

Continuing Commentary

Commentary on **Merlin Donald (1993) *Précis of Origins of the modern mind: Three stages in the evolution of culture and cognition*. BBS 16: 737–791.**

Abstract of the original article: 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.

Did courtship drive the evolution of mind?

Eric B. Baum

NEC Research Institute, 4 Independence Way, Princeton, NJ 08540

eric@research.hj.nec.com

Abstract: The driving force in the evolution of language and the human mind was the advantage gained in courtship by efficient communicators. Ontogeny recapitulating phylogeny may offer an alternative to Donald's picture of the relative origins of language and mimesis. Very recent evidence is pertinent to dating the origin of language.

Theories of the origin of the human mind (see e.g., Clutton-Brock & Harvey 1980; Darwin 1874; Dunbar 1988; Milton 1988) typically stress the utility of improved intelligence for survival, for example, tool making, gathering fruit, hunting, war. Donald (1991) stresses the role of communication in organizing society, explicitly and implicitly emphasizing the survival values of such organization. This commentary discusses an alternative scenario: that courtship drove the evolution of human communication much as courtship drove the evolution of the peacock's tail.¹ In this view it is in some substantial part an accident that language and large brains are useful for survival. By contrast, the peacock's tail is believed to be created by similar evolutionary forces, is also remarkably large and beautiful, but is presumably detrimental to survival of the species.

Was language evolution driven by courtship? Courtship is presumably the driving force in the evolution of bird song. As with language, the evolution of bird song has produced elaborate modifications to both the sound-producing apparatus and the bird brain. The mental abilities of parrots, for example, are prodigious; it has recently been claimed that they are capable of learning (to a degree) to count, make relational class distinctions, and respond to complex questions in English (see Pepperberg 1994). Parsimony suggests (but of course does not compel) a common cause for the remarkable evolution of parrot and human.

Courtship-driven evolution is perhaps the primary influence on mammalian evolution in general. The elephant seal provides an

extreme example. Elephant seal males, who must fight off other males to ensure a harem, have evolved to be four times the size of females. More generally, mammal species frequently grow larger with time, while the average size of mammals stays roughly constant, due to extinction of large mammal species. One explanation is that selection pressure of competition for mates presses mammals to grow larger, at the expense of the survivability of the species.

Positive feedback effects in courtship-related evolution are theoretically well understood (e.g., Dawkins 1986; Fisher 1980; Lande 1981) and presumably explain such bizarre evolutions as the peacock tail and the tail of the African long tailed widow bird. Say a gene developed in some women which led them to prefer men who could communicate well with them. (We assume that most fertile women have children but not all men do, as is common in birds and quite plausible in people, especially as some men are cuckolded.) Since these women choose mates who communicate better than average, their offspring are likely to carry genes both for being good communicators and for preferring good communicators as mates. So their male children, for example, will pass on to their female grandchildren a greater than average tendency to prefer good communicators as mates. This can lead to a positive feedback effect which can push rapid evolution of even such traits, for example the peacock's tail, which are detrimental to the survival of the individual carrying them.

Features harmful to survival provide the clearest examples of courtship-based evolution because it is difficult to imagine how else they could have arisen. Such features are typically present only in males. Females do not need the feature, would be endangered by it, and thus do not possess it. By contrast, an enlarged intelligence would have been useful to both sexes. Whatever the cause of its original evolution, we might expect to find both sexes so gifted.

Donald notes that Neanderthals, who lived in the period between 150,000 years ago and 35,000 years ago, appear from skeletal evidence not to have possessed modern vocal apparatus. Thus he argues that language did not develop much before 200,000 years ago. Donald relies on recent mitochondrial DNA

studies, which claimed that present-day humans all had a common ancestor 150,000 years ago, to argue that language had started to evolve by that time. Unfortunately, since publication of Donald (1991) this mitochondrial evidence has been retracted (e.g., Hedges et al. 1992; Templeton 1992) so that it might now be plausible that language evolved only 50,000 years ago or so, when very rapid cultural and toolmaking progress began. More recent mitochondrial evidence supports this possibility (Harpending et al. 1992). The courtship mechanism discussed here is capable of very fast evolution, particularly when one realizes that it can reinforce other mechanisms based on survival advantage.

Darwin (1874) and Donald (1991) argued that "mimesis" must have evolved before language, in part on the grounds that language is so much more precise a communication medium; mimesis could not evolve after language, but must instead be an evolutionary vestige. At first glance, Donald's "mimesis first" proposal seems almost tautological, since he doesn't offer any formal definition of mimesis, but operationally uses the term to mean any communication or reasoning skills not involving language on the one hand, and of which apes are incapable on the other. Reasoning apes are capable of what Donald dubs "episodic" reasoning, arguing that their memories include only specific events, rather than generalizations. But this ignores the issue of how animals generalize from one event to another. Modern research on machine pattern recognition has shown that this is a complex problem, which animals seem to solve in sophisticated ways.

The old rule "ontogeny recapitulates phylogeny" suggests an alternative picture. Note that the problem that evolution and a child solve are related; Donald himself argues that language would not be stable until the mind had the ability to invent it; and evolution must always build on what has come before. Children learn to speak gradually, learning "mimesis," and their conceptualization of the world, simultaneously. Are apes significantly stupider than children at the point where children's speech becomes equal to the best that apes can attain? Mechanical production of speech is one of the hardest things for children to master, paralleling the anthropological record and perhaps explaining why the speech centers developed in the left hemisphere, near the motor areas (although Donald, with his alternative view, explicitly rejects this possibility [1991, p. 71]).

Courtship advantages could plausibly drive the evolution of mimesis after, before, or during evolution of language. There are alluring connections between courtship and mimesis and the arts. For example, stone age sculptures were primarily either overtly sexual female figures or aroused males. Throughout history, much poetry has been written by men and appreciated by women, and has included a large number of explicit love poems. Popular musicians (e.g., the Beatles) seem to be able to bypass the normal intellectual centers and arouse hysterical passion.

Why did humans evolve greater intelligence and communication capabilities than other primates? It might just have been luck, coupled with positive feedback. Another observation, however, is that, since it is not evident when human females are fertile, a dominant male can't readily prevent his mate from seeking other attention. By contrast, among chimpanzees (Goodall 1986), the dominance hierarchy appears to be decided among the males largely, though not entirely, on the basis of fighting ability. The days of her cycle (known as periovulatory period or POP) when a female is fertile are evident to the males. The alpha male has the capability of monopolizing a female, and higher ranking males sometimes follow a particular POP female around trying to inhibit copulations by lesser ranked males. Lower ranked males do sometimes mate, however, and the correlation between dominance and offspring is unknown.

Alternatively, one might speculate that human females gained more control of mate selection when humans became bipedal, perhaps because of adopting a frontal coital position. Whatever the cause, it is clear that in the most mammalian species pecking order is determined among the males without consulting the females

and largely on the basis of brute force. Human females exercise choice and typically select mates based on other criteria.

In human cultures, "sensitivity," intelligence, and communication skills are important in courtship, and also major factors in overall human "pecking order," itself important in winning mates. Evidently this must have had an evolutionary impact, although the relative evolutionary importance of advantages in courtship and advantages in survival may be debated.

Donald also discusses work by Bruner (1986) distinguishing two major modes of thought: the narrative and the paradigmatic. Narrative imagination constructs stories and myths. Paradigmatic imagination seeks logical truth. The courtship-based evolutionary scenario has important consequences for this analysis. On any given political topic², half the people may firmly hold one opinion and half the opposite, both convinced they are logically correct and their opponents idiots. A plausible alternative is that neither is thinking rationally, and that their perception of this is blocked by defects in their cognitive process. Even in science, the archetypal domain of paradigmatic thought, much of the literature and the funding is determined by myth and fad. If our minds evolved primarily for the attraction of mates, rationality was not the primary design consideration. Is the peacock's tail rational? Thinking about the evolution of mind should lead us to reexamine our own beliefs and mental processes.

NOTE

1. After submission of the commentary I became aware that a similar thesis has been advanced by G. Miller and discussed in Ridley (1993).

2. Note also that expression of political opinions, especially among students, has characteristics of sexual display and that people rarely choose mates whose politics they disagree with.

On Neanderthal speech and human evolution

Philip Lieberman

Department of Cognitive and Linguistic Sciences, Brown University, Providence, RI 02912-1978. lieberman@cogvax.cog.brown.edu

Abstract: Loring Brace's assertion that "Neanderthals were just as capable of articulate speech as we are" reflects ignorance of speech anatomy and physiology. Metrical analyses of hyoid bone morphology cannot predict supralaryngeal vocal tract (SVT) shape. Houghton's (1993) "modern" Neanderthal SVT reconstruction yields an impossible creature who had a larynx positioned in his chest. The reconstructed modern SVTs of early fossil *Homo sapiens* indicate brains that can regulate speech, consistent with Merlin Donald's timetable for the evolution of language.

Loring Brace's (1993) comments on Donald (1993b) concerning the evolution of speech do not take into account the anatomical and physiological bases of speech. He suggests that Neanderthal speech capabilities were similar to those of modern humans. Brace apparently believes that the Kebara hyoid bone described in Arensburg et al. (1989; 1990) is the fossil's "voice box" – that is, the fossil's larynx, which is "indistinguishable in form from those of modern humans" – and that this shows that "Neanderthals were just as capable of articulate speech as we are." Unfortunately, none of this is correct.

First of all, the hyoid is a bone positioned above the larynx, connected to it by ligaments and muscle. The larynx (Brace's "voice-box") is a complex structure of cartilages, muscles, and other soft tissue. No "completely preserved" Neanderthal larynx has ever been recovered. Nor is it likely that any fossil larynx ever will be recovered. Moreover, the metrical analysis of hyoid bone morphology presented in Arensburg et al. (1989; 1990) argues against its being "indistinguishable" from a modern human hyoid. For two of the six variables originally measured by Arensburg et al. (1989), the Kebara hyoid was more than three standard deviations away from modern hyoids (in particular, it is completely outside the range of modern humans). Perhaps not surprisingly, data for these two

variables were omitted in Arensburg et al. (1990). Furthermore, the remaining metrics presented in the 1990 paper, grouping the Kebara hyoid with modern human hyoids, are still not persuasive, because the authors are unable to distinguish human hyoids from the hyoids of other mammals. A test of the methods used by Arensburg et al. (1990) shows that these linear measurements identify pig hyoids as modern human (Laitman et al. 1990).

As for the physiology of speech, the larynx is not the physiological component that accounts for the probable differences between modern human and Neanderthal speech capabilities. The distinctions that are in dispute derive from the airway above the larynx: the supralaryngeal vocal tract (SVT). In normal modern humans the SVT can form the extreme and abrupt discontinuities that are necessary to produce sounds like the vowels [i] and [u], the vowels of the words *tea* and *to*. The vowel [i] is the "supervowel" of human speech and plays an important role in facilitating the process of formant frequency "encoding-decoding" that yields the high data transmission rate of human speech. The human tongue, which has a round contour in a midsagittal view, can generate the SVT shape necessary for producing [i] because half its length is in the pharynx, half in the mouth. The *position* of the larynx is the key to the difference between the SVTs of all other mammals – for example, canids, apes, Neanderthals, and the adultlike modern human SVT. In human newborns, the larynx is positioned close to the base of the skull with the tongue being almost entirely inside the mouth. As Darwin (1859) noted, this yields an airway in which the pathways for food and air are almost independent. In contrast, in normal adult humans the larynx descends to a low position relative to the cervical vertebrae and mandible; the tongue is concurrently reshaped and repositioned to yield the human SVT. Our SVT is better adapted for speech, but we are more likely to choke to death on food lodged in the larynx. The key to the Lieberman and Crelin (1971) Neanderthal reconstruction was the relationship between skeletal morphology and larynx position as it descends in the course of human ontogenetic development. The hyoid bone which supports the larynx also descends, without any known change in its morphology. It is accordingly impossible to determine larynx position and SVT morphology from an isolated hyoid bone (Lieberman 1994a).

Arensburg et al. (1990) based their claim that the Kebara fossil had a human SVT on the assertion that the hyoid and larynx do not shift downwards from the high neonatal to the low adult position. However, this assertion is falsified by all published data. The descent of the larynx in humans after birth has been noted in many independent studies – for example, by Bosma (1975), George (1978), Grosman (1979), Laitman and Crelin (1976), Lieberman and Crelin (1971), Truby et al. (1965), and Wind (1970), and by Senecail (1979), who discusses the descent of the hyoid in detail. In fact, the data of Falk (1975) cited by Brace, contradict another claim of Arensburg et al. (1990): that the position of the hyoid is fixed with respect to the mandible.

The arguments concerning Neanderthal speech often seem to be driven more by theoretical positions than by data or logic. For example, Falk's (1975) argument against the Lieberman and Crelin (1971) reconstruction was that a high hyoid position in humans, similar to that found in chimpanzees, would prevent our swallowing food in an upright position. If Falk's argument were correct, chimpanzees, who habitually feed in an upright position, would likewise be unable to swallow (Lieberman 1982). Houghton (1993), whom Brace cites as a recent assessment of "all the available data," bases his arguments on archaic illustrations from the British edition of *Gray's Anatomy*, which show tiny, distorted tongues, ignoring recent cineradiographic and MRI data that provide a truer picture of tongue and SVT morphology.

Houghton (1993) also unwittingly refutes his own hypothesis that the La Chapelle-aux-Saint's Neanderthal fossil could have had a modern human tongue. One of the many arguments against the Neanderthal's having a modern human tongue is that it would not fit within the Neanderthal neck. The tongue must span the long

Neanderthal oral cavity. Since the oral cavity is long, the radius of a human tongue fitted to the Neanderthal skull must also be long. This places the larynx below the cervical vertebrae in the Neanderthal chest because the length of the human pharynx equals the length of the oral cavity. The "reconstruction" would yield an impossible creature who had a larynx positioned in his chest; no mammal has its larynx in its chest (Lieberman 1984; 1989; 1991). Houghton attempted to fit the Neanderthal with the *Gray's Anatomy* tongue, a human cadaver's shrunken tongue. He positioned the hyoid bone under the mandible, however, following the recipe furnished by Arensburg et al. (1990). This again placed the larynx below the cervical vertebrae but neither Houghton nor Brace appear to have noticed this, because Houghton neglected to complete the SVT to the level of the larynx in the sketch that serves as his Neanderthal reconstruction (Lieberman 1994b). In her recent review, Schepartz (1993), a member of the Arensburg research team, repeats these arguments based on flawed data, superficial analyses, and unfamiliarity with basic speech physiology.

Brace makes a point of differentiating speech anatomy and brains. However, the basis for the heat in the Neanderthal controversy is the linkage between brain mechanisms and speech-producing anatomy (Lieberman 1989; 1991). It is apparent that humans have specialized brain mechanisms that are necessary to regulate the complex, voluntary, articulatory gestures that underlie speech. This is apparent in the fact that chimpanzees are unable to talk. The chimpanzee SVT cannot produce the full range of human speech sounds (Lieberman et al. 1972). However, the chimpanzee SVT could produce nasalized, vowel-reduced speech that, though it wouldn't be as intelligible as normal human speech, would suffice for communication. Yet chimpanzees cannot produce *any* speech, nor can they be trained even to produce arbitrary concatenations of the elemental sound contrasts (phonetic features) found in their vocalizations, resembling similar elements in human speech (Lieberman 1968; 1975). I have therefore argued that the apparent presence of a fully human SVT about 100,000 year ago in fossil hominids such as Skhul V and Jebel Quafzeh is an *index* for brain mechanisms that could regulate human speech. The biological disadvantages of the human SVT (choking on food, impacted molars, reduced chewing efficiency) would reduce fitness unless they were being used to enhance the intelligibility of speech communication (Lieberman 1989; 1991). Given the apparent linkages between the brain mechanisms underlying human speech production, syntax, and certain aspects of cognition in humans, it is probable that hominid neural specializations for speech production enhanced "higher" cognitive functions. Brace (1993) is not quite accurate in his assessments of Neanderthal and early human culture. The early modern Skhul V and Jebel Quafzeh fossils appear to have had the only burials containing grave goods that would have ritualistic significance in present human cultures. The comparatively recent date for the last "transition" proposed by Donald (1993) is therefore consistent with a recent African origin of modern *Homo sapiens*, with evolution that enhanced speech production being a factor in the process (Lieberman 1991).

Does this mean that Neanderthals lacked speech and language? Probably not. I have noted since my first paper on their speech capabilities that the archaeological record indicates that they had some form of language and speech. Their speech was different, however, being less intelligible than ours. Given the genetically isolating factor associated with even minor dialect differences in humans – people tend to have mates who speak the same dialect, even when they have the inherent biological capability of learning any dialect (Barbujani & Sokal 1991) – the physiologic restriction limiting Neanderthal speech could have acted as a powerful genetic isolating mechanism (Lieberman 1992). Thus, a distinct Neanderthal dialect could have played a part in their genetic isolation and eventual replacement by *Homo sapiens*. This hypothesis is clearly inconsistent with Brace's views on human evolution, which fail to differentiate between Neanderthals and other hominid fossil populations.

Taxing memory: Writing, memory, and conceptual change

David R. Olson

Centre for Applied Cognitive Science, Ontario Institute for Studies in Education, 252 Bloor Street W., Toronto, Ontario M5S 1V6, Canada.
dolson@oise.on.ca

Abstract: Writing is important not because it “extends memory” but because it invites new concepts. The basic cognitive resources, including memory, have remained unchanged for perhaps a million years but the objects on which it has to work have changed significantly partly because of writing which has tended to turn speech into an object of reflection and analysis.

Merlin Donald (1991; 1993) has proposed that human cognition has advanced over the past two million years through three stages – prelanguage (mimetic), language (mythic), and writing (theoretic) – by adding, at each stage, an external memory to biological memory. My comment will focus primarily on this third stage, building on the commentaries of Halverson (1993) and Small (1993) by way of a nod to Bickerton (1993).

It is a commonplace to say that such cultural artifacts as writing “extend memory,” but that suggestion is essentially a metaphor. Donald, however, means it literally. Memory is literally larger, he suggests, when we have a written record. To show that he means it literally he calls such knowledge representations as books and maps “external symbol systems” (ESS). “Grammatical conventions, like rhetorical skills, were developed and stored through an ESS loop” (p. 353). Again, he argues “the essential cognitive adaptation underlying each of the three great cognitive transitions in human evolution is a new system of memory representation” (p. 366). For example, rhetoric, grammar, and logic were “invented, tested, and elaborated in an iterative loop involving the linguistic controller (the reader) and an external memory field (including books)” (note Figs. 8-7, p. 354). Just as memory can be “externalized” so also “the thought process itself can be largely externalized and institutionalized” (p. 358).

In their commentaries, Halverson and Small both point out that the role of writing in the “Greek miracle” is probably overstated. Few Greeks were literate, and even those who tended to resort to writing only after the thoughts were formulated. Laws, too, were debated orally and only later, if at all, written down. Halverson points out that “theoretic culture does exist, but to characterize it as a revolutionary ‘symbiosis’ of individual minds and external symbol storage systems may be somewhat exaggerated” (p. 762). Small, too, points out that “an external symbolic store (writing) did not provide the great panacea, but instead forced the Greeks and Romans to improve the only method of retrieval they knew – biological memory” (p. 770). Even documents which were written were rarely, if at all, consulted for the simple reason that retrieval systems – classification, alphabetization, and so on – were nonexistent. The explanation of “theoretic culture” will have to be sought elsewhere. Donald’s response that the Greeks were becoming “more efficient at combining internal and external memory sources” (p. 784) lacks conviction.

My worry about how cultural artifacts related to cognition is similar. Donald is surely right in his general claim that cultural artifacts such as books in some sense alter how we think and what we know. The question is whether these changes are usefully described as changes in the structure of memory. Do boxes and arrows labelled ESS and EXMF play any explanatory role? Is there some explanatory advantage to saying that “children have to be trained for many years (to master) some narrow area of the ESS” (p. 322) rather than “to master some specialized forms of knowledge?” It is not clear that there is. If not, the “extension of memory” metaphor loses its explanatory value.

My view (Olson 1993; 1994) is that what changed hominids into humans was the development of mental resources which made language possible, that is, syntactic resources for relating concepts, as Bickerton argued (p. 749). What changed an oral (mythic)

culture into a literate (theoretic) culture was writing, as Donald argued. However, writing plays its cognitive role not through extending memory – that, in my view, is mere metaphor. What writing can do, I suggest, is bring aspects of language into consciousness. What writing allows, an allowance which may be seized upon only in certain cultural contexts, is the opportunity to think about representations of things rather than things themselves. Let me spell this out briefly.

What was new about “mythic” cultures, it may be argued, is what was attended to, remembered and acted upon, namely, the world as represented collectively in language and ritual rather than the world to which one is biologically pre-attuned. The cognitive resources that allow for the acquisition of a public language also allow for the formation of a “language of thought”; it is that which makes us human. What was new about “theoretic” cultures was that what was attended to, remembered, and acted upon was the form of the representation, rather than simply its content. This is what allows us to formalize our representations into explicit statements, theoretical models, mathematical relations, and the like. In neither case does one require a change in the architecture of memory. What is required is a theory specifying how syntactic representation became possible in the first place and second, how attention came to be directed to different levels of symbolic form. In my own work, I have tried to show specifically how the invention of different writing systems brought previously unrecognized levels of linguistic structure into awareness.

Evolutionarily speaking, the human brain has resources unavailable to our closest primate cousins. We can lie, for example, whereas chimpanzees cannot even tell the truth. But beyond that, human mental resources are much the same everywhere – capable of symbolization, including language, and of entertaining perhaps two or three entities in mind at any one time, linking them into propositions, comparing them, and the like. Cultural evolution has not significantly changed those basic operations. What has evolved is not new memory resources but rather new cultural resources, in particular written language, including mathematics, to which those basic mental operations are applied. Our advantage over stone age man is not that we have bigger or better memories but only that we can apply our equally limited resources to such cultural products as the works of Newton, Vermeer, and Beethoven whereas he could not. Of course, it may be argued that he had better, or at least more important, things to do.

ACKNOWLEDGMENT

I am grateful to Karen Li and to Shaun Maxwell for their comments on this paper.

Episodic is what apes are not

Keith Stenning

Human Communication Research Centre, University of Edinburgh,
Edinburgh EH8 9LW, Scotland. k.stenning@ed.ac.uk

Abstract: Donald presents a three-stage theory of the evolution of cognition – episodic, mimetic, symbolic. This commentary is chiefly concerned with his use of the first term “episodic” which conflicts with standard usage in the memory literature. This conflict of usage has more than terminological implications for Donald’s theory.

The origin of the human mind has to be one of the central questions of cognitive science. This phylogenetic problem has not received the central attention that it deserves from a contemporary community focussed overwhelmingly on synchronic issues about the structure and processes in the modern human mind. This avoidance of the issue has been, for the most part, with good reason. Cognitive science is nothing if not an enterprise of constructing computational theories, and this has begun with a concentration on individual mental processes, but we owe Merlin Donald (1993b) a great debt for following in the footsteps of

Darwin and for formulating the issue of origins in a way that makes it clear what needs to be done to base an explanation on a theory of the computational destination.

Donald adopts Darwin's central insight (from *The Ascent of Man*) that the central innovation in the transition from ape to human cognition is not language but a capacity for inventing representational systems. The evidence that this capacity operated mimetically before it operated symbolically has grown considerably since Darwin's day and is persuasively marshalled here. Innovations of mimetic culture are a necessary basis for the creation of symbolic speech and language. The nonlinguistic core of archaic cognition is an indispensable component of the modern mind. To understand language one must acknowledge this nonlinguistic foundation. The truly modern mind has developed by incorporating external representational systems into its computational architecture.

This is not yet a computational theory. Just as Darwin's other theory of the origin of species provided only a fundamental principle which required a century of analysis of mechanisms to construct modern biology, so Darwin's second theory, of the origin of mind, as developed here by Donald, calls out for computational analysis of its major concepts. Darwin's principle of natural selection was actually inconsistent with his contemporary understanding of mechanism (requiring a Lamarckian explanation of the frequency of occurrence of beneficial variation). We may expect Donald's development of Darwin's theory of mind to be inconsistent with today's embryonic understanding of basic mechanisms. These inconsistencies can only be resolved through a deeper understanding of human computational architecture and its functioning in human minds and societies.

Of Donald's three-stage theory of the evolution of cognition – episodic, mimetic, symbolic – it is the memory innovations that underlie the transitions I wish to comment on here. As Donald says, memory must be the key to understanding representational changes.

Donald's use of the term 'episodic' to describe the cognitive culture of the primate is problematic in relation to what is generally called episodic memory, following Tulving (1983). Donald takes the defining characteristic of episodic memory to be the ability to store and retrieve information from single episodes of experience as opposed to the sort of memory that extracts regularities from repeated types of experience. [See Shanks & St John: "Characteristics of Dissociable Human Learning Systems" *BBS* 17(3)1994]. His theory holds that apes have episodic memory and that the architectural innovation that takes apes into mimetic Homo is the advent of mimetic representation. The innovation which underlies the second transition from mimetic to symbolic culture is then the advent of semantic memory.

This understanding of the memory distinctions is nonstandard, however, and does not seem to fit with the neuropsychological evidence. There are two essential aspects of a truly episodic memory. Episodic memory records the contents of single experiences without repetition. But this capacity alone is possessed by many organisms' memories: the rat that steps off the platform onto the electrified grid retains information about this single experience, but retrieving this information requires cuing by the environment in which the shock was first received. To a great degree this remains true of the apes.

The innovation that produced human episodic memory was that of adding internal control over retrieval from one-shot memory. Donald has an original proposal for the part played by the changes in proprioception and the integration of body image in the development of this uniquely human capacity. Human beings originally gained this controlled access by using the internal cues generated by acting out episodes of the type they wished to cue. The essence of truly episodic memory can be dramatised as the ability to travel in time at will.

As Tulving (1983) pointed out, episodic memory has all the hallmarks of a recent neurological innovation. It is late to appear in ontogeny and fragile in the face of brain injury. In contrast,

semantic memory, which Donald sees as the basis for the second stage transition from mimetic to symbolic cognition, exhibits neither of these properties. It develops earlier in cognition and is relatively robust under injury. The evidence suggests that the wetware implementation of the *memory* component of language capacity is primitive. This fits with the functional facts. Semantic memory stores environmental regularities that are computationally much closer to the primitive elements of prehuman memory than are personal memory episodes. The fact that they are symbolic regularities does not necessarily affect their implementation. This is not to claim that the *contents* of semantic memory do not change with the advent of symbolic representations, but only that their implementation may be primitive.

Perhaps it is merely that Donald's terminology is idiosyncratic (as he admits on pp. 149–150). But if so, his aberration causes trouble. He is puzzled that the "mimetic controller" does not reveal itself through any neuropsychological syndrome, as one would expect of a new wetware innovation. Surely it does. The computational innovation which turns the environment-driven ape into the mimetic human being is the change in the encoding and retrieval mechanisms which establish episodic memory.

Donald's theory of the second transition from mimetic to symbolic cognition also seems sound in outline but idiosyncratic in detail. He sees the "problem of reference" as the key problem that a theory of the development of symbolic cognition must solve and he sees Katz, Quillian and Johnson-Laird as the tradition which developed approaches to this problem. This is a peculiarly psychological and anticomputational perspective. Where, one asks, are Gödel, Tarski, and Montague? The tradition that invented semantics is dismissed out of hand as unable to ground its symbols. Donald does admit that mental models "fail to break out of the self-contained symbolic world" (p. 231) but leaves us wondering why they are so preferable.

Mimetic cognition is capable of reference and is therefore in just as acute a need of grounding its mimetic references. The main "problem of reference" is not that symbols are arbitrary but that both symbol and referents have to be "carved at their joints." This is as much as or more of a problem for mimetic sequences as for sentences. The problem of logical semantics is not primarily that of connecting symbols with the world (which it does with great aplomb) but of implementing them in the mind. Here what is wanted is not a mystical insistence that mental models have "direct access" to their referents but a theory of memory and of symbolic invention which will sustain the computations which go on there.

But it is churlish to complain about this book's computational views. The book's great contribution is its broad outline of a theory of phylogenetic cognitive stages and its persuasive argument that we have enough evidence that a computational reconstruction of prehistory may yet be possible.

ACKNOWLEDGMENTS

The authors gratefully acknowledge the support of the Joint Councils' Initiative in Cognitive Science and HCI through project number G9018050 ("Signal"). HCRC is supported by the Economic and Social Research Council.

Author's Response

The role of vocalization, memory retrieval, and external symbols in cognitive evolution

Merlin Donald

Department of Psychology, Queen's University, Kingston, Ontario, Canada K7L 3N6. donaldm@qucdn.queensu.ca

Abstract: The human vocal apparatus is part of a vertically integrated system, and I agree with Lieberman that modern high-speed phonology

co-evolved with our capacity for grammar. **Olson** and I agree that some distinctly human thought skills appear to be fairly recent cultural acquisitions related to the introduction of new symbolic technologies and external (that is, nonbiological) memory storage. **Stenning's** concern with my use of the term "episodic" can be resolved by distinguishing between episodic storage and retrieval. **Baum's** suggestions regarding courtship and cognitive evolution seem to apply better to mimetic expression than to language.

These four commentaries raise questions about important subsidiary issues encompassed within the theory of human cognitive evolution proposed in my 1991 book. The first, by **Lieberman**, adds another page to the contentious debate over Neanderthal speech. The second, by **Olson**, focuses on the effects of literacy and external memory on human thought processes. The third, by **Stenning**, brings out the problems revolving around the issue of explicit memory retrieval and the use of the term "episodic." The fourth, by **Baum**, discusses the possible relationships between courtship, mimesis, and language. I will respond in the above order.

R1. Neanderthal vocalization and the evolutionary transition to speech

Lieberman's reply to **Brace's** (1993) commentary on my book is entirely appropriate, since much of **Brace's** criticism was directed at my agreement with some of **Lieberman's** claims. To put this controversy in perspective for the neophyte, both **Brace** and **Lieberman** have written other articles making essentially the same points they make here; in fact, the debate over **Lieberman & Crelin's** (1971) claims regarding Neanderthal vocalization has been going on for more than two decades. I have tried to avoid getting drawn in by taking what seemed to me a conservative position, namely, that there was probably some truth on both sides. **Lieberman's** reply speaks for itself. I won't comment on the significance of the infamous Kubarah hyoid bone, but I am persuaded that in itself it cannot really settle the issue of Neanderthal speech. More important, it seems obvious to me that **Lieberman** is right on the axiomatic level: he simply argues that form and function usually go together in evolution. Although it is conceivable that the brain had already acquired modern speech capacity before the final form of the human vocal system was achieved, it is extremely improbable that *after* the central nervous mechanism of speech had already reached its present form, radical (and risky) morphological evolution of both brain and vocal tract would nevertheless continue, without further changes to speech capacity. That is precisely the position, however, that **Brace** seems to have adopted. Few of **Lieberman's** other critics would still seriously claim that Neanderthals have a virtually modern capacity for language, and **Lieberman** has acknowledged time and again that he does not deny them some capacity for speech. The reasoning behind **Brace's** hardline position remains mysterious to me, a mere (but perhaps not entirely innocent) bystander in this exchange.

On the other hand, there is some merit to **Brace's** demand for a longer lead-in time for human vocalization skill, a period during which the genes of archaic hominids "experimented" with various vocal adaptations that culminated in the evolution of speech as we know it. My own position (**Donald** 1991) is that hominid vocalization probably started to evolve towards its present form at least two million years ago, and possibly even earlier than that. But an expanded capacity for voluntary vocalization is not equiva-

lent to a capacity for speech or language. Human vocalization has at least two very distinct realms, prosody and phonology. This distinction between prosodic vocalization and phonological vocalization is not always made in evolutionary theories built exclusively from gross anatomical data, and as a result some anatomists tend to see any major change in the hominid vocal apparatus as an indicator of the emergence of speech. However, the distinction between prosody and phonology stands out as self-evident and absolutely fundamental from the vantage point of neuro-linguistics. These two different kinds of voluntary vocalization both involve changes in peripheral anatomy, serve different communicative and representational functions, and have a different neurological foundation (**Donald** 1991; 1993b). Prosodic control of vocalization is a logically prior adaptation, and has its own evolutionary rationale, quite independently of any capacity for language. For this reason, as well as for others, I placed expanded prosodic control of the voice far back in evolutionary time, embedding it in a relatively early hominid adaptation that enabled more effective nonverbal communication.

Our capacity for phonology came in a much more recent stage in our evolution. It was a more specialized adaptation, undoubtedly tied to the acquisition of oral language by sapients. The Neanderthal issue forces the theorist to focus on the close link between the peripheral vocal apparatus and the central neural mechanisms that control it. Both **Lieberman** and I have argued that there is a compelling rationale for treating the speech adaptation as an integrated system; there is an exquisite fit between our high-speed vocal apparatus, with its corresponding auditory feedback mechanisms and memory capacity, and the central neural mechanisms that control and integrate the whole system. Judging from his flippant dismissal of cognitive neuropsychology, and his ridicule of **Fodor's** (1983) famous terminological framework for the field, **Brace** has little patience with the careful experimental work – he calls it "phrenology" – that produced this conclusion. But it makes a great deal more sense to treat the speech system as a single intrinsically complex adaptation driven at the cultural level, rather than as a set of semi-autonomous components that might have evolved in an asynchronous manner, as **Brace** must if he insists on sticking to his position on Neanderthal speech. Again in this, I side with **Lieberman** in principle if not in every detail.

R2. Literacy and recent cognitive evolution

Olson's comments were focused on the latter part of my book, the so-called "third transition," which was concerned with the cognitive effects of writing, symbols, and external memory storage. I agree with most of what he says about literacy, and much of the last part of my book is devoted to the way in which written texts interacted with the rest of human culture to change the way people thought and represented reality. Texts are powerful reflective devices; they change the way we think, and ultimately even the way we speak. We are in agreement here; if he saw a potential controversy here, I missed it.

In other areas, however, we are apparently not in agreement, mostly because **Olson** has seriously misunderstood one part of my theory (albeit in a rather creative way). He claimed I stated that human cognition has advanced "by adding, at each stage, an external memory to biological

memory." That is not really what I said at all. As I pointed out in both my book (1991) and my *Précis* (1993b), the memory architecture added in the first two transitions constituted refinements of *biological* memory, not external memory. What I call external memory came later, with external symbols, in the final stage of our cognitive evolution. But mimetic and linguistic capacity, which were acquired in the first two stages of human cognitive evolution, involved changes to internal biological memory. Both of these adaptations introduced qualitatively new kinds of internal representations (for example, narrative autobiographical memories), and as I pointed out in the previous section, these new representations also gave humans new memory retrieval skills whereby explicit recall became possible. All these changes constituted changes to *biological* memory, not external memory.

I said **Olson's** misreading was creative, however, because there is a grain of truth in it. In a sense, any shared culture can be regarded as holding knowledge that is external to the individual. Olson obviously thought I had referred to that aspect of oral and mimetic culture as external memory. However, as I pointed out at some length in my book, the knowledge contained in a preliterate culture must ultimately reside in individual biological memories; until the invention of permanent external symbols (during the third transition), the maintenance and transmission of all stored cultural knowledge depended on biological memory. Such memories are distributed across large numbers of individuals, but still reside in biological storage devices, that is, individual minds.

By external memory I mean permanent or quasipermanent storage media external to biological memory that remain outside biological memory and interact with individual minds by means of various display devices I label the "external memory field"; the latter involves a neural projection of symbolic storage media, usually onto the visual brain. This development came only with the invention of permanent external symbols and allowed a revolution in the kind of stable perceptual access humans had to their representations. I never claimed, although **Olson** also attributes this notion to me, that improvements in external memory storage media could in themselves explain the transforming power of literacy on thought. But I believe strongly that the novel storage and retrieval properties of external symbolic devices – for example, the unlimited size of single entries, their much more flexible formatting, and their ease of organization for retrieval – were instrumental in allowing humans to think their way out of the intellectual limitations of oral culture. In the last part of my book, I made essentially the same point **Olson** makes in his commentary, namely, that paradigmatic thinking skills evolved through an iterative process that involved the use of external symbols, especially of large symbolic products like books and libraries, and the social institutionalization of the programming needed to maintain and transmit the decoding and thinking skills that emerged from increasing symbol use.

In short, advanced thinking skills are not innate; they are cultural products supported (especially at the point of their invention) by our memory technology. In effect, paradigmatic thought is a cultural invention, sustained by elaborate social structures and the conventions they enforce. But it could not have developed without the armamentarium of external symbols and new storage media that we have developed. Unfortunately, I think **Olson** missed the crucial

role I gave to storage media and display media in the *initial invention* of new kinds of representation and thought. As he pointed out, and as I acknowledged both in my book and in my first-round reply to some of my commentators, oral culture has always been, and remains a very powerful transmission device; many advanced paradigmatic ideas – for example, in science – are still routinely transmitted orally. They are not initially *invented*, however, without the external representational tools on which scientists and scholars depend: the variable and tailor-made structure of the external memory field; the enormous variety of symbols, equations, specialized languages, and representational devices; the larger-scale cross-referencing devices and visualizing media; and notations, acronyms, and paradigms. All of these are necessary for the core cognitive processes that drive the entire theoretic enterprise and produce theoretic ideas that can then be disseminated and assessed orally.

Moreover, the cognitive architecture I described was not meant to reflect the architecture of every mind in the culture; it is mostly needed and used for cutting-edge cultural invention, not necessarily the day-to-day thinking of most citizens of the modern state, who are not fully engaged in the theoretic enterprise. Again, **Olson** and I are not really in disagreement here, once his misinterpretation of my work is cleared up.

R3. Episodic competence and explicit recall

Stenning's commentary brings up some important points about my use of the term "episodic." I did not really try to redefine the term "episodic" memory; rather, I coined the phrases "episodic competence" and "episodic culture" to describe the mentalities and group behavior patterns of higher mammals. I realize that my use of the adjective "episodic" may appear to be nonstandard in the specialized context of human memory theory, but it is not that much at variance with the wider literature on memory; in fact, it has its origin in that literature. I was greatly influenced in my choice of terminology by Sherry and Schacter's (1987) article on the evolution of memory. They reviewed a variety of classification systems for memory and proposed that there was something in common between the concept of episodic memory and what was known as "locale" memory in animal research (in the latter category they included studies as varied as radial maze data in mammals and ethological observations on bird food-seeking). The common element was that in both cases, subjects had to store details of specific situations and episodes and often showed rapid, even single-trial, learning; they called this Type II memory, as opposed to Type I memory, which resembles human procedural memory, ignores situational details, and follows a gradual acquisition course. Sherry and Schacter argued that Type II memory, which is close to what I call the storage of episodic knowledge, is found in most mammals and birds.

Tulving's original (1983) adoption of the term "episodic" stayed fairly close to its dictionary meaning in English; it referred to our memory for the details of specific life episodes; but, unfortunately (perhaps because it became paradigm-bound), it also acquired a strong association with conscious recall in humans. In an exchange of comments following a *BBS* multiple review of his book, Tulving (1984) acknowledged that animals may have some capacity for episodic memory; he made this point in reply to **Olton's**

(1984) commentary in the same issue. He emphasized the apparent inability of animals to voluntarily retrieve, or recall, their "episodic" knowledge, but he pointed out that there was a similarity between semantic memory and episodic memory and that "by doing away with the sharp boundaries between episodic and other memory systems it renders less controversial the proposition of episodic memory in animals" (1984, p. 259). Stenning makes a similar point, and I agree completely; some animals evidently retain situational details after perceiving complex events; yet they apparently cannot retrieve that knowledge without an environmental cue.

Dennett (1992) raised a similar point with regard to my nonstandard use of the term. To Dennett, as to **Stenning**, episodic recall is a very sophisticated, and thus probably late, evolutionary acquisition. My reply to him will also serve as my reply to Stenning: we must differentiate the *storage* of episodic knowledge from the *retrieval* of episodic knowledge. Episodic storage involves the acquisition of a long-term memory record based on stored bits and pieces of specific event-perceptions; in animals, such information seems to be accessed only when it is triggered by environmental cues, since animals, with the possible exception of some apes, lack the capacity to retrieve explicitly. Their implicit episodic competence is evident, however, in their ability to draw on incidental details of previous experiences; thus, Olton (1984) argued that cats had what I would call episodic competence, based on his work using the radial arm maze.

Storing episodic event-impressions is clearly not the same thing as developing a capacity to retrieve those same impressions consciously and voluntarily. In my evolutionary proposal, I have suggested that humans gradually evolved explicit recall (of episodic as well as other kinds of knowledge) as the result of a succession of changes to their ways of recoding reality in retrievable form (a somewhat similar proposal has been made in the context of ontogenesis, by Karmiloff-Smith 1992; see Donald 1994). [See also multiple book review of Karmiloff-Smith's *Beyond Modularity*, *BBS* 17(4)1994.] Thus, our explicit episodic competence depends on a variety of representational systems that have evolved over the past five million years. We retrieve episodic memories with the help of language and external symbols; but we can also retrieve episodes by other means, such as with self-generated imagery, or with methods of nonverbal self-reminding, all of which evolved since hominids deviated from the cognitive pattern of their primate predecessors. We have good reason to believe that different explicit retrieval paths came at different evolutionary stages, and involved different neurological mechanisms. As a result of this, explicit retrieval, including episodic retrieval, is not achieved by a unitary or simple retrieval system; it is made possible only by a complex retrieval architecture that was added long after higher mammals had evolved a capacity for episodic *storage*. This distinction has not been made clearly enough, if at all, in mainstream human memory research, but it is fundamental.

Stenning makes another interesting point that I will try to place in my own context; he claims that it might have been the evolution of episodic retrieval capacity that enabled humans to construct the kinds of nonverbal representations I call "mimetic." I agree that mimetic representations are explicit, or at least potentially explicit, but I think he has it backwards; mimetic representations are the most

basic human means of recoding experience into retrievable form (usually by self-generated action, or kinematic imagery). Thus, the evolution of mimetic representational ability would have secured the first possibility of explicit episodic recall for hominids, rather than vice versa. It was, and remains, a limited retrieval device, and was by no means the final or most effective one, being substantially superseded as a retrieval device by language, which may be seen from one perspective as a massive and highly complex memory retrieval device. One corollary of this is that the "normal" operational definition of episodic memory as the conscious recall of specific experiences confounds several distinct aspects of the memory system, whereas my usage of the term "episodic" does not.

I agree with **Stenning** that my theory needs to be reformulated in some form that will allow simulation. In its present form, it is empirically testable, and with currently available methodology we can try to verify the broader contours of the many specific proposals and predictions that it makes. But until we can simulate the action of complex nervous systems in much greater detail, we will not really understand the phenomena it describes in satisfactory depth. The challenge of understanding the knowledge base that serves as the underpinning of language and mimetic representation is surely the greatest one facing the next generation of cognitive scientists, and one of the greatest challenges science has ever faced.

R4. Courtship and evolution

Baum has suggested that courtship might have been a major factor favoring language evolution. I hope he is not suggesting that courtship is the primary or sole selection pressure involved in the emergence of language. There is an obvious problem in making courtship the prime mover for language evolution: no major sexual dimorphism exists regarding human language. In most examples of courtship-driven adaptations (birdsong, the peacock's tail, etc.) there is a large sexual dimorphism. But in humans, both sexes have full-fledged language capacity, and although there may be some measurable differences in language competence between the sexes, they are very small and run in both directions, depending on which aspect of language is singled out for measurement.

Even if this problem were explained away, courtship is only one of many feasible candidates for "justifying" the evolution of better communication skills in terms of selection pressure. Even a slight improvement in the communication skills of early hominids would have improved their coordinated hunting, their responses to intergroup aggression, and the sophistication and rate of diffusion of various other skills including toolmaking. This would have immediately and directly affected the relative fitness of hominids possessing such capacities. The most likely scenario is that all of these, as well as aspects of emotional behavior and social bonding (of which courtship certainly formed a part) were included among the various selection pressures that favored the emergence of improved social communication in hominids.

But none of this necessarily implies that these were the evolutionary pressures that led to full-fledged language. Mimetic communication can achieve a great deal in itself, is much closer to primate capacities, and is therefore more likely to have been affected by these kinds of pressures,

possibly quite early in hominid evolution. I might add that Baum seriously misunderstood many aspects of my theory. He misinterpreted me badly on the subject of language laterality, and did not seem to grasp the idea that selection pressures can act simultaneously on several levels, favoring a whole complex of individual behaviors and capacities. Nor did he see that mimetic skill provides some of the necessary preconditions for the evolution of protolanguage, and therefore must have come first. There is simply no credible case to be made for a single evolutionary "saltation" to language. Mimesis provides a major bridge, and provides a basis for the emergence of a more complex hominid cultural environment later in human evolution, in which new selection pressures would then have emerged that favored an adaptation as radical as language.

Baum suggests a role for mimesis in courtship. Here we are closer to agreement. Mimesis is closely related to – and appear to be an extension of – behavior patterns that are evident in modern apes, and that early hominids undoubtedly inherited from their Miocene primate ancestors. Courtship, social bonding, aggression, and dominance behaviors continue to have a strong mimetic component in humans. As I have suggested, the more abstract forms of mimesis – for instance, the intentional use of pointing, gaze, and metaphoric gesture – probably evolved "out of" those basic primate expressive patterns, with some major changes occurring as early as the australopithecine/paranthropus era. Full-fledged language, with its large lexicons, complex grammars, and very rapid rates of transmission, eventually emerged and allowed the species *Homo* to transcend the limitations of mimetic representation. But mimesis, and mimetic culture, should not be underestimated in terms of its expressive capabilities. It still provides the cultural environment for language acquisition (see especially Nelson 1995). Language may have supplemented mimesis, but it did not replace it. In fact, mimesis still works wonderfully well in its own cultural domains, one of which is undoubtedly courtship.

References

- Arensburg, B., Tiller, A. M., Vandermeersch, B., Duda, H., Schepartz, L. A. & Rak, Y. (1989) A middle paleolithic human hyoid bone. *Nature* 338:758–60. [PL]
- Arensburg, B., Schepartz, L. A., Tiller, A. M., Vandermeersch, B., Duda, H. & Rak, Y. (1990) A reappraisal of the anatomical basis for speech in middle paleolithic hominids. *American Journal of Physical Anthropology* 83:137–46. [PL]
- Barbujani, G. & Sokal, R. R. (1991) Genetic population structure of Italy. II. Physical and cultural barriers to gene flow. *American Journal of Human Genetics* 48:398–411. [PL]
- Bickerton, D. (1993) Putting cognitive carts before linguistic horses. *Behavioral and Brain Sciences* 16:749–50. [DRO]
- Bosma, J. F. (1975) Anatomic and physiologic development of the speech apparatus. In: *Human communication and its disorders*, ed D. B. Towers. Raven. [PL]
- Brace, C. L. (1993) "Pop science" versus understanding the emergence of the modern mind. *Behavioral and Brain Sciences* 16:750–51. [rMD, PL]
- Bruner, J. (1986) *Actual minds, possible worlds*. Harvard.
- Clutton-Brock, T. H. & Harvey, B. H. (1980) Primates, brains, and ecology. *Journal of Zoology* 190:309–32. [EBB]
- Darwin, C. (1859) *On the origins of species*: facsimile edition 1964. Harvard. [PL]
- (1874) *The descent of man*, second edition. John Murray. [EBB]
- Dawkins, R. (1986) *The blind watchmaker*. Longman. [EBB]
- Dennett, D. (1992) The role of language in intelligence. "Darwin Lecture," Cambridge University, March 6 1992. Publication CCS-92-3, Center for Cognitive Studies, Tufts University. [rMD]
- Donald, M. (1991) *Origins of the modern mind*. Harvard. [rMD, DRO, EBB]
- (1993a) Human cognitive evolution: What we were, what we are becoming. *Social Research* 60:143–70. [rMD]
- (1993b) Précis of *Origins of the modern mind*. *Behavioral and Brain Sciences* 16:737:91. [rMD, PL, DRO, KS]
- (1994) Representation: Ontogenesis and phylogenesis. *Behavioral and Brain Sciences* 17:714–15. [rMD]
- Dunbar, R. I. (1988) Primate social systems. Croon Helm. [EBB]
- (1990) Ecological modeling in an evolutionary context. *Folia Primatologica* 53:235–46. [EBB]
- Falk, D. (1975) Comparative anatomy of the larynx in man and the chimpanzee: Implications for language in Neanderthal. *American Journal of Physical Anthropology* 43:123–32. [PL]
- Fisher, R. A. (193) *The genetical theory of natural selection*. Clarendon [EBB]
- Fodor, J. (1983) *Modularity of mind*. MIT Press. [rMD]
- George, S. L. (1978) A longitudinal and cross-sectional analysis of the growth of the postnatal cranial base angle. *American Journal of Physical Anthropology* 49:171–78. [PL]
- Goodall, J. (1986) *The chimpanzees of Gombe: Patterns of behavior*. Harvard. [EBB]
- Grosman, C. (1979) Base du crane et pharynx dans leur rapports avec l'appareil de langage articulé. *Mémoires du Laboratoire d'Anatomie de la Faculté de Médecine de Paris*, no. 40–1979. [rMD]
- Halverson, J. (1993) Mythos and logos. *Behavioral and Brain Sciences* 16:762. [DRO]
- Harpending, H., Sherry, T., Rogers, A. R. & Stoneking, M. (1992) The genetic structure of ancient human populations. *Current Anthropology* 34:483–96. [EBB]
- Hedges, S. B., Sherry, S. T., Rodgers, A. R., & Stoneking, M. (1992) Human origins and analysis of mitochondrial DNA sequences. *Science* 255: 737–39. [EBB]
- Houghton, P. (1993) Neanderthal supralaryngeal vocal tract. *American Journal of Physical Anthropology* 90:139–46. [PL]
- Karmiloff-Smith, A. (1992) *Beyond Modularity*. MIT Press. [rMD]
- Laitman, J. T. & Crelin, E. S. (1976) Postnatal development of the basicranium and vocal tract region in man. In: *Symposium on development of the basicranium*, ed J. Bosma. U.S. Government Printing Office. [PL]
- Laitman, J. T., Reidenberg, J. S., Gannon, P. J., Johansson, B., Landahl, K. & Lieberman, P. (1990) The Kebara hyoid: What can it tell us about the evolution of the hominid vocal tract? *American Journal of Physical Anthropology* 81:254. [PL]
- Lande, R. (1980) Sexual dimorphism, sexual selection, and adaptation in polygenic characters. *Evolution* 34: 292–305. [EBB]
- (1981) Models of speciation by sexual selection of polygenic traits. *PNAS* 78:3721–5. DNA sequences. [EBB]
- Lieberman, P. (1975) *On the origins of language: An introduction to the evolution of speech*. Macmillan. [PL]
- (1982) Can chimpanzees swallow or talk? A reply to Falk. *American Anthropologist* 84:148–52. [PL]
- (1984) *The biology and evolution of language*. Harvard. [PL]
- (1989) The origins of some aspects of human language and cognition. In: *The human revolution: Behavioural and biological perspectives in the origins of modern humans*, ed P. Melars & C. B. Stringer. Edinburgh University Press. [PL]
- (1991) *Uniquely human: The evolution of speech, thought, and selfless behavior*. Harvard. [PL]
- (1992) On Neanderthal speech and Neanderthal extinction. *Current Anthropology* 33:409–10. [PL]
- (1993) The Kebara KMH-2 hyoid and Neanderthal speech. *Current Anthropology* 34:172–75. [PL]
- (1994) Hyoid bone position and speech: Reply to Arensburg et al. (1990). *American Journal of Physical Anthropology* 94:215–78. [PL]
- Lieberman, P. & Crelin, E. S. (1971) On the speech of Neanderthal man. *Linguistic Inquiry* 2:203–22. [rMD, PL]
- Lieberman, P., Crelin, E. S. & Klatt, D. H. (1972) Phonetic ability and related anatomy of the newborn, adult human, Neanderthal man, and the chimpanzee. *American Anthropologist* 74:287–07. [PL]
- Milton, K. (1988) Foraging behavior and the evolution of primate intelligence. In: *Machiavellian intelligence*, R. Byrne & A. Whiten, eds. Plenum [EBB]
- Nelson, K. (1995) *Language in cognitive development: The emergence of the mediated mind*. Cambridge. [rMD]
- Olson, D. R. (1993) How writing represents speech. *Language and Communication* 13:1–17. [DRO]
- (1994) *The world on paper: The conceptual and cognitive implications of writing and reading*. Cambridge. [DRO]
- Olton, D. (1984) Comparative analysis of episodic memory. *Behavioral and Brain Sciences* 7:250–51. [rMD]
- Pepperberg, I. M. (1994) Numerical competence in an African gray parrot. *Journal of Comparative Psychology* 108:36–44.

References/Continuing Commentary

- Ridley, M. (1993) *The Red Queen: Sex and the evolution of human nature*. Viking. [EBB]
- Schepartz, L. A. (1993) Language and modern human origins. *Yearbook of Physical Anthropology* 36:91–126. [PL]
- Senecail, B. (1979) L'Os hyoïde: Introduction anatomique à l'étude de certains mécanismes de la phonation. *Mémoire du Laboratoire d'Anatomie de la Faculté de Médecine de Paris*, no. 36–1979. [PL]
- Sherry, D. & Schachter, D. (1987) The evolution of multiple memory systems. *Psychological Review* 94:439–54. [rMD, KS]
- Small, J. P. (1993) Memory, text, and the Greek revolution. *Behavioral and Brain Sciences* 16:769–70. [DRO]
- Templeton, A. (1992) Human origins and analysis of mitochondrial DNA sequences. *Science*: 255:737. [EBB]
- Truby, H. L., Bosma, J. F. & Lind, J. (1965) *Newborn infant cry*. Almqvist and Wiksell. [PL]
- Tulving, E. (1983) *Elements of episodic memory*. Oxford. [rMD]
(1984) Précis of *Elements of Episodic Memory*. *Behavioral and Brain Sciences* 7:223–68. [rMD]
- Williams, P. L. & Warwick, R. (1988) *Gray's anatomy*, 6th edition. W. B. Saunders. [PL]
- Wind, J. W. (1970) *On the phylogeny and ontogeny of the human larynx*. Volters-Noordhoff. [PL]