds of slides after as little as one presentation per slide. A more parsimonious view is that evolution has opportunistically availed itself of different forms of memory, depending on the needs of the animal in its niche (Shettleworth 1993). There is no comparative evidence for Donald's view of the progressive evolution of episodic memory systems.

Some of the comparative statements to make this claim are unfounded or require the overinterpretations of data. Lesioning certain brain nuclei in song birds will render the birds unable to sing. Other birds, when given hippocampal lesions, lose the ability to cache food. Donald interprets this to mean that (1) food caching is an episodic memory task and birdsong is a procedural memory task, and (2) episodic and procedural memory systems involve different neural mechanisms (p. 150). This is wrong for a number of reasons. First, it is doubtful that either food caching or birdsong relies on only one type of memory. Both of these behaviors are complex and require the animals to integrate long-term procedural information as well as short-term, dynamic, episodic information. Second, there is no evidence that lesions to the song system nuclei cause memory deficits. The bird's inability to sing could be a motor deficit. Third, there is no evidence that the hippocampus is not involved in song learning. For all we know, the song system may require the hippocampus. Donald's speculations seem dangerous, as they rely on the outcome of yet unperformed experiments. We can only presume that Donald's interest is not to understand birdsong and avian food caching, but to hold these systems hostage to his interpretation of the evolution of the human mind. Another comparative approach to the neurobiology of cognition (Kesner & Olton 1990) begins with a chapter that warns of the dangers of interpreting animal studies in this manner (Hodos & Campbell 1990).

**Evolution.** Donald's approach is to check for evolutionary plausibility when considering scenarios for the origins of mind. The dangers of this adaptationist approach have been discussed elsewhere (Gould & Lewontin 1979). From his arguments we infer that, to Donald, evolutionary plausibility means that intermediate forms in the evolution of a trait must be adaptive and that one should see "vestiges" of earlier forms. Although we agree with the first point, the necessary occurrence of vestiges, mentioned by Darwin, is not a tenet of modern evolutionary theory. He refers to human behaviors, such as baring the teeth in anger or wailing, as vestigial (p. 3). Some clinical psychologists would argue that any human without these capabilities is not lacking in vestiges of nonhuman animals, but is deficient in essential human qualities. Donald refers to a "continuum" from reptiles to mammals to primate to human and to the "gains" of our hominid ancestors. These all connote a *scala naturae*, a notion of evolutionary progress that has been thoroughly discredited (Hodos & Campbell 1969; 1990).

Donald's argument often suffers because he seems unaware of or uninfluenced by modern developments in evolutionary theory. When discussing whether language arose as a consequence of the evolution of a single cognitive module or several independent modules, Donald argues that the unitary theory only requires a single selection pressure while the modular theory

requires multiple selective agents. This need not be the case if a genetic covariance exists between the genes underlying each of the modules: in that case a single selection pressure could drive the evolution of multiple modules.

A great deal of Donald's argument centers on the influence of culture on cognition and cultural evolution. Here again, Donald seems unaware of the extensive and rigorous literature on this subject (e.g., Boyd & Richardson 1985; Cavalli-Sforza & Feldman 1981; Lumsden & Wilson 1981; Pulliam & Dunford 1980). Some of the quantitative models contained in these references might improve Donald's thesis. For example, Donald argues that the appearance of language would lead to more cultural innovation and thus speed up the rate of cultural/cognitive evolution. This is analogous to arguing that the rate of mutation determines the rate of genetic evolution. This view was held by some early evolutionists but refuted by J. B. S. Haldane (1964). It seems unlikely therefore, that the rate of "cultural mutation" limits cognitive/cultural evolution.

**Conclusion.** We commend Donald's objectives but we object to the anthropocentric and therefore insular nature of the book. Almost all of psychology has suffered from anthropocentric tendencies (Staddon 1989). Donald has not successfully escaped this trap. This is evident in his casual use of evolutionary and ethological terms and concepts. In terms of a commonly used allegory, cognitive psychologists by and large remain hedgehogs, viewing the world in respect to one thing, that is, humans. This is contrary to the approach of ethologists and evolutionary biologists, who, like foxes, grapple with the complexity of human and animal behaviors in terms of evolution (Berlin 1957; Marler 1969).

## What about pictures?

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Merlin Donald has written a scholarly yet pleasurable book; a rare achievement. In it he traces the development of the human mind through four stages which he terms episodic; mimetic; mythic; and external symbolic storage and theoretic. The evidence for these changes is derived from archaeology, anthropology, neuropsychology, primatology, and above all, cognition. The contributions of these approaches are unevenly spread; notably, and not surprisingly, archaeological evidence dominates the more distant and shadier stages, but the theme that runs through them and unifies them is derived from cognition. This is as it should be in a book dealing with the mind. An episodic culture whose carriers were incapable of abstraction, a "here and now" culture, changed into a mimetic culture wherein deliberate communication was possible and which in turn developed into mythic culture, in which invention and the development of language advanced symbolic thought; finally came externalisation of symbolic operations by the adoption of material symbols.

Since this is a multiple review the reviewers are permitted greater freedom than usual to view the book mainly from the stance that their expertise provides. My modest expertise is in the realm of pictures as means of communication. [See Deregowski: "Real Space and Represented Space" *BBS* 12(1) 1989.] Donald postulates (on good archaeological evidence) that the art of picturemaking developed rather late, that the art was *invented*, and that pictures chronologically followed language and were "The critical innovation underlying theoretic culture" (Donald, p. 275). This argument raises two queries, the principal one being: Why did pictures have to be *invented* when it was much easier to *discover* them? Kennedy (1975) made this point explicitly but it is useful to review it.

The essence of the argument is that a depiction of an object is not arbitrary, as its name (the noun) is; it derives directly from the object's appearance. It may not present all the details of appearance but only a selected few, and it may stress those few and thereby distort them, as in caricatures; but the information encoded in a picture and available to the viewer always overlaps with the information provided to the viewer by the depicted object. The depicted object and the depiction thus have much in common. The perceptual task the observer has to perform is that of recognition of the communication of information derived from these two sources. The task is not as cognitively formidable as it might appear *prima facie* because 3D objects seldom provide viewers with information that is invariant; for example, such information varies with direction of illumination and with the orientation of the object relative to the viewer, so that objects are easier to recognize in some circumstances than in others.

In short, the "ding-dong" theory, which Jespersen (1922) rightly rejects as an inappropriate explanation of the origins of language, the theory that there is a harmony between sound and sense in the world, does apply *mutatis mutandis* to depictions. This thesis is supported, paradoxically, by Kennedy's (1982) work on the blind, which shows that they readily accept certain "pictorial" images, thus demonstrating the readiness of the cognitive system to regard them as appropriate. If this is accepted then a question must be asked: Why did drawings not appear at the mimetic stage at which we are told our ancestors could deliberately communicate by representing an object or an action? It seems unlikely that the obstacle lay in their poor motor skills so that, although cognitively capable of drawing, they could not execute drawings. Did it lie in the absence of a *need* for this kind of representation? This might have been the case, and the existence until recently of pictureless cultures (e.g., Fortes 1940; 1981) suggests that (in some instances, at least) language development was not followed by the development of pictorial representation. The two phenomena appear to be mutually independent. This conclusion, however, does not agree with the thesis put forward by Davidson and Noble (1989) and elaborated by Noble and Davidson (1991) according to which the development of language is intimately linked with the development of art. This thesis, if correct, would argue for revising the relationship between mythic and external symbolic storage cultures postulated by Donald.

There is yet another point which ought perhaps to be made, namely, that the sequence of development from depiction to writing may not be linear. It is possible to argue (Deregowski 1990) that there are two perceptually distinct and quintessentially different modes of pictorial representation, one concerned with the depiction of individual objects and the other with characteristics of attributes shared by objects of a particular category. These two distinct modes are exemplified by, say, a portrait of a man and a stick-figure drawing of a man, respectively; the former identifies a particular individual within a group (men), the latter shows attributes. This bifurcation complicates the scheme put forward by Donald because only the latter form seems a likely candidate for fostering the development of writing. It is therefore of interest to consider the origin of the bifurcation. Were men driven into this division of symbols or did they stumble upon it by accident? Did the fact that to an observer unfamiliar with an individual the portrait of that individual has a broader meaning than it has to an observer familiar with the person portrayed contribute to the split? Were early depictions portraits of individuals rather than depictions of members of certain groups (say, of certain species)? If the former, then presumably this would reflect the strength of social bonding, and it would perhaps have implications for other cognitive attributes such as language.

In Joseph Conrad's *Typhoon*, Captain MacWhirr, when taking charge of a new steamer, the Nan-Shan, notices an ill-fitted door lock and has it replaced. He does not question the soundness of the ship, and subsequent events show he was right not to

do so. I feel that in this commentary I have acted likewise. I have questioned one small aspect of a bold thesis which, on the whole, seems to me well constructed and certainly merits very careful consideration.

(Yet another minor point: Donald refers [p. 25] to Adam Smith's [1804] work as done "in England." If my supposition is correct that the work referred to, but omitted from the bibliography, is the "Dissertation on the origin of languages," then it was *not* done in England. Adam Smith was a Scotsman born in Kirkcaldy; the work in question was first published as an addendum to the 1761 edition of his *The theory of moral sentiments* based on the lectures delivered at the University of Glasgow, where Smith held the Chair of Logic.)

## The modern mind: Its missing parts?

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Biologists find other disciplines' parochialism frustrating. The very nature of their subject matter has forced organismic biologists to recognize that phenomena have to be approached at four (and maybe more) very different levels. Questions about mechanisms, function, development, and history are logically independent of each other (at least in principle), yet a proper answer to the question "Why is X the case?" ultimately requires all four kinds of answers.

Merlin Donald's book is greatly to be welcomed in this respect because it represents one of the first attempts by psychologists to move beyond questions of mechanism and ontogeny to consider other levels (in this case evolutionary history). History, however, suffers from all the worst faults so beloved of the sociologists of knowledge: our understanding of it is limited by the information we happen to have available, thus making it especially susceptible to "just-so" storytelling. This is no criticism: it is simply a fact we have to cope with as best we can. It does, however, place cautionary markers against some of Donald's interpretations of the sequence of events.

Among the elements in the story Donald tells which I would regard as doubtful are: Lovejoy's (1981) claim (p. 105) that monogamy evolved early in the hominid lineage, possibly even as early as 4 million years ago (the anatomical evidence makes monogamy implausible before the appearance of *Homo sapiens* 250,000 years ago, and Lovejoy's argument in any case conflates pairbonding with monogamy); the claim that *Homo erectus* engaged in organized group hunts (p. 175) is very doubtful (and in any case, if true, it refers only to the very latest members of this species at the point of transition to *H. sapiens*); the claim (p. 186) that rhythm is unique to humans (gelada baboons clearly exhibit it in their contact call exchanges, as shown by Richman [1987] - though I accept the point that only humans use movement of body parts to maintain rhythm); and the claim (p. 215) that language evolved as a tool for thinking about the universe.

This last claim, I believe, conflates two rather different features of language, namely, the fact that we use language to exchange a great deal of information about ourselves and other people (the exchange of *social* knowledge) and the fact that (occasionally) we use language to formulate and exchange knowledge about the nature and structure of the physical world in which we live. We are undoubtedly impressed by the achievements we have produced with the latter (it has, after all, given us religion, philosophy, and science). But our self-congratulation overlooks the fact that these activities are the products of an insignificantly small number of individual minds. It is quite clear from a great deal of research on conversational analysis (including my own) that ordinary people do not often talk about

such lofty topics (indeed, the great majority probably neither care nor concern themselves with the findings of science or the disputations of theologians). What ordinary men and women talk about most of the time is the social world in which they live. Language, as I have argued elsewhere (Dunbar 1993 [target article, this issue]), surely evolved to facilitate the bonding of social groups. The symbolic content of language is essentially a byproduct of a window of opportunity opened up by the evolution of a large computer intended to perform another (social) task. To argue otherwise is to demand the biological equivalent of a jumbo jet to assemble itself spontaneously within the grounds of a medieval castle.

This is not to suggest that I think Donald is wrong in his general account: on the contrary, I believe he has the story more or less right, and it is a story that makes a great deal more sense of the available evidence than any others so far proposed. My point is simply that I believe he has left out a major component of the story, and that is the extent to which the social world dominates the cognitive lives of primates. Primates, as Cheney & Seyfarth (1990; see multiple book review, *BBS* 15(1) 1992) among many others have repeatedly pointed out, are very good natural psychologists, but rather poor ecologists: their understanding of the social world is much better than their understanding of the physical world. Similar claims have of course been made on behalf of humans (e.g., by Cosmides & Tooby 1989).

I found Donald's emphasis on abstract and objective thinking and his failure to consider social thinking one of the few really disappointing features of the book. This failure, I think, leads him to assume that (symbolic) language (as a product of stage 3 mind) evolved at a rather early stage in human evolution (namely, about 250,000 years ago). I think this implausible, if only because the archaeological record shows no evidence for symbolic thought until the Upper Palaeolithic Revolution some 200,000 years later. A more plausible argument (elaborated in detail by Aiello & Dunbar 1993) is that language (as a medium for the exchange of social information) evolved with the appearance of archaic *Homo sapiens* 250,000 years ago, but that language in the symbolic sense did not evolve until the appearance of anatomically modern humans 150,000 or so years later.

My only other criticism of the book really stems from Donald's failure to address the last (and in many ways most important) of the four questions that biologists always ask; namely, *why* did language and the modern mind evolve? Functional questions are all but absent from this otherwise stimulating account. Why did early hominids (but not other apes) require stage 2 minds? Why did modern humans need a stage 3 mind? These questions are fundamental with respect to understanding both evolutionary history (the sequence of events: why then and not later?) and the constraints imposed on further evolutionary change (why are we so limited in many of our cognitive abilities?).

These are mere quibbles with the details, however. Merlin Donald has clearly done a splendid job in synthesizing and making sense of a substantial scattered literature. His is a book that we will all, biologists and psychologists alike, benefit from reading.

## From mimesis to synthesis

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I will review Donald's book from two perspectives. One is as a computer scientist with general interests in intelligence. The other is a much more focused view based on a current project on the acquisition of natural language spatial concepts and grammars. From my generalist perspective, the book is an ideal

choice for a *BBS* multiple review because it is unlikely that anyone besides Donald has tried to synthesize the same slice through the relevant disciplines. In the areas most familiar to me, the book is usually not exactly right and not fully current – in short, not authoritative. I plan to return to the book after seeing reviews from experts in other fields. But only one of the weaknesses I noticed seemed fundamental to the points being made and my working assumption is that Donald got it basically right in other fields as well. If so, the book is a major contribution to our thought about the origins of language. The claims and the reasons for making them are presented clearly and with an appropriate degree of qualification. I found the mimesis hypothesis and the role of external storage in the third phase quite plausible but the mythology story totally unconvincing – nothing like an adequate selectional advantage for myths is demonstrated. But it doesn't much matter; the book provides a framework for continuing interdisciplinary work on the origins of language that was previously missing, at least for me.

From the perspective of my current research on language acquisition, the book was less satisfying. For many of us, the central question is how to reconcile neurobiological reality with the information processing models of traditional computer and cognitive science, corresponding roughly to Donald's "external symbolic storage" chapter. It eventually occurred to me that the book was incoherent on this issue because Donald just retells each story in its original framework. Simply put, the book is a scholarly survey rather than a scientific synthesis. I look forward to a sequel, by Donald or anyone else, that attempts to formulate a consistent model of the modern mind that respects its origins. The current book is still at the mimetic stage.

The major computational error of the book lies in its overestimation of the state of distributed computing systems. Computer networks are currently used almost exclusively for communication and no one knows how to use a network effectively in concert on general tasks. In fact, one suspects that Donald has never tried to construct a distributed system or to manage anything of scale. His vast underestimation of the difficulties of coordinating multiple agents (human, machine, or hybrid) might be part of the reason he misses the obvious and traditional explanation of the second stage of language development – the support of ever more complex group action.

## Evolution needs a modern theory of the mind

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*Origins of the Modern Mind* appears to suffer from at least three basic limitations. First, distinctions Donald draws between episodic, mimetic, mythic, and theoretic are neither fully developed nor adequately justified. Second, the referential (or "propositional") model of language he adopts needs to be replaced by a more adequate "speech act" conception. And third, no theory of the origins of the modern mind can succeed without an acceptable conception of the nature of mind. Since I have addressed the first two problems elsewhere (Fetzer 1993), in the present context I want to focus upon the third.

Although Donald does not elaborate the precise nature of the mind, he does provide examples of the kinds of functions he takes to be characteristic of cognition. These include imitation, focused attention, memory, dreaming, imagination, reasoning, caution, tool usage, abstract intelligence, self-consciousness, and various social and moral capacities, encompassing social cooperation, mutual defense, social bonding, and social intelligence (pp. 28–31). Among these, the one on which Donald places greatest emphasis is memory, where his "stages" in the

emergence of the modern mind involve increased memory storage and retrieval capacities.

Donald's use of these specific aspects of human and animal activities may be appropriate as examples of human and animal cognition, but without an explicit conception of the nature of mind (which he does not provide), it is impossible to tell. A more adequate account of the emergence of the modern mind, therefore, might benefit from the introduction of a modern conception of the nature of the mind. An account that promises to serve the function of providing a framework for understanding cognition as an evolutionary phenomenon can be developed on the basis of the theory of signs proposed by Charles S. Peirce (Fetzer 1988; 1989; 1990; 1991; 1992).

According to Peirce, a *sign* is a something that stands for something (else) in some respect or other for somebody. Inverting and generalizing Peirce's account makes available the conception of a *mind* as a something for which other things can stand for other things. Minds thus become the kinds of things that are capable of utilizing signs. Indeed, since Peirce suggested there are three basic kinds of signs – where *icons* stand for other things because they resemble them, *indices* because they are their causes or effects, and *symbols* because they are habitually (or conventionally) associated with those other things – there seem to be at least three corresponding kinds of iconic, indexical, and symbolic minds.

Things that are capable of utilizing signs that stand for other things because they resemble those other things (as different instances of the same shapes and sizes, for example) thus have the most basic kind of *iconic mentality*. Things that are capable of utilizing icons and signs that stand for other things because they are causes or effects of those other things (such as food standing for satiation of hunger, for example) have a higher grade of *indexical mentality*. Things that are capable of utilizing icons, indices, and signs that stand for other things because they are merely habitually associated with those other things (such as words in ordinary language) have an even higher grade of *symbolic mentality*.

An extension of Peirce's view suggests that there are higher modes of mentality: the capacity for formal reasoning, especially inductive and deductive reasoning on the basis of rules of inference, distinguishes *transformational mentality*, and the capacity for criticism (of ourselves, our methods, and our theories) exemplifies the highest grade of *metamentality* within the scope of this conception. For all five kinds of mentality, the same criterion serves as a usually reliable but not infallible indicator of the presence of mentality, namely: *the capacity to make a mistake*, because, in order to make a mistake, something must have the capacity to take something to stand for something, while doing so wrongly.

This framework can be applied to Donald's examples of cognitive functioning. Mental functions such as focused attention, memory, and dreaming could be properties of iconic minds (of type I), since the objects of focused attention, memories, and dreams might be merely images – perhaps sequences of images – that resemble what they stand for. Functions such as tool usage, imitation, and self-consciousness, by comparison, seem to require indexical minds (of type II), because they involve comprehending cause-and-effect relations of various kinds. Imitation is an interesting example; it also appears to involve some analogical reasoning capacity.

Social cooperation, mutual defense, and social intelligence (which ants, termites, wasps, and bees display) may or may not require mentality that goes beyond the indexical, especially when they are instinctual behaviors. Indeed, the difficulty encountered in evaluating whether and to what degree functions of these sorts involve mentality is that it depends on the character of their sign-using (or "semiotic") ingredients. Without information of this kind, it is difficult to say. Abstract intelligence and reasoning seem to go beyond indexical mentality to the level of symbolic minds (of type III). Indeed, when

reasoning takes the form of dependence upon rules of inference (as in the construction of deductively valid or inductively proper arguments), then transformational minds (of type IV) are involved.

What turns out to be most intriguing about caution, from this point of view, is that it appears to exhibit the exercise of the critical capacity indicative of metamentality (of type V). Since prudent behavior can result from behavior-shaping experiences (of the kind that operant conditioning, especially, can produce) as well as from critical reflection on alternative beliefs and behavior, however, this case too requires further contemplation. The distinctive feature of metamentality is the use of signs to stand for other signs (using words to talk about movies, for example). Unless signs are being used to stand for other signs, prudent behavior need not be of type V.

If birds can mistake the shapes and sizes of vinyl owls for the shapes and sizes of the real thing, if dogs can salivate at the sound of a bell as if it were going to satiate their hunger, and if pigeons can press bars in the false expectation of receiving pellets, for example, then things of each of these kinds can make mistakes and have minds. From this perspective, I would suggest, the concept of minds as semiotic (or "sign-using") systems affords a framework for understanding the evolution of minds of successively stronger and stronger kinds that promises to go far beyond the distinctions Donald has drawn in his extremely stimulating work.

It seems plausible, for example, that different species have distinct semiotic abilities in the form of distinctive ranges and capacities for using signs of various kinds. Some of these semiotic abilities may be inborn (or innate), whereas others are learned (or acquired). Presumably, if the semiotic systems conception is right-headed, lesser forms of life should exhibit lesser kinds of mentality and higher forms of life higher forms of mentality, where their exact range and variety depends upon specific social and environmental variables. The semiotic abilities distinguishing various species may even turn out to be the key to their behavior.

### Cultural transitions occur when mind parasites learn new tricks

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Donald suggests that the history of human culture should be classified on the basis of cognitive stages instead of in terms of technology or religion or manner of obtaining food. I find this suggestion sensible, as enhanced ability to build internal representations underlies progress in any of these realms. Donald proposes that human culture has undergone three fairly sharp transitions during periods of reorganization of brain architecture, characterized by the emergence of increasingly sophisticated modes of representation. These are (1) mimesis (the ability to represent knowledge through voluntary motor acts), (2) spoken language, and (3) graphic invention, external memory, and theory construction. Donald's account of this process is plausible and well-documented. He could be more explicit, however, about how and why our modes of representation became more sophisticated. Showing that we had the *capacity* and *need* for more complex representations is not enough to explain why we came to possess them; it is necessary to outline the mechanisms at work that ensured their creation and evolution.

Donald's arguments might be strengthened were he occasionally to relinquish the focus on humans as the active "evolvers" of culture. Cultural evolution, like genetic evolution, involves the generation, selection, and differential reproduction of patterns. Once a pattern has the ability to self-replicate with variation,



and does so on the basis of some selection criterion, then the process of evolving has momentum. This is true whether the patterns are implemented in genetic material or as patterns of activation across neurons. One could make a good argument that there is no more reason to credit humans as the evolvers of cultural evolution than there is to credit DNA as the evolver of genetic evolution. We can be seen as mere hosts for the representation and replication of "idea-parasites"; we are the medium by which one evolutionary process has become superimposed upon another. Our relationship with idea-parasites is symbiotic, analogous to the relationship we have to the bacteria in our gut or the viruses that have inserted themselves in our genomes. Those that evolve in directions that benefit us flourish, whereas those that evolve in directions that harm us lose the hardware upon which their livelihood depends. Our well-being provides idea-parasites with the fitness landscape that guides their evolution. When we view ourselves as the substrate for a relatively autonomous process, we see why there is a close relationship between the architectural complexity of the brain and the complexity of culture. Much as the products of biological evolution adapt and evolve without top-down instruction in response to changes in their environment or in the stuff that encodes or implements them, ideas adapt and evolve in response to changes in the needs and skills of their hosts or in their hosts' brain architecture. Extending this line of thought: during each of Donald's cultural transitions, as the representational capacity of the brain expanded, idea-parasites acquired the opportunity to travel new evolutionary trajectories, broadening the space of viable "conceptual niches" to encompass events that could be communicated through mime, through speech, and through artifacts and theoretical analysis. This is not unlike the spread of seeds to regions with different climates and different resident flora and fauna, which exposes them to new selective pressures that broaden the space of viable genotypes.

Donald argues that the difference between human culture and that of the great apes is vast (p. 161) and that humans are unique in evolving a "generalized capacity for cultural innovation" (p. 10). Apes are capable of mimetic representation, but this remains strictly episodic, whereas for young children, "the practice, rehearsal, and refinement of action takes on a generative property; the same elementary actions . . . may be combined and recombined into sequences that represent events" (p. 172). The same is true with language. Although apes can link signs to signifiers they do not have the capacity for linguistic innovation. By contrast, for children, "the first word inventions . . . have the quality of an intellectual adventure . . . capturing a chunk of episodic experience, or a concept, with a word requires experimentation" (p. 218). Donald states that the crucial difference between apes and humans is that humans are capable of semantic memory, which depends upon a distinctively human representational system (p. 160). However, he does not relate the absence of semantic memory to any breakdown or bottleneck in the cultural evolution process (the generation, selection, or differential reproduction of patterns).

Although this might also be clarified by drawing upon our analogy between culture and biology, what is interesting here is the way in which the analogy breaks down. In biological evolution, the generation of variants is largely random and precedes selection. The success of the process can be attributed to the sheer number of variants generated. In cultural evolution randomness plays a smaller role; innovation is guided by an internalized model of the world that is continually honed by experience. The success of this process can thus be attributed to internalization of the fitness landscape, enabling the variation phase to be merged with the selection phase so that only need-fulfilling variants are generated. This merging is made easier by the nature of brain-style representation. Distributed networks naturally complete partial patterns and generate prototypes by tweaking and combining patterns from memory, and the pat-

terns they draw upon – the ones that are most easily activated – are likely to be relatively successful, as they will have been used most recently or frequently. Thus not only do newly generated idea-parasites "play new tricks," but they tend to play tricks that are useful or satisfying to their hosts. Perhaps apes lack the necessary feedback loops, control mechanisms, or whatever it takes to carry this off successfully, whereas humans, with practice, learn to do it with ease.

## Working memory and its extensions

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Cognitive psychologists are frequently exhorted to look beyond the laboratory and Donald has done so on a large scale in his fascinating exploration of cognitive evolution. Many of the concepts used in this speculative but much-needed endeavor derive from laboratory studies. Working memory is a prime example and I will focus on Donald's proposals regarding working memory and the role of external symbolic storage systems (ESSs) and external memory fields (EXMFs) in thinking.

Recently there has been a growing interest in the role of working memory in thinking and particularly in problem solving. Hitherto, it has tended to be simply assumed that working memory limitations constrain possible strategies in problem solving; there has been no detailing of how working memory is used on the way to a solution. From the work of Baddeley (1992) and colleagues it seems clear that working memory needs to be considered as a composite system involving specialized memory subsystems (visuospatial scratchpad and articulatory loop) plus a coordinating central executive mechanism. Thus, in considering working memory in thinking, one must address the roles of the subsystems. Dual task methods assist such detailed investigation; we have some sample results that indicate roles for the articulatory loop and central executive in syllogistic reasoning but not for the visuospatial scratchpad (Gilhooly et al. 1993). It has also become clear that the limits and fragility of working memory as shown in memory tasks pose great difficulties for its effective use in problem solving. As Newell (1992) and Broadbent (1993) have recently commented, no AI problem solver succeeds with such extremely limited working memory capacity as has been attributed to humans.

Of course, the apparently narrow limits of working memory fit well with Donald's stress on the role of ESSs and particularly of EXMFs in problem solving. Simon (1981, Ch. 4) has also pointed out the benefits of external memory; and Suchman's (1987) situated cognition approach, too, stresses the role of external props in complex real-life problem solving. It would indeed be difficult to deny that external memories are in practice of great importance; however, the balance may need redressing in that internal means of extending working memory have not been given much weight in Donald's treatment. It should be noted that the low values often reported for working memory capacity are based on short-term memory tasks involving the presentation of meaningless sequences of items. If the items can be meaningfully structured by the subject then short-term memory performance can be enhanced dramatically, as in Ericsson's (1985) studies of expert memory, in which digit spans of over 80 were obtained. Furthermore, even without specific mnemonic training, experts, when compared to novices, always show superior memory for new material in their field after a brief exposure. This suggests a greatly expanded effective working memory capacity for material in the domain of expertise. This result is highly robust and has been found over many domains since De Groot's (1965) well-known pioneering demonstrations in chess. It is plausible to suppose that during problem

solving too, experts can effectively extend their internal working memory capacity and store intermediate results generated internally. In that way extensive search is possible for experts without reliance on EXMFs. Blindfold chess is perhaps an extreme example of extensive mental search without external memory.

An explanation for extended working memory has been put forward by Ericsson and Kintsch (1991) in terms of Ericsson's (1985) Skilled Memory Theory. Essentially, the explanation is that material is efficiently coded into long-term memory in such a way that rapid retrieval into short-term working memory is facilitated. The expert overcomes the limits of working memory by frequent swapping of information between long-term and working memories as problem solving proceeds. Thus, before ESSs and EXMFs were widely available, archaic experts could still make progress in their domains by using internal extended working memory.

## Mythos and logos

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The stages Donald proposes for the evolution of the modern mind seem basically sound in outline. His suggestions are not altogether new. Gordon Hewes, for example, has for twenty years been advocating a mimetic-gestural stage in human evolution (Hewes 1973; 1974), and Goody (1977) has long emphasized the importance of external symbol storage in the transformation to theoretic culture. Fairservice (1975) outlined a similar three-stage theory of cultural evolution, labeling the stages enactive, iconic, and symbolic after Bruner's developmental sequence. But Donald's exposition is richly embellished, particularly from the literature of the cognitive sciences, and lucidly argued. There are, however, some weaknesses in the presentation which, though not seriously damaging to the overall scenario, are perhaps worth pointing out. I would mention first the characterization of the Upper Paleolithic and "mythic culture" generally, and second, the imputed causes of "theoretic culture."

In the few pages discussing Paleolithic art (pp. 279-84) there is hardly a sentence that is not dubious or merely wrong. Whether any of the decorated caves had ceremonial or ritual significance is completely unknown (in the case of Altamira perhaps, for hundreds of others unlikely). That hunting and fertility constitute "the two major themas" is a very outdated notion, for which there is virtually no evidence. That these themas are "usually cast in mythic or narrative terms" is a generalization that has no basis in the art itself: before the Mesolithic paintings of the Spanish Levant, there are at best but a handful of depictions, out of many thousands, that even suggest a narrative content. And of Paleolithic myth we have absolutely no knowledge at all. Yet Donald asserts that these pictorial representations "appeared in the context of an existing oral-mythic culture."

Upper Paleolithic culture in general, he claims, "had a rich social and religious life, marked by the use of dance, chants, masks, and costumes for various religious performances." People had "capacious verbal memories, capable of long, highly formalized verbal exchanges." They also had political structures and "various semiotic devices to indicate clan, status, and totemic identification" (p. 211). All of this is possible, but there is no archeological evidence to warrant any of these confident declarations. Then where do they come from? Clearly from ethnographic analogy, the characteristics of modern "Stone Age" societies being retrojected on the Upper Paleolithic. If the Kalahari Bushmen and the Australian Aborigines have myth,

ritual, and the rest, the Cro-Magnons must have had them too. Needless to say, this is a precarious reconstructive procedure.

Similarly, we read, "Language, in a preliterate society, . . . is basically for telling stories . . . . Narrative is so fundamental that it appears to have been fully developed, at least in its pattern of daily use, in the Upper Paleolithic" (p. 257). Moreover, "Myth is the inevitable outcome of narrative skill and the supreme organizing force in Upper Paleolithic society" (p. 258). It is true that storytelling is a common use of language in oral societies, but it has many other uses as well. How can we conclude that this is what language is basically for? And again we have unjustified extrapolation from ethnography to the Paleolithic. The fact is, we do not know whether the Cro-Magnons even had a fully developed, grammaticized *language*, let alone a mythology. It may be doubted, moreover, whether myth has ever been "the supreme organizing force" in *any* society. Even in the most myth-ridden cultures, it seems pretty clear that most people have gone about most of their business most of the time without much thought about their myths. Indeed, myths have probably always been as much a form of entertainment as anything else (as among American Indians, for instance).

But let the Upper Paleolithic rest. After all, Donald's period of "mythic culture" extends well beyond that era right up to classical Greece, and certainly all its characteristics were fully developed by then, whenever they may have begun. When myth does develop, it does often have an integrative function. But it is only one expression of that function, and I see no *prima facie* reason to accord it "pre-eminence" (p. 215). Donald suggests the possibility that integrative *thought* was the primary human adaptation rather than language *per se*, which developed in response to pressure to improve the conceptual apparatus (p. 215). Alternatively, we might suppose that it developed in response to social pressure to improve the communication of concepts. Most likely, conceptual and linguistic systems evolved by reciprocal bootstrapping (Pinker & Bloom 1990; Jackendoff 1990). In any case, "mythic culture" seems a misleading designation for what might better be called "oral culture" (which in fact Donald sometimes calls it).

My second reservation concerns the external symbol storage system (ESS) and the rise of "theoretic culture." In almost any society, preliterate as well as literate, knowledge is differentially stored among its population, so that any one individual depends on others - priests, law-speakers, and bards, say - for their special memories. External storage in accessible symbol systems, especially writing, does greatly increase the holding capacity of collective knowledge, but whether the difference is more than quantitative is not clear. That the ESS played a very significant role in the Greek Enlightenment is doubtful. I think most classicists would agree that even in the fifth century Greece was very largely an oral society (Havelock 1963; Thomas 1992), and the growth of reflective thought seems not to have depended seriously on the presence of writing.

In the modern world, the EES does indeed have an enormous role in the advancement of knowledge, particularly in those many intellectual and technological enterprises that are data-driven. On the other hand, philosophical thought is far less dependent (one would hardly guess from his writings that Wittgenstein had ever read a book), and even theoretical physics, with its beloved thought experiments, does not seem powerfully beholden to such systems. Theoretic culture does exist, but to characterize it as a revolutionary "symbiosis" of individual minds and external symbol storage systems may be somewhat exaggerated.

## 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 re-

sponse, 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 conven-

tion 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 onomatopoeic 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 life-forms 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 Tāi 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 Tāi 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, prearithmetical "notation" (Marshack 1991c). That

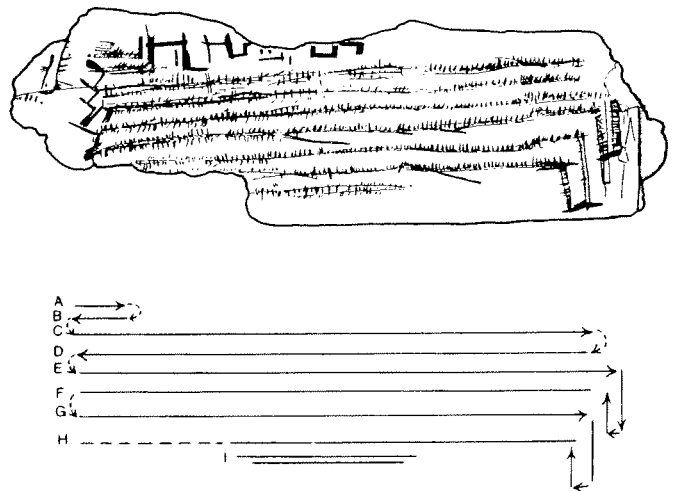


Figure 1 (Marshack). Top: Upper Paleolithic prewriting, prearithmetical notation for 3½ years incised on a fragment of scrap bone, from Tāi, France, terminal Magdalenian period, ca. 10,000 B.C. The accumulation documents a complex sequence of changing visual problem-solving, cueing, and abstracting strategies. 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 Tāi 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 chil-

dren" (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, tool-making, 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 non-symbolic 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 sys-

tems 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 & Joannette 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 ontogenetic 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 Joannette 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,<sup>1</sup> 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 Frigaszy (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 per-

ceptual 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., Vygotsky 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 dis-

puts 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 forbid 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 investi-



gators 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 Vaclair & 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 homi-

nids, 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 ge-

netically 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 & Joannette 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. Pre-literate 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. Aborigines 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 encap-

sulating 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 ap-

proach; 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 mean-

ing 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 dis-



cussions 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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