

that the hyoid is *morphologically*, and therefore importantly, very human-like and very un-pig-like.

There are many similar examples of selective use of the evidence scattered throughout this contribution. Although few palaeoanthropologists would disagree that modern humans have a more sophisticated system of vocal communication than did the earlier hominids, many would dispute the assertion that this enhanced vocal ability was necessarily tied as tightly to modern human anatomy as is suggested here. By being selective with their evidence, Milo and Quiatt create a just-so story. It is internally consistent and believable, but unfortunately it does not have a firm anchor in reality. On present evidence, an equally strong argument could be made for similar linguistic, cognitive, and behavioural abilities in early anatomically modern humans and their morphologically more archaic contemporaries. If this alternative interpretation proves correct, the important question to answer is why cognitive and linguistic skills continued to develop in modern humans and not in Neanderthals. There is no clear answer at present to this question, and undoubtedly this is the reason so many anthropologists are eager to tie linguistic and cognitive ability to modern human morphology in the face of potentially contradictory evidence. In my opinion, the solution is not so simple, and the problem of linguistic and cognitive evolution in the later hominids is far from solved.

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Following Lieberman, Milo and Quiatt propose that the anatomically modern humans who followed the Neanderthals had rapidly spoken phonemicized speech while the Neanderthals did not; superior language, they suggest, gave the more modern form a decisive advantage over their predecessors. They cite a broad spectrum of scholarship in support of their ideas, but the paper is difficult to read, in part because too many sentences are thinner in content than in words: "We see premodern human culture as a suite of learned social behaviors including systematic cooperation and sharing of information and resources and fundamentally dependent on technology." "The dispersion of *H. erectus (sensu lato)* out of Africa after 1.5 million years ago, implying partial release from ecological constraints, seems to us presumptive evidence of relatively advanced cultural behaviors." After I have conquered the syntax of sentences like these I still don't feel that I have learned very much. Milo and Quiatt suggest that "early anatomically modern people were already acquiring an ability to organize their social and territorial behavior and to respond flexibly to changing environments which their predecessors had not possessed." This is as likely to have been true of australopithecines (or, for that matter, of early reptiles) as of early modern humans. Vague statements like this are not really enlightening.

Too often, I feel, Milo and Quiatt offer opinions unsupported by data. For example, they assert that the relatively advanced cultural behaviors of *Homo erectus* "must have included language, which we regard as indispensable in maintaining a social environment capable of buffering the cooperating group against the exigencies of an increasingly large, heterogeneous, and unpredictable natural environment." They also cite the opinions of others rather than the data that would support these opinions: "Pulleyblank (1983) and Lieberman (1986, 1991) agree with Hewes that it was the achievement of fully phonemicized speech, with duality of patterning, 'that . . . was the prime factor in the creation of an 'autonomous mental world' . . .'" To be sure, the problem about studying language evolution has always been the paucity of data, but collecting opinions seems an unsatisfactory substitute.

Thus I finish this paper feeling that I have been given a series of opinions bearing on two controversial ideas: that only with anatomically modern *H. sapiens* did the vocal tract become capable of full phonemicization and that anatomically modern *H. sapiens* wiped out rather than evolved from or interbred with the Neanderthals. Maybe so, but I do not find new data that would support these arguments, and I feel no more persuaded by them than when I began the article.

Milo and Quiatt's use of the term "group selection," apparently for the defeat of Neanderthals by anatomically modern *H. sapiens*, is unfortunate, since the phrase is used in evolutionary biology to refer to a thoroughly discredited selective mechanism. When they write, "Apparently biology was still of primary importance to human adaptation," they appear to believe that some behavior can be credited to biology and other behavior to something else, presumably culture, whereas in fact everything that we and other animals are and do results from the interaction of biology with the natural and cultural environment. They write throughout as if phonemicization must have been an all-or-none matter, but I see no reason that the ability to recognize and produce phonemic contrasts could not have developed step by step, one contrast at a time, over hundreds of millennia.

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Once is an aberration (CA 30:157-90), twice a trend (CA 32:513-41), but this third publication of Neanderthal-bashing appears to reflect a policy, turning CURRENT ANTHROPOLOGY into a Neanderthal-focused *Zeitschrift für Rassenkunde*—a sinkhole for any idea that belittles the European Mousterians. In many respects, this article is so speculative as to defy any kind of scientific rebuttal, and, given the authors' own admission that none of their ideas can be considered a "scientifically falsifiable hy-

pothesis," we wonder how this kind of armchair musing ever makes it past the editor's desk.

Milo and Quiatt's misunderstandings of the anatomy/behavior interface are exposed in their discussion of Kebara. They argue that the purportedly greater number of points at Kebara (considered Neandertal) is an indication of primitiveness compared with Qafzeh (considered "modern" *H. sapiens*), where there are fewer, and go on to speculate that "modern" *H. sapiens* "were expending less overall effort on subsistence, which accords well with their reduced postcranial robusticity." Would a hominid found with no points then be fully modern? That the Kebara 2 skeleton resembles Neandertals in some of its features does not mean that individuals who resembled it made every tool or were responsible for every living floor at Kebara. Indeed, Middle Paleolithic Near Eastern populations are extremely variable, and there is some doubt that taxonomic distinctions between the Neandertal and "modern" populations there even exist (Arensburg 1991). Furthermore, we find no evidence for "reduced postcranial robusticity" at Qafzeh or Skhul; rather, these specimens possess a pattern of muscularity (e.g., as measured by cortical thickness and the expression of *linea aspera*) different from European Neandertals and significantly greater than that in modern specimens from the later Pleistocene in the Near East. Similarly unsupported is their contention that the number of points found at a site can even remotely be related to the postcranial morphology of an individual. It is not even as though the points at Kebara were found within the grave of the Kebara 2 individual and associ-

ated with it; rather, they were scattered through the strata.

According to Milo and Quiatt, "one ought not to expect direct correspondence between morphological and technological change. . . . Neither should the appearance of the anatomical prerequisites for rapidly spoken language be taken as necessary and sufficient evidence for the contemporaneous appearance of that or associated behaviors. . . . the . . . motor changes which made possible rapidly spoken phonemic communication did not in themselves produce that behavior." Besides contradicting their idea that the number of points correlates with hunting intensity and modern behavior, this statement makes it apparent that they have little regard for morphology in reconstructing evolutionary changes. This supposition is supported by their misunderstanding or misstatements concerning the morphology of fossil hominids at critical junctures of their argument (see our table 1).

How can we reconcile Neandertals' brain size and anatomy with a reconstruction that denies them even the most basic capabilities of other anthropoids? For example, Milo and Quiatt contend that Neandertals seem to have been practicing a radiating foraging pattern and early moderns a circulating pattern implying "enhanced ability to plan ahead and to predict the availability of resources." Yet many primates (not to mention birds and other vertebrates) exploit resources across their range in a seasonal round, arriving in different areas just as, or even before, the resources are optimal. Moreover, why is staying put indicative of the lack of planning

TABLE 1
Reality Check

Milo and Quiatt Citation	Our Study of the Human Fossil Record
"Cranial fragments [from Klasies] suggest a substantially shortened face."	The KRM 16651 zygomatic is the second-largest in the entire African fossil record of <i>Homo</i> , exceeded only by that of Bodo.
"The [Klasies] mandibles generally have mental eminences."	Two of the four Klasies symphyses lack mental eminences, and a third is pathological in the region.
"Selection was operating on at least one genetically variable population in Africa to . . . reform . . . the . . . basicranium."	There are no basicrania for the African early modern humans listed here.
"The shortened [African] face is related to the appearance of the relatively short, broad, and rounded tongue."	There are no fossil tongues, and in fact there are no faces among the African early modern humans listed.
"Reduction and retraction of the face contributes to the production of [the] marked flexure of the basicranium."	A number of <i>Australopithecus boisei</i> specimens (OH 5, ER 17400) have very flexed bases but the largest faces and teeth known for any hominid.
"Marked flexure of the basicranium . . . [is] associated with the modern human supralaryngeal tract."	We wonder about this association because of the above-mentioned <i>A. boisei</i> specimens, but if true the Heim (1989) reconstruction of the La Chapelle Neandertal places its flexure well within the modern human range.
[in Europe ca. 40,000 years ago] "there is a persuasive connection between anatomically modern human fossils, elaborated and differentiated blade tools, the regular production of formal tools in bone, ivory, and antler, and . . . possibly, calendrical notation."	There are no 40,000-year-old anatomically modern human fossils in Europe. The earliest dated specimens are at least 10,000 years younger, and according to Marshack (the source cited) calendrical notation is even later. Milo and Quiatt seem to equate the early Aurignacian with anatomically modern humans, although the makers of this industry are yet to be found.

depth and a nomadic existence a "modern" trait? Certainly it takes considerable planning depth to find resources throughout the year when a group occupies a restricted territory (if indeed this was the case with Neandertals). Beyond this, following the logic of Milo and Quiatt, one of the hallmarks of contemporary humans is their successful sedentary existence, not roaming the countryside for food and work. In this regard, the behavior of Neandertals is more like that of contemporary humans than the patterns Milo and Quiatt reconstruct for the early so-called anatomically moderns from Skhül and Qafzeh.

Finally, there is little to support the arguments they offer concerning limited language capacity in Neandertals. Cranial-base flatness can no longer be viewed as an important morphological feature in single Neandertal specimens (Heim 1989) or in Neandertals as a group (Frayer 1992). Milo and Quiatt underestimate the importance of the Kebara hyoid and reiterate the misleading statement that it metrically resembles that of a pig (Laitman et al. 1990). This hyoid, as described by Arensburg et al. (1989), is morphologically indistinguishable from that of modern humans (see also the photos in Culotta 1993). It has no close morphological similarity to that of a pig (Frayer 1993), and, while the specimen alone may not tell the precise location of the larynx in the throat, given its totally modern appearance there is absolutely no reason to suspect that laryngeal location was anything but modern. Furthermore, to report that hyoid morphology is variable is reasonable, but none of the variation described or depicted by the researchers cited would allow nonhuman primates (or pigs) to be included within humans, nor would the variation they review allow Kebara 2 to be excluded from the patterns found in living humans.

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I agree with Milo and Quiatt on a late origin for vocal, semantic language, and I sympathize with their desire to reconstruct the cognitive precursors of modern human abilities. Nonetheless, I find their argument for a gestural premodern language weak, because it develops from the problematic inference that language has been around since "the first appearance of genus *Homo*."

Milo and Quiatt define premodern culture as "a suite of learned social behaviors including systematic cooperation and sharing of information and resources and fundamentally dependent on technology." They assert that the "dispersion of *Homo erectus*" qualifies as "presumptive evidence of relatively advanced cultural behaviors" which "must have included language." In other words, *H. erectus* could not have spread across the Old World without language; but the Miocene hominoids, for example, expanded their range as far as that of *H. erectus*, presumably without language to aid them.

The existence of culture among premodern hominines is not a given; it must be inferred. And there is by no means unanimity among paleoanthropologists on the existence of culture prior to about 60,000 years ago (see, e.g., Hayden 1993). I am aware of no archaeological evidence that premodern hominines shared resources or information, and "technology" may be too grand a term to apply to premodern tools. Thus, in view of the questionable cultural nature of *H. erectus* behavior, it cannot be unequivocally stated that language was under strong selection in the Middle Pleistocene.

Milo and Quiatt assert that premodern culture must have relied upon language to "maintain amiable social relations," "to establish and enforce norms of . . . behavior, including sharing, and to organize and regulate access to food and mates." But chimpanzees manage to do all of these things without even a gestural language such as is proposed here (see Burling 1993), and so do social carnivores. If it cannot be said for certain that premodern hominines shared or that they required language to regulate their behavior, what further need is there to posit a language for them? The answer can be found in the third problem with Milo and Quiatt's thesis, their assumption that tool use implies technology and that premodern technology required language for its existence.

I would argue that premodern tool use does not imply premodern technology, because among modern humans technology *presupposes* linguistic ability—it involves a tool's production and the knowledge and cultural meaning associated with it, both of which depend on language and its attendant neurological structures (see, e.g., Ridington 1982, 1988). Moreover, technology is required for many more things than just tools (e.g., clothing, personal ornamentation, symbols of rank, methods of food preparation), all of which are heavily freighted with cultural meaning. By basing their argument on the inference of technology, which in turn assumes that culture and language existed, Milo and Quiatt may have seriously compromised it. Whereas modern human "technology" requires a fabric of meaning, including linguistic competence, self-consciousness, and intention, breaking rocks to make sharper rocks does not require any of these things and need not be deemed "technology" at all.

Davidson (1991), Davidson and Noble (1993), and Noble and Davidson (1991) persuasively argue against the a priori notion that premodern stone tools are evidence of sophisticated cognitive abilities, and I would add that other animals (e.g., the beaver) manage to transform natural materials in structured and repetitive ways without language, culture, or technology. It might therefore be argued that language and culture are a sufficient but not a necessary condition for the production of premodern stone tools. Although the cognitive abilities necessary to make a Levallois point are greater than those possessed by chimpanzees and may have laid the foundation for modern human cognition, there is no necessary connection between the production of stone tools by modern humans and their production by earlier fossil

as *near*-modern in recognition of their mosaic of archaic and modern features. We suggest neither that these hominids were fully morphologically or behaviorally (or linguistically) modern nor that either population represents the direct ancestors of fully modern humans, but we do contend that they prefigure modern humans and that they broadly support the notion of an African origin of modern humans sometime between 50,000 and 100,000 years ago. This part of our argument, at least, entails a clearly testable prediction: that future research will ultimately reveal, in Africa, modern human fossil and archaeological remains much older than any now known.

In making a distinction between a primarily gestural premodern human language and the primarily vocal language of modern humans, we did not mean to suggest a stark dichotomy or a cognitive discontinuity. Just as modern spoken languages have a rich gestural component, we accept that vocalization, even if not phonemic in structure, was likely a rich supplementary component of the language of premodern people. Similarly, we posit that *many* of the underlying neurological faculties (e.g., syntax and cross-sensory stimulus processing) required for the production and use of language were already present in premodern hominids. In this we agree with Peters's welcome comments. We certainly do not mean to say, however (as Kien suggests we have), that "Neanderthals had all the mental apparatus and were just caught out by a physical lack," i.e., the lack of a modern face and supralaryngeal tract. On the contrary, we are persuaded by Kien's suggestion of a fundamental neural reorganization, which we associate with the appearance of anatomically modern humans and regard as a prerequisite for the cognitive production (e.g., rapid, hierarchical lexical access) and decoding (e.g., rapid, hierarchical aural symbol processing [and see the comments of Wallace]) of phonemically based spoken language. The behavioral consequences of such a reorganization would likely have been profound and might well have included sufficient changes in language behavior to drive subsequent modifications to the face and vocal tract. In short, we are more in agreement with Kien than we apparently have led her to believe. It is appropriate here, as well, to acknowledge the very insightful comments of Wallace, who points out both that the advent of phonemic speech likely was not an unalloyed blessing and that the demands of aural speech processing may have been significant to the cognitive evolution of modern *H. sapiens*.

In regard to what a predominantly gestural premodern human language might have been like, we wish to emphasize that the signed languages produced by deaf or otherwise impaired living humans are inappropriate as analogs. Whereas the brains of users of these languages are fully modern, with all of the structures for symbolic thought and language use (significantly, Kien refers to "speakers" of American Sign Language), the brains of users of premodern language were presumably organized differently from our own. It is therefore misleading to impute to such language the structural and expressive

qualities and the capacity for abstraction that characterize modern signed languages.

Our final point follows from our perception of a biological difference between premodern and modern humans and arises from the comments of Frayer and Wolpoff. Their interpretations of the hominid fossil record and of modern human origins are well known, and so the thrust and tenor of their comments are not unexpected. We trust that the reader will consult the primary data and texts and weigh the various interpretations, and so we will not take up the specifics of their rebuttal here. We wonder, however, at their accusation of "Neanderthal-bashing." While we are concerned to understand the differences between premodern and modern people, we have not intended any deprecation of the former, and we have made clear our conviction that Middle Pleistocene humans ought not to be considered either somewhat more than ape or somewhat less than human. In our opinion, the charge of "Neanderthal-bashing" betrays an extrascientific agenda which at the least presents an unfortunate impediment to a scientific inquiry into human evolution.

In closing, we wish to express our admiration for the ongoing research reported by Peters and by Gibson and Jessee; these are the kinds of work that are crucial to more informed thinking about the evolution of human language and cognition and the interpretation of the fossil record. Clearly, as is illustrated by the juxtaposition of the comments by Gibson and Jessee and by Krantz (whose important paper we regret having failed to cite), the last word has yet to be spoken on the anatomy of speech.

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