

Book Reviews

THE ORIGIN OF MODERN HUMANS AND THE IMPACT OF CHRONOMETRIC DATING. Edited by M.J. Aitken, C.B. Stringer, and P.A. Mellars. Princeton, NJ: Princeton University Press. 1993. ISBN 0-691-032424. vi + 248 pp. \$39.50 (cloth).

Over February 26-27 of 1992 a prestigious conference was held at the Royal Society, London, entitled "The Origins of Modern Humans and the Impact of Science-Based Dating." It was organized by the editors of this book of similar title, the important difference being that "Origins" was changed to "Origin," a more honest statement of which view was represented at the conference and in the volume. The resulting papers were quickly published in 1992, in the *Philosophical Transactions of the Royal Society of London*, Series B (337:125-250). They have been replicated in this handsome volume, with its cover featuring a photographic comparison of two Neandertals and the famous Cro-Magnon cranium in an unusual orientation (at least, unlike one cover of a conference collection from the previous decade, these are in focus). It is ironic that a volume stressing the impact of new dating techniques should feature this misleading comparison, as Cro-Magnon has been re-dated and is now thought not to be the earliest "modern" European; to the contrary, it is 10 kyr or more younger than the age of the earliest so-called moderns from the western edge of Eurasia.

The volume can be divided into dating, genetic, archaeological, and paleoanthropological sections. The dating part, the focal point of the presentations, may be the most important as it brings together discussions of the three main innovations widely applied to paleoanthropological problems by the scientists who have been applying them: uranium series (Henry Schwarcz), luminescence techniques (Martin Aitken and Hélène Valladas), and electron spin resonance (Aitken and Rainier Grün). In the process of revising *Paleoanthropology*, I found these

three papers (then available in the *Philosophical Transactions*) an invaluable source of information. They each combine clear explanations and critical reviews of the techniques that have revolutionized the dating of Middle and Late Pleistocene humans and their archaeological sites. I consider them "must reading" for any anthropologist interested in the current issues of Middle and Late Pleistocene human evolution. The Gifford Miller et al. paper on ostrich eggshell epimerization (an amino acid diagenesis process similar to racemization of bone) is more technical and somewhat less useful, especially as the authors choose to compare the method with other dating methods in the Border Cave sequence, thereby addressing the modern human origins question at a site where virtually all of the so-called early modern humans are without provenience. One of the great problems in the origins debate is the separation of fact from fiction, and it cannot be repeated often enough that the Border Cave adult cranium and most of the mandibles do not have provenience, however accurate the dates from the various levels in the site may (or may not) be.

Genetics of modern human origins are discussed by groups from what have become the two main centers of Eve theorists, Stanford and Pennsylvania State Universities. Some nuclear DNA data, as reviewed by the Stanford group (Joanna Mountain and colleagues), indicate that "the suggestion of an African origin for humans could reflect a migration out of Africa either 1-1.5 myr ago, 100 kyr, or both. Nuclear DNA provide no time scale." The mitochondrial analyses of Mark Stoneking and colleagues are somewhat more specific. They attempted a 95% confidence interval of "Eve's" age, and even tried (albeit unsuccessfully) to clarify the meaning of the ranges of estimates presented in previous works that were *not* calculated confidence intervals (for instance, to the 50-500 kyr estimate in the 1987 Cambridge meeting often referred to through the volume). The new calibrations presented here rest on the reasonable assumption that Greater Australia was first populated at

some 60 kyr, but the much less credible assumptions that the migration was only in one direction, and that it only happened once. Demonstrably incorrect are the further necessary assumptions that

- the rate of mtDNA evolution is the same in all human populations (especially remembering the importance of stochastic losses resulting from differing population histories),
- founding groups were not polymorphic in their mtDNA,
- all mtDNA variants are known.

What are these new dates? To find out, the reader will have to get the book or write for reprints, as this reviewer will not shoulder the responsibility for publicizing yet more dates derived from a biological process that cannot be expected to provide them. I would just say that the dates proposed are compatible with the "Eve" theory, but only if one assumes that Eve's age was very much older than the specimens which Eve theorists identify as the earliest modern humans.

Four reviews of archaeological sequences and their role in understanding modern human origins are presented. Hillary Deacon, whose excavations at Klasies River Mouth cave are justifiably regarded as superb, reviews the context, dating, and other details of this Middle Stone Age site, of course, focusing on the human remains found there because these are the source of its importance. His ventures into biological anthropology are somewhat less convincing than his archaeology, however. When writing that Bräuer et al.'s (1992) study of a new Klasies maxillary fragment "is an answer to the contention of Wolpoff and Caspari (1990) that the Klasies remains can be associated with archaic rather than modern humans on metrical analysis," Deacon is misleading because the point in the Wolpoff and Caspari analysis is that the first appearance of modern humans cannot be diagnosed by the earliest appearance of one or several modern features but by the characteristics of the sample. It is here that the Klasies material is revealed as an archaic one. Moreover, the published interpretation of modernity in the new fragments would be more convincing if the normal scientific checks and balances

system could operate and Deacon's specimens became available to other scholars besides those who agree with his beliefs.

Desmond Clark fully supports the Eve theory for modern human origins ("I see every reason to accept that anatomically modern humans evolved in Africa at the time suggested by the molecular record"), and yet realizes that archaeology provides no explanation for their spread and success ("they had not yet acquired the intellectual abilities that made possible the complex social organization and symbolic behavior of the Upper Paleolithic populations"). This is a surprisingly Eurocentric view from a scholar with worldwide archaeological experience, in a paper focused on African and Asian perspectives on modern human origins. In a sweeping review of African and Asian archaeology, he traces the African moderns to Middle Stone Age industries, like Deacon venturing into biological anthropology and stepping into a morass of conflicting opinions about very fragmentary skeletal data. My impression is that much greater clarity of thought, if not understanding of the past, could be achieved by stripping attempts to discern behavioral evolution from the archaeological record, such as this one, away from the seemingly obligatory efforts to associate these changes with the evolution and spread of modern human form. This lesson could be most productively applied to Europe, where the assumption that hominids and archaeological industries are associated has the greatest antiquity and is most firmly embedded in the interpretations of what happened during the Middle to Upper Paleolithic transition there.

In his review of European evidence for modern human origins, Paul Mellars defends the proposition that the transition from archaic to modern populations is later than in the rest of the world, and that it is associated with the spread of the Aurignacian. There is, he believes, chronological overlap between Neandertals and modern populations. Mellars contends that the "fundamental behavioral adaptations" of the "Upper Paleolithic Revolution," possibly including language, are responsible for the population replacement during the overlap

period. It has the hallmarks of one of the origins myths that Misia Landau (1991) describes. Although the situation in the Levant should have provided ample warning, Mellars assumes that in Europe the Middle Paleolithic is a Neandertal industry and the earliest Upper Paleolithic, Aurignacian, an industry of anatomically modern humans—this despite the fact that *diagnosable skeletal remains have yet to be found with the early Aurignacian* and of the fragmentary specimens found some (contra Mellars' assertions) have Neandertal (not just robust) characteristics. And what of the former earliest Upper Paleolithic, Châtelperronian (or Szeletzian in the east)? Despite its diagnostic features (Upper Paleolithic Industries, after all, are defined by type tools), with an associated Neandertal at St. Césaire the Châtelperronian must be considered terminal Middle Paleolithic in his scheme—or an example of “acculturation.” The best solution for the perceived contradictions was muttered at the Cambridge conference, “perhaps the St. Césaire Neandertal was a pet.” The new reconstruction of the La Chapelle cranial base (Heim, 1989) and the Kebara Neandertal hyoid (Arensburg et al., 1990) clearly show that, unlike the smartest poodle, these “pets” could have talked back, with a facility equal to that of their presumed owners according to Ralph Holloway (1985). Mellars' reliance on language to fuel his replacement scenario may be misplaced, and at the moment appears unsupported by the available evidence. For that matter, the “Upper Paleolithic Revolution” most archaeologists write of *follows* the earliest Aurignacian by a considerable time. Mellars' rendition of the often repeated replacement scenario in Europe is treated as common knowledge by many writers, but it depends on unverifiable, or in some cases outright invalid, assumptions. Even viewed in the most positive light (from the replacement standpoint), the anatomical and archaeological data, and especially telling in this volume their temporal details, are amenable to very different interpretations.

Like-minded, in the sense that they each purport to show that archaeological data support an African origins and replacement

model, very different evidence is used in these papers, and it might appear from them that the Eve theory has achieved broad consensus among archaeologists. Not so, however, as scientists who have analyzed archaeological data from these and other critical regions and believe they show no evidence of an out-of-Africa migration during the Eve period (Geoff Clark, Anthony Marks, Geoff Pope, Lawrence Straus, and others) were not there to present the results of their research. Moreover, in his analysis of the Levant sites, Ofer Bar-Yosef presents evidence that, while confusing in its own light, thoroughly undermines the idea that modern people sweep out of Africa with new ideas and behaviors and replace indigenous natives.

Bar-Yosef presents his usual thoughtful review of the Levantine sequence and its importance. He is particularly concerned with the TL and ESR dates that show the so-called moderns of Skhul and Qafzeh to be earlier than the Levantine Neandertals, if we can assume that the Tabun woman from level C is actually from B (another unresolved dating/provenience problem). This reverses the sequence every worker has proposed except Ted McCown and Arthur Keith (1939)—it is often forgotten that these paleoanthropologists, the last to study the entire Mount Carmel collection together, regarded the Skhul and Tabun remains as sampling one variable population and believed it to be descendent from a generalized stem hominid and evolving in the direction of European Neandertals.

Given the propensity of some scholars, especially Eve theorists, to regard Skhul and Qafzeh as “anatomically modern *Homo sapiens*,” one would think that the Levant played a critical role in modern human origins, as it would be here that the earliest complete modern skeletons could be accurately dated and provenienced. Yet, such a case is confused by three issues:

1. Differences in morphology seem to have nothing to do with differences in behavior.
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3. The differences in morphology can be characterized as “European-like” and “non-European-like” far more accurately than “Neandertal” and “anatomically modern.”

On the first point, Bar-Yosef raises the question of what behavioral variations underlie the anatomical differences and led to the Neandertal successes. As he (p. 139) admits:

the search for behavioral differences by looking at daily activities and motions . . . disclosed similar patterns for the two groups of industries. The only difference between the Kebara and the Qafzeh assemblages is the higher frequency of points in the former.

This undermines basic assumptions that paleoanthropologists make about the relationship of form and function, and suggests that the origin of the observed differences may be geographic rather than adaptive. There *is* a major behavioral change, but it is much later, after modern humans replace the Neandertals and become established in the region. Bar-Yosef grapples with the disassociation of this anatomical variation and the behavioral revolution he envisages between the Middle and Upper Paleolithic. He concludes that the behavioral change is independent of any evolving biological difference. Finally, there are very real questions of whether the specimens from Skhul and Qafzeh are truly “modern” in any reasonable sense (cf. Corruccini, 1993; Kidder, et al., 1992; Sohn and Wolpoff, 1993). These samples overlap with Levantine Neandertals far more than usually portrayed, and their deviations from the so-called Neandertal specimens of Amud and Tabun more likely reflect different races than different species. There has been an invalid, unrealized assumption that “non-Neandertal” means “modern” in the Levant (cf. Sohn and Wolpoff, 1993).

The inability to resolve these problems is a deep blow to the Eve theory, which posits—indeed relies on—a direct link between an intrusive, clearly identifiable modern human anatomy, and behavioral changes that provide the advantages that allow it to spread and prevail. Cognizant of these difficulties, Bar-Yosef proposes to model the Paleolithic transition after the Neolithic revo-

lution. This provides considerable insight for understanding the origin of modern human *behavior*, although admittedly not for the origin of modern humans, except for those few who, like myself, believe that people become modern when they begin to behave in a modern manner: Modern humans are a state of mind.

There are four papers by human paleontologists. Fred Smith grapples with the fossil evidence for the “Out of Africa” theory. He argues that because no unique features link the earliest modern Eurasians to Africa, the basis for this theory must lie in the demonstration that modern morphology appeared in Africa first. Like Reiner Protsch (1975), who first articulated a specifically “African Origins” hypothesis for modern humans (although reference to this fact is inexplicably missing from the review of the “Out of Africa” theory presented by Mellars et al. in their introduction to this volume), Smith examines the evidence for the two elements necessary to sustain this theory: (1) an African transition from archaic to modern anatomy, and (2) the identification of the earliest African moderns. He concludes that evidence for a transition from archaic to modern forms in Africa is good [contra Rightmire’s (1986) assessment], but that, despite the new techniques, dating problems still preclude the determination of whether transitions to modern anatomy were earlier in Africa than elsewhere. As for the earliest moderns, like Deacon he focuses on the Klasies River Mouth material as the best provenienced and dated of the human remains attributed to early modern *Homo sapiens* and examines the question of whether this attribution is justified. With its low frequency of chins, large and archaic zygomatic, and gracile central frontal fragment of unfortunately indeterminate age, he concludes (in agreement with Wolpoff and Caspari before him) that the Klasies sample is “not demonstrably fully anatomically modern.” His reading of the fossil record differs from most authors who support multiregional evolution in that he asserts “it is not my claim in this paper that the human paleontological evidence refutes an African origins model” (p. 244). However, he admits “the argument that modern humans *senso*

stricto appear earliest in Africa is equivocal.”

Also writing on the African fossils, Jean-Jacques Hublin focuses on the North African evidence from the terminal Middle Pleistocene and early Late Pleistocene. He is particularly concerned with the Jebel Irhoud remains, which he concludes lack unique Neandertal features and therefore cannot be considered “African Neandertals.” Citing evidence that the Irhoud crania predate oxygen isotope stage 4, he regards them as earlier than the European Neandertals (although they could easily be no older than the 100 kyr Krapina sample on this evidence). Hublin interprets them as “a grade immediately preceding the first modern humans of the Middle East.” Yet the dating evidence is not convincing, as the relation of the adult crania to the dated material (three horse teeth) is unclear, except for the contention that the crania were very probably higher in the cave’s stratigraphy. Moreover, the range of ESR dates does not imply great accuracy, 90–125 kyr for the early uptake assumption and 105–190 kyr for linear uptake. There is really no reason to suppose these crania predate the European Würm Neandertals. Two other recent papers have addressed this issue in a like manner, but view a much greater linkage between the circum-Mediterranean populations, in spite of the differences separating the Europeans from the Africans. Yoel Rak (1993) has a similar view, although admitting to more circum-Mediterranean gene flow. However, Rak *does* think speciation occurred during this period. He views the genic exchanges around the Mediterranean, from Spain to Morocco, as creating the potential for ring species to form because enough differences could accumulate for the terminal ends to have speciated. In his view the distinct populations were the European ones, and the European Neandertals thereby became isolated from the rest of evolving humanity. Yet, Tal Simmons and Fred Smith reach a quite different conclusion, although also showing evidence for late Pleistocene circum-Mediterranean gene flow from the Levant, across northern Africa and linking with the south. They argue that this persistent pattern, spanning greater than the last

100 kyr, shows that Africa could not be considered isolated from the rest of the world. Like Hublin they suggest that without isolation Africa is unlikely to have been the site of a new human species. In both cases there is fundamental agreement with Hublin’s conclusion (p. 128):

the fossil evidence would support the regular, even if accelerated, incrementation of modern features in the late Middle Pleistocene and early Upper Pleistocene rather than the quick and clear cut emergence of an Adamic “anatomically modern man.”

Peter Brown brings an interpretation from a different part of the world, presenting data from east Asia and Australasia. However, unlike most other scholars who have discovered and/or written on this material, he concludes that the evidence for continuity in these regions is too limited to validly interpret. Brown’s experience is mainly with the large (but regrettably reburied) Coobool Creek skeletal collection, and this might account for his caution in interpreting the much less complete sample of earlier fossil hominids, but there is more to it than that. He has maintained that most modern Australian characteristics come from post-Pleistocene adaptive changes, and that variable amounts of subtle but undemonstrable pressure exerted on children’s foreheads by mothers explains the flattened frontal shape differentially expressed in Late Pleistocene Australian samples. Less explicable are assertions he makes about some of the Australian fossils. For instance, the pivotal WLH 50 calotte is described as having a cranial shape that has “little in common with Ngandong”—a demonstrably incorrect contention (Thorne, 1989). In fact, an analysis of shared unique nonmetric features shows WLH 50 to resemble Ngandong to the complete exclusion of archaic-appearing Late Pleistocene Africans attributed to the Eve group, such as Laetoli 18 (Frayser et al., 1993).

Finally, Chris Stringer speaks for the proponents of multiregional evolution in comparing it to (what he calls) the recent African origins model in a multivariate analysis. Discarding doubts about the technique that

might arise when one understands its poor discriminatory power (Stringer admits "even between the *Homo sapiens* groups there is . . . a poor signal of regionality"), this most extreme of the Eve theorists shows (p. 179) that "the results of Penrose shape comparisons narrowly favor a late archaic African-modern special relationship over an East Asian-modern one." Stringer regards this as support for the Eve theory, although conceding (p. 192)

if we can turn away from a universal multiregional model for modern human origins, we cannot so easily exclude a dual African and East Asian ancestry model.

But what is this "universal multiregional model"? It is certainly nothing I ever wrote about, alone or with my colleagues. My respected antagonist is at it again, making up a "multiregional theory" that he can disprove. But, the fact is that European Neandertals could have been completely replaced by other populations some 30 kyr ago *as part of* a worldwide pattern of multiregional evolution, and this replacement would not prove "Out of Africa" is correct any more than the replacement of native Tasmanians by British proves an "Out of Britain" hypothesis for the origin of living populations. Neandertal replacement in Europe would be a disproof of the multiregional hypothesis only if it was part of a worldwide pattern of replacement of archaic by modern human populations from a single source, for instance, Africa, and this is manifestly *not* what Stringer's analysis shows.

There are a lot of assumptions shared by most of the papers in this volume, the main one being that modern humans and/or their behaviors first arose recently in Africa. Only a few authors, notably Hublin, take specific issue with this contention. Yet little direct evidence was presented to support this, the archaeologists largely turning to biological anthropology, the human paleontologists often relying on dates which, in spite of the dramatic advances in dating techniques discussed in the volume, often remain uncertain, and virtually everybody resting their case on the genetic advances in mtDNA analysis. Yet even before the conference was convened, in the February 7th issue of *Sci-*

ence Alan Templeton, following earlier work by David Maddison (1991), showed that the phylogenetic program being used to ascertain the place of origin for "Eve," PAUP, was incorrectly applied and that its valid use provided no knowledge at all about where the last common mitochondrial ancestor lived. Without this, the strongest support for an "Out of Africa" hypothesis was gone. In a subsequent publication Templeton (1993) went on to fully discredit the genetic basis for the "Eve theory." He showed that the "Out of Africa" interpretation of genetic variation could not be sustained for three additional reasons: (1) genetic variation, the other basis for African origins, had been incorrectly calculated; (2) the actual 95% confidence interval for Eve's age is an order of magnitude—70–700 kyr; (3) the mtDNA cladogram rejects a single ancestry model for nuclear DNA. Indeed, his conclusion (1993, p. 70) that "all humans represent a single-long term evolutionary lineage with regional subdivision and always have throughout the entire time period marked by mtDNA," undermines any basis for the comments attributed to a well-known paleoanthropologist attending the conference [according to a report by Stringer (1992, p. 603) there were "some strong statements from Clark Howell about the biological improbability of multiregional evolution"].

How could an international group of participants at a major conference be shown up so quickly? One might wonder whether there was any warning that so severe a blow was coming. Indeed there should have been, as Templeton's first (1992) paper on the issue had already been published and was cited by geneticists in writing up their conference papers, and his more substantive review of the "Eve theory" was the most widely circulated prepublication manuscript I have ever heard of (also see Barinaga, 1992; Gibbons, 1992). What happened, I believe, is that by inviting an assembly made up almost entirely of like-minded scholars to address the origins issue, the organizers seem to have isolated the only group of human paleoanthropologists and archaeologists who were blissfully unaware of what was about to happen. They made sure that it could be concluded, as Stringer was to write

later in his review of the conference (1992, p. 600), "we have seen significant growth in support for a recent African origins of modern humans." This impression was created by the simple but effective expediency of not inviting the opposition—neither the scientists invited to attend nor their audience were exposed to the multiregional evolution model or its biological, genetic, palaeontological, or archaeological bases. If a "truth in packaging" law were to be applied to volume titles, it might be better to rephrase this one as "The Origin of Modern Humans and the Impact of Chronometric Dating on One Side of the Controversy."

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HANDS. By John Napier (Revised by Russell H. Tuttle). Princeton, NJ: Princeton University Press. 1993. ISBN 0-691-02547-9. xiii + 180 pp. \$10.95 (paper).

Hands is an elegant treatment of the anatomy, function, and behavior of the human hand. In witty yet authoritative prose, John Napier manages to combine science,