

to 40 years' time) turn out to be either erroneously dated or erroneously interpreted is a less than compelling reason to discount it now. If we accept this evidence, we could equally well—and perhaps more convincingly—argue that the anatomical correlates of modern human phonemic language first appeared in Africa and the Levant, with the earliest appearance of modern cranial morphology, and spread from there throughout the world (see also Milo and Quaitt 1993). If this spread involved either population movement and hybridization or centrifugal gene flow, it would also be compatible with the presence of apparent continuity features in descendant populations.

However, we should not jump to the conclusion that the evolution of phonemic language was necessarily associated with the first appearance of modern humans, from a single origin or otherwise. Although phonemic language is fundamental to modern human culture, we do not really know when or why it appeared (Aiello and Dunbar 1993). Any arguments implementing phonemic language in the evolution of modern human cranial form are based on the assumption that modern humans are anatomically more capable of phonemic speech than are the Neanderthals or other archaic precursors. In my opinion the jury is still out on this point. There are sufficient arguments against the interpretations of Lieberman and his colleagues to suggest that the only differences between Neanderthal and modern human speech would be one of accent and nasalization.

Furthermore, there is the problem of Krantz's facile dismissal of the postcranial evidence. The difference between the postcrania of archaic and modern humans is not just one of robusticity (Aiello and Dean 1990). Based on our current knowledge, there are fundamental differences in the morphology of the pelvis and the femur that would suggest a different system of weight transfer during walking. I would be interested to hear Krantz's explanation of how language, in the context of regional continuity, could have acted as the selection pressure resulting in modern human bipedalism, which appears to be of a significantly different nature than that found in the more archaic hominids.

Krantz's resolution of the archaic-to-modern transition is seductive because it appears to offer an easy answer to a controversial issue. It is just too bad that it is not firmly rooted in the evidence.

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Reply

DAVID W. FRAYER
Department of Anthropology
University of Kansas

MILFORD H. WOLPOFF
Department of Anthropology
University of Michigan, Ann Arbor

ALAN G. THORNE
Department of Prehistory
Australian National University

FRED H. SMITH
Department of Anthropology
Northern Illinois University

GEOFFREY G. POPE
Department of Anthropology
William Patterson College

We thank the respondents for their comments on our article. Krantz takes on the difficult burden of trying to resolve the conflict between the Eve theory and multiregional evolution. His attempt to forge a compromise may be a successful approach in politics, but we think it is a mistake to propose that there must be some intermediate position that preserves the fundamental predic-

tions of these two incompatible hypotheses. When there is no middle ground between conflicting theories, attempts to define a compromise position that resolves the conflicting predictions actually undermine the possibility of a resolution. Indeed, progress in science comes from the refutation of clearly stated theories with unambiguous predictions. Whether the Eve theory is refuted by existing data, as we and Templeton (AA 95:51–72, 1993) believe, or both are incorrect, as Aiello contends (AA 95:73–96, 1993), it is the process of refutation, not the reconciliation of mutually exclusive predictions, that underlies the scientific method. As we view it, one of the great advantages of this debate and the best hope for its resolution lies precisely here, in how fully contradictory the two theories are. Therefore, in our view, it is a mistake to minimize the importance of conflicting interpretations of the data.

Clearly, we do not concur with Krantz that “we must begin by acknowledging the truth of the initial observations of both.” For example, the genetic basis for the Eve theory relies on the assumption that mtDNA is selectively neutral. But, as we (and Templeton, AA 95:51–72, 1993) review, there is considerable evidence for the operation of strong selection. Because mtDNA is inherited as a single allele, there is no justification for compromise on this issue, and it is impossible to accept both its neutrality and its non-neutrality as true. For the fossil record, the Eve view regards the Klasies River Mouth (KRM) material as early “anatomically modern *Homo sapiens*.” We demonstrated that the KRM fossils are variable and that they do not constitute a modern sample, citing unambiguous anatomical details found in the face and mandibles (see also Smith 1993; Wolpoff and Caspari 1990). If one takes the position that anatomical modernity is a distinct, diagnosable entity (and especially if one believes the entity is diagnosable because of descent from a common, recent ancestor), there is no compromise about these conclusions.

We do not accept the position that a few “modern features” are sufficient to diagnose modern *Homo sapiens*, but if we did, as Pope (1992) and others have pointed out, there are far earlier samples that could be characterized this way. The Klasies issue stems from different observations (that can be verified on the original fossils, which, for the most part, are available for study) and varying ideas on how to diagnose modernity (which reflect deep-seated differences in species definition and, more generally, evolutionary theory). In

sum, we believe that these two fundamentally contradictory issues can be resolved in the normal scientific manner by attempts to refute hypotheses, and that the resolution comes not from compromise but from data.

More disturbing to us are some misunderstandings of the multiregional model. Like others, Krantz seems to presume that it is a theory focused on how modern humans arose, when from the onset it was presented as a model for the evolutionary process in polytypic species (Thorne 1981; Thorne and Wolpoff 1992; Wolpoff et al. 1984). In developing the model, we were interested in the pattern of evolutionary change—the boundary conditions, if one prefers—and not every specific detail for every particular place or time. We never contended that multiregional evolution assumed a constant rate, or any particular magnitude of evolutionary change, so we could not have presumed that the rate of evolutionary change “cannot be as fast as suggested by the fossil record,” as Krantz has attributed.

The multiregional model only predicts that the rate of change can be expected to differ from feature to feature, from time to time, and from place to place. Recently, Pope (1992) has shown this kind of patterning in the Asian sequence, and Frayer (1992) has demonstrated that the European fossil record from the Middle to the Upper Paleolithic reveals rather slow evolutionary rates compared to the rapid changes associated with later periods. In our AA article, we never wrote that “the modern population within each region was derived *primarily* from the archaic forms of the same area” (our italics), and we wonder how Krantz could have deduced this from our text or from our figure 1. The reader should appreciate that this figure contrasts with those published by the Eve theorists—for example, Aiello (AA 95:73–96, 1993), Howells (1993), Lewin (1991), or Stringer (1990)—which purport to summarize the multiregional model and then form the basis of “explanations” about why it is wrong.

Multiregional evolution has always stressed the critical importance of genic exchange in the evolutionary process as it underlies the clinal explanation of how there can be both local evolution (e.g., regional continuity) and specieswide evolutionary change. For Australasia, which Thorne and Wolpoff addressed more than a decade ago (1981), a major point concerned evidence for significant skeletal continuity from earlier Pleistocene Indonesia to later Pleistocene Australia.

But nowhere have we argued for totally exclusive regional continuity, even though this is often attributed to us (by authors who thereby describe a "multiregional theory" they can disprove). To suggest exclusive in situ continuity would be foolish, given that all Australian genetic input has been by sea-borne migration. To the contrary, Thorne (1989) has shown significant input from East Asia as well as Indonesia.

We do not agree that the multiregional model describes a process that is necessarily gradual at all times, or that it cannot involve replacement, or that the model shows the Eve theory is wrong. What we have contended is that it is not our hypothesis but the fossil data that falsify the Eve theory. In the end, we are confounded as to why the specific predictions of the multiregional model are so often misread, and we can only hope that those who wish to test the model will start with an accurate rendition of it.

Krantz's compromise resolution for the origin and spread of modern *H. sapiens* is a reiteration of language as the mechanism that allowed for the replacement of all indigenous populations outside of Africa. This is not a new idea; Allan Wilson once suggested that language was the reason for replacement and that a gene for language was located in the mitochondrial DNA (Brown 1990:262). Even in the unlikely event that such a "language" gene exists, it violates the requirement that mtDNA is neutral and would corrupt the predictable ticking of the molecular clock. Beyond this, the field should now be weary of attempts to tie everything important in human evolution to language ability.

We do not reject the idea that some commonalities in human evolution respond to the spread of ideas and behaviors; this is an integral part of our model. Nor do we believe that language is unimportant. The problem is that language may not have "appeared" as a single, discrete discernible event in human evolution. In part, because of this, language ability has proven very difficult to read from the existing paleoanthropological record. Much of the original work of Lieberman, Crelin, and their students has been negated by more reliable fossil reconstructions, such as Heim's for La Chapelle-aux-Saints (1989), or by new discoveries such as the Kebara hyoid (Arensburg et al. 1990). For the former, it is now known that flatness of the Neanderthal cranial base is not outside the normal range of recent *H. sapiens*. For the latter, the only known Neanderthal hyoid is completely modern in anatomy and, by impli-

cation, in position and function. It certainly does not resemble a pig hyoid, as some have erroneously maintained (see Culotta 1993).

Today's understanding of potential Neanderthal speech abilities is in line with their large brain sizes and endocranial anatomy (Holloway 1985). The brain is surely where most unique aspects of human language ability lie and details of brain evolution clearly place what evidence there is for humanlike language ability well before the attainment of modern forms anywhere. We wonder why so many people have forgotten that language is a function of what lies inside the cranium, not what is below it. We also wonder why people conflate events in Europe after 50,000 years ago with the origin of modern humans, a process that began much earlier even in the refuted Eve model.

Multiregional evolution is not the explanation of one particular event, but rather a modeling of the evolutionary process in an internally subdivided species. It attempts to frame how evolutionary change continues in the face of geographic dispersion without requiring speciation. Multiregional evolution has proven to be robust and has held up to refutative attempts from several sources, including the Eve theory and, more generally, some of the interpretations of mtDNA evolution, which assume a homogeneous process. We look forward to continued testing of this model, since it is this process in scientific discourse that will lead to a better understanding of human evolution.

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Anthropology and Adoption

JOHN TERRELL
Department of Anthropology
Field Museum of Natural History

JUDITH MODEL
Department of History
Carnegie Mellon University

Anthropologists, we believe, are likely to forget that "what every anthropologist knows" is not necessarily what everyone else knows. In the quest for tenure, professional visibility, and academic achievement, anthropologists may also overlook the possibility that what they know could be important to people who are not anthropologists, too, if only they knew. Here is one example.

Adoption in America

In North America, most children grow up living with at least one of the parents they were born to; most children grow up assuming they will live with children born to them. Consequently, perhaps, many people in our society think of adoption as a second-best way of becoming a family (Schaffer and Lindstrom 1989:15). The psychological and social ties binding an adoptive family together are looked on as weaker than "natural" ties of blood. And adoption is seen as difficult and risky. The risk is held to be especially great when a child does not "match"—look like or share the background of—its adoptive parents (Bates 1993). This is preeminently true of transracial and international adoptions, in which a child, who has no say in the matter, is severed not only from its "real" family but also from ethnic roots and cultural heritage: in a word, from its true identity.

Recently, advocates of adoption have been emphasizing the difference between adoptive and biological families (e.g., Melina 1986; Register 1991; Schaffer and Lindstrom 1989), often as a way of helping parents through such "alternative parenthood" (Kirk 1984).