The authors recommend expressing a patient's measurements as z-scores. The book would have been more useful if the composition of the sample providing the data for z-score computation had been specified and the means and standard deviations presented. These limitations do not detract from the book's chief aim, but those seeking guidance on the role of variation in clinical application will have to look elsewhere.

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NEANDERTALS AND MODERN HUMANS IN WESTERN ASIA. Edited by Takeru Akazawa, Kenichi Aoki, and Ofer Bar-Yosef. 1998. New York: Plenum Press. 552 pp. ISBN 0-306-45924-8. $79.50 (cloth).

"Not another one?" you ask. "Yes, another one," I reply. More focused in region and topic perhaps, and this is an important region and a reasonable topic, but indeed here is another volume on modern human origins, the same opera performed with the same parts being sung. The four sections of this volume, based on a November 1995 conference held at the University of Tokyo Museum, cover a broad range of related topics, including: 1) issues of evolution and chronology, 2) substantial writings on the archaeology of the region with cultural interpretations and subsistence strategies, and 3) views of cultural and human evolution from neighboring regions. The focus in this review will be on the issues of Levant paleoanthropology. Assuming familiarity with the conferences of the past decade, we might ask what progress there has been in understanding the paleoanthropological issues that this new compendium reflects. The answer is, not as much as hoped.

To cite one example, let us look at the question of when the Tabun woman died. Tabun is *the* key site for understanding Levant archaeology, as it continuously spans the entire archaeological sequence that is only represented piecemeal elsewhere in the region. The Tabun woman is one of the three fairly complete Levantine specimens identified as Neandertal, and not far from it was found Tabun 2, a mandible that some regard as an early "modern human." Thus the provenience of the Tabun woman in the cave, and her age, address the problem implicit in the title of this volume. The relationship between Neandertals and moderns in the Levant, even if these labels are valid descriptions of the hominids found there (this is questioned by Arensburg and Belfer-Cohen's insightful paper, "*Sapiens and Neandertals*"), must fundamentally depend on their dating. So after more than a half century of dating and analysis, it is fair to ask how old the Tabun woman is. Prior to this conference two problems confused this. First, differing interpretations of Garrod's excavations placed the woman's skeleton alternatively in Tabun layers D, C, or B. As Stringer notes ("Chronological and Biogeographic Perspectives on Later Human Evolution"), this has not yet been clarified. Bar-Yosef ("The Chronology of the Middle Paleolithic of the Levant") believes she is a burial from layer B, although there is a widespread assumption that she was found in layer C (she is, after all, usually designated as the C1 hominid), and thereby penecontemporary with the Tabun 2 mandible. Second, the dating attempts based on different techniques were providing widely variable results. Just focusing on previously published layer C dates, the electron spin resonance (ESR) models gave 102 and 119 kyr, the uranium series gave 101.6 kyr, and the weighted thermoluminescence (TL) average was 110 kyr (the probable error ranges are purposefully left off these figures, which vary by much more than the error ranges encompass). Papers in this volume do not hint that there is yet a more confident answer, and estimates of the woman's age are more dispersed than ever. The paper by Valladas et al. ("GIF Laboratory Dates for Middle Paleolithic Levant") gives a TL date of 171 kyr for layer C, while Schwarcz and Rink ("Progress in ESR and U-Series Chronology of the Levantine Paleolithic") give a direct gamma-ray spectrometry estimate of 60 kyr from elements of the skeleton. Subsequently this was published as 33 kyr (Simpson et al., 1998). One can now wonder whether the 50 kyr date of Jelinek (1982) was wrong.

The situation for the possibly-and-possibly-not-contemporary Tabun 2 mandible is equally confusing, although for different reasons. Rak ("Does Any Mousterian Cave Present Evidence of Two Hominid Species?") is quite specific about its modernity. In support, he uses: 1) characteristics of the shape and position of the mandibular notch, 2) the chin (while Lieberman hints here, and subsequently argues elsewhere (Lieberman, 1999), that various Late Pleistocene hominid chins are not homologous, his arguments are unconvincing), and 3) the endlessly-discussed retromolar space which Rak measures in a quite ingenious way that does not involve the space at all. For Rak, the mandible is evidence of two hominid species in the Tabun cave, an interpretation that he regards as leading to the fewest problems, although I would have thought there were some sizable archaeological ones (but see below). Quam and Smith ("A Reassessment of the Tabun C2 Mandible") would agree that the chin of Tabun 2 is not homologous, but this is because key elements are made of plaster. Relying on a different, more phenetic, approach than Rak's, they nonetheless reach a similar conclusion about the mandible's modernity. Quam and Smith interpret this in an evolutionary context, concluding that this is the earliest mandible with unequivocally modern features, but because there is only one example, it is not possible to choose between explanations for it based on homoplasy, an early origin for modern anatomy if not for moderns, or hybridization. Finally, Stefan, writing with Trinkaus (who is represented in this volume but addresses other issues), examined a number of discrete mandibular traits and some morphometric aspects of the Tabun 2 dentition (Stefan and Trinkaus, 1998). They analyzed the largest number and most varied set of features, and concluded, "its assignment to an early phase of the Near Eastern late archaic human lineage [is] the most parsimonious solution" (p. 465). They quite specifically note that it "can be categorically separated from the Qafzeh-Skhul (and European earlier Upper Paleolithic) hominids." We might be able to untangle these contradictory interpretations if we accept Otte's ("Turkey as a Key") view that modernity is a myth, a mirror opposite of the 18th century's "primitive man" that should be replaced with the understanding that "one does not find the abrupt appearance of a Modern Man, or of new behavior, but rather *processes* of a slow evolution that are distinct and complex." This would reconcile all the interpretations of Tabun 2 quite well, by removing the taxonomic question it is constantly used to address.

However, *less* taxonomizing is hardly the direction of paleoanthropology's latest lurch. Howell's introductory paper ("Evolutionary Implications of Altered Perspectives on Hominine Demes and Populations in the Later Pleistocene of Western Eurasia") provides an example. In it, he proposes a unit of analysis for paleontological problems, the paleo-deme or p-deme, that is meant to lie between the individual and the species. P-demes are described as the essential components for the recognition of species, the basis for assessing phylogenetic affinities, for recognizing evolutionary trends, for studying dispersals and extinctions, and much more. But for what *are* they? Howell writes, "past hominine demes are denominated as p-demes" and a deme is "a communal interbreeding population within a species." The trouble is that populations do not persist as such; they endlessly expand and divide, or merge with other populations, or become extinct (Moore, 1994; Templeton, 1998). And there are more problems. While p-demes are meant to reduce the "emphasis on incomplete/ambiguous specimens," one p-deme Howell names is the Dmanisi mandible, while another is the femur fragment from Gesher Benot Ya'aqov. He insists on using phylogenetic procedures for establishing their relationship, even though p-demes can and do reticulate. But it is also possible that the approach would be more useful if the information it contained was more accurate. In discussing the "Cro-Magnon p-deme," for instance, Howell describes it as being "intrusive (with the Aurignacian technocomplex)," echoing similar statements by Kozlowski ("The Middle and the Early Upper Paleolithic Around the Black Sea") and Mellars ("The Impact of Climatic Changes on the Demography of Late Neandertal and Early Anatomically Modern Populations in Europe"). Yet, we might ask, is there any evidence linking the origin of modern Europeans to the beginnings of the European Aurignacian? Howell admits that most human remains are associated with the "Middle Aurignacian," not with the first occurrences of the industry, and although in a detailed and complex discussion of the problem he provides dates for the earliest members of the p-deme, they are from sites where the specimen associations with the dated materials are uncertain. Howell describes specimens from sites such as Mlade as having "nothing 'transitional' or 'intermediate' with respect to their morphology in any instance," contradicting authors who have studied the remains (Frayer, 1986; Jelínek, 1983). Other specimens with various combinations of "unique" Neandertal features associated with Aurignacian tools from levels G_l and F_d at the Vindija cave in Croatia are not mentioned, even when the associations are with middle-range Aurignacian levels, as are the F_d teeth. There are reasons to believe that p-demes are no more than an added level of unnecessary taxonomic burden, but it could also be said that the p-deme approach has not been given a fair chance.

Perhaps no chance could be fair enough because our information is just not good enough. Lovejoy often says in lecture that understanding the form and function of crania is much more difficult than understanding postcrania, and it is Rak (p. 353) who quipped, "the jaw is the work of the Devil." Analysis of postcranial evidence seems to be much more straightforward, but perhaps also with more surprising conclusions. Rosenberg ("Morphological Variation in West Asian Postcrania") provides a long-awaited discussion of female pelvic anatomy, whose importance lies in the fact that, as she points out, all prior discussions of the birth characteristics of Neandertal pelvic anatomy have been based on *males*. What allows her to do this is her comparisons with the archaic Chinese pelvis from Jinniushan, discussed here. But the fact is that there has been a fairly complete Neandertal female pelvis all along, i.e., Tabun, and its analysis has been ignored. Tabun has pubic length characteristics and pelvic inlet dimensions that scale to a modern woman of her size: nothing unusual, at least as far as the birth process is concerned, according to Rosenberg. The male pelves are indeed different, but not just Kebara; note also Skhul 9. Rak (1993) to the contrary notwithstanding, this is yet to be explained in a biomechanical or behavioral context. Rosenberg suggests, but subsequently rejects, the idea that perhaps selection on females resulted in a correlated response (see Lande 1980) in males, in a sense "dragging them along." Her data suggest an intriguing variation of this; perhaps selection on *male* pelves, especially on the pubic region, created a correlated response in the *females*, in effect dragging *them* along.

Trinkaus, Ruff, and Churchill ("Upper Limb Versus Lower Limb Loading Patterns Among Near Eastern Middle Paleolithic Hominids") examine a different aspect of postcranial adaptation. They do cross-sectional strength analysis for the humerus, femur, and tibia, and some of their results are unexpected. One point that could potentially confuse comparisons is the fact that they include Shanidar in a "Near Eastern" sample, while some other authors do not and restrict their archaic (or Neandertal) sample only to the Levant hominids. In the forearm they show that the archaic sample (Shanidar, Amud, Tabun) is both stronger than Skhul/Qafzeh and has a higher magnitude of strength-related asymmetry, so that the most significant differences are in the right arms. In the legs, however, there are no significant differences in strength. This is important because it addresses the climatic variation hypothesis. If the differences between these samples do not signify different strengths as the result of locomotor-related activities, they might instead reflect differences in climate in the regions where the populations originated. Since the longer distal limbs at Skhul/Qafzeh are supposed to be a consequence of their tropical origins, this looks good for an African Origins hypothesis. It does not look as good for explaining the similarities in the archaeology and other evidence of behavior, and these authors puzzle over why "a series of archaeological studies has failed to demonstrate major behavioral differences," like ones that should result from the forearm comparisons.

One explanation is that there may be a snake in this picture of Eden. Direct comparisons of limb proportions do not clearly support a climatic explanation of their variation. For instance, Skhul 4 and 5 are quite different in the distal-to-proximal proportions of both arm and leg bones, and Amud's crural index of 80 can no more be said to reflect a cold adaptation than Skhul 5's does. The strength comparisons are also problematic. Trinkaus, Ruff, and Churchill use the area moment of inertia for strength estimation, and this moment is scaled to body size by dividing by the 4th power of bone length (a 5th power denominator for the tibia). These adjustments were developed from beam analysis equations that assume structures with regularity in form and material distribution and density. There is no reason to be sure that the same approach can be used to compare structures that vary differently in shape, and the distribution and density of material. And the consequences of this adjustment should be clear. Elevated by the 4th or 5th power, the estimates of strength are extremely sensitive to limb length differences, and the Skhul/Qafzeh skeletons have longer limbs, especially distal limbs. If this is overcompensation, it is possible that the actual forearm strengths and loading patterns of the two groups are more similar (this would not affect the dimorphism comparison as greatly), while leg strength could be greater in the Skhul/Qafzeh sample. Such a result would be more in line with the locomotor implications of differences in leg proportions and limb length differences, and with the archaeological data.

And what are these archaeological data? They are mostly about Neandertals and their behaviors. In interesting papers, Stiner and Tchernov ("Pleistocene Species Trends at Hayonim Cave") and Klein ("Why Anatomically Modern People Did Not Disperse From Africa 100,000 Years Ago") address the significant question of population size and its increase towards the end of the Late Pleistocene. Klein, *not* writing about either Neandertals or the Middle Paleolithic, found evidence of increasing population density in southern Africa, beginning some 50 kyr ago, in the decreasing size of tortoises and mollusks at the Late Stone Age sites. Stiner and Tchernov found the same evidence of low harvesting rates of easily gathered species, which they interpret to imply low population density, but in this case in the Middle Paleolithic at Hayonim. In both areas subsequent changes happened at about the same time and reflect increasing population densities and not new technology or organization. What is significant at Hayonim is the fact that the striking increases in population density were *within* the Middle Paleolithic of the Levant, over a period when archaeologists such as Bar-Yosef suggest Neandertals were prevailing at the expense of "moderns." If so, it was the Neandertal populations undergoing the expansion, and this undercuts the suggestion of replacement-archaeologists such as Mellars (p. 499) that replacement was easy in Europe because Neandertal population densities were low.

Neandertals continue to exceed paleoanthropological expectations, coming up as similar to "early modern humans" in their behaviors so often that one might wonder if it is not time to disentangle their behavior from their biology, which of course would be another example of being similar to modern humans. The fact is, as Trinkaus, Ruff, and Churchill, Lieberman, and others in this volume note, Neandertal and Skhul/Qafzeh behaviors in the Levant are not archaeologically distinct. Henry ("Intrasite Spatial Patterns and Behavioral Modernity") concludes: "Clearly, the hominids who occupied these sites some 70K years ago had the capacity to both anticipate needs beyond their immediate future and to significantly alter their adaptive strategies in response to differences in their environment. The rather detailed reconstructions of their inter- and intrasite patterns fail to show any significant differences with those of fully modern humans" (p. 141). Speth and Tchernov ("The Role of Hunting and Scavenging in Neandertal Procurement Strategies") note that "failing to find major behavioral contrasts between the two hominid taxa [of the Levant] reflected in their stone tools or burials, some archaeologists have turned to the faunal remains, suggesting that AMH were skilled hunters of large, often dangerous prey, whereas Neandertals . . . were opportunistic scavengers" (p. 224). But these authors provide evidence that archaeologists such as Ambrose ("Prospects for Stable Isotopic Analysis of Later Pleistocene Hominid Diets in West Asia and Europe") are incorrect in the supposition that Neandertals were for the most part scavengers, and only inefficient hunters, and "had predation patterns like those of carnivores rather than humans" (p. 286). To the contrary, Speth and Tchernov show that Neandertals leaving their debris at Kebara were fully effective hunters, "preferentially targeting large, and potentially dangerous, prime adult prey" (p. 236). Finally, writing on Levant Neandertal cultural and mental capacities, Goren-Inbar and Belfer-Cohen ("The Technological Abilities of the Levantine Mousterians") discuss the modularity and flexibility of the raw material choices and technology at Levant sites. For them, the mosaic nature of the various cultural attributes reflects modern mental capacity and processes, and "endeavors to explain the cultural variability . . . as reflecting the 'demise' of Neandertals and their failure to compete with their mentally better-equipped *Homo sapiens sapiens* contemporaries seem to belong to the realm of fiction rather than that of science" (p. 218).

Yet what are the implications of these behavioral similarities? Lieberman ("Neandertal and Early Modern Human Mobility Patterns") would have us believe there are just about none.

> If Neandertals and early modern humans [in the Levant] were behaviorally more similar to each other than either are to recent modern humans, should we place them in the same taxon as some researchers argue? The answer is clearly no. . . . Morphological characteristics, not archaeological residues, are the . . . most reliable source of information to assess their systematic relationships. Neandertals and early modern humans most likely belong to separate taxa because each have a unique set of derived characters (autapomorphies). (p. 272)

Fair enough. Lieberman mentions several and Hublin ("Climatic Changes, Paleogeography, and the Evolution of the Neandertals") lists 25 of these autapomorphies in a table, while Rak adds mandibular features to the list, making a most impressive rendering which, if correct, could lend powerful support to the notion that Neandertals are a distinct taxon. But are they correct? The assertion that Neandertals have unique shared derived features, or autapomorphies, has not held up to scrutiny. Creed-Miles et al. (1996), Frayer (1993, 1997), Szilvássy et al. (1987), Tillier ("Ontogenetic Variation in Late Pleistocene *Homo sapiens* From the Near East: Implications for Methodological Bias in Reconstructing Evolutionary Biology"), and others show that for case after case this is *not* the case.

Tillier's argument is based on ontogenetic comparisons of European Neandertal and Upper Paleolithic children, with the Skhul 1 child. She shows that ontogenetic comparisons are necessary, and that adult anatomy generally is not reflected in the anatomy of children, but at the same time the results of ontogenetic comparisons depend largely on the comparative sample employed. They also depend on an accurate assessment of the range indicated by the comparative sample. For instance, in the analysis of Skhul 1, Tillier shows that its cranial breadth is low relative to its age, when compared with European Neandertal children, and even lower than Predmosti 6, one of the Upper Paleolithic children in the sample. But the *distribution of breadths* for the fossil children is well within the range of the modern comparative sample, a point made all the more significant by the uncertainties in age assessment for the fossils, and a null hypothesis of no significant difference cannot be rejected. This conclusion, and approach, are quite different from the analysis of the Dederiyeh infant by Dodo, Kondo, Muhesen, and Akazawa ("Anatomy of the Neandertal Infant Skeleton From Dederiyeh Cave"). When these authors make comparisons with a modern sample, it is with Japanese children. Just to look at one consequence of this, the 2-year-old Dederiyeh infant has a parietal breadth of less than 130 mm, which is well above both the Japanese mean of 118 mm for 2-year-olds and even the 2-year-old maximum of 125 mm. However, it is much closer to Tillier's Slovakian comparative sample (mean of 126 mm, with a 75% range that includes Dederiyeh). Minugh-Purvis ("The Search for the Earliest Modern Europeans") compares Skhul 1 and the Krapina 1 child, in an analysis that exposes an additional key variable, i.e., uncertainty in age determination. Her assessment of the Skhul 1 age is 4.5 years, considerably older than Tillier's 3 years. The Krapina 1 age is even more difficult to estimate, as there are no associated facial or dental remains. More problematic is the fact that while Minugh-Purvis claims the sagittal length of the parietal *most strongly separates* Neandertal and modern samples, this is the cranial dimension in which Krapina 1 is very much like Skhul 1. It can hardly be a modern autapomorphy. In all, without really meaning to pun, it could be said that in some respects ontogenetic studies of fossil remains are in their infancy.

Alleging the presence of autapomorphies that aren't autapomorphies is not a new thing. More than a half-century ago, Weidenreich (1943a, 1947), addressed this and commented on one of the autapomorphies that Hublin lists:

> [It is] a sport of a certain group of authors to search for the skeletal parts of Neandertal Man for peculiarities which could be claimed as "specialization," thereby proving the deviating course this form has taken in evolution. (1947) . . . Adloff and Keith found that the molar teeth of European Neandertal Man are characterized by a particular spaciousness of the pulp cavity which, according to them, was thought missing in anthropoids, as well as *H. sapiens*. The authors, therefore, considered taurodontism an expression of specialization characteristic of Neandertal Man. Since the peculiarity would not appear in modern Man, they concluded that their bearers, that is the Neandertalians, must have been extinguished without leaving any descendants. But already at the time when this feature was claimed as an example of specialization, it was known that both presuppositions were erroneous. Typical taurodontism occurs in orangutan and chimpanzee and, on the other hand, is not rare among certain races of modern mankind, as for instance, Eskimos and Bushmen. Thus [it might be possible] . . . that the European Neandertalian inherited it from an orang-like ancestor and transmitted it to the Eskimo. But in no case could it be concluded from these facts that the Neandertal man had disappeared from the surface of the earth without descendants. (1943a)

The constant repetition of autapomorphic lists as in the Lieberman, Rak, and Hublin papers provides its own best, and in many cases its only, supporting evidence. As far as the validity of a Neandertal species defined by autapomorphies is concerned, "if repetition could make a thing true, then it would be most emphatically and wonderfully true . . ." (Hertzberg 1998, p. 5).

In any event, the idea that species can be defined by autapomorphies in closely related groups has been proposed before, and it is problematic. Using similar criteria, Hill (1940) concluded, "it is impossible to escape the view that there are several 'species' of living man, and several more fossil kinds." He proposed to restrict *Homo sapiens* either to "white man," or to some even more specific group of Europeans, to which Weidenreich (1943b, p. 245) responded with more sarcasm, this time about how one could account for different living human species:

> Modern taxonomists consider "sexual aversion" a sufficient specific difference. Now, why not extend this to political aversion also? In recent history political aversion has assumed the form of sexual aversion (see some sorts of Whites and colored peoples, or "Aryans" and "Non-Aryans"). There is indeed a parallelism between . . . two bird groups which are specifically different, because they do not interbreed under natural conditions—attitude in public in the case of the human examples—

but do so in captivity—private life attitude.

More than a half-century after Weidenreich wrote, he is not out of date. Can the same be said for the contents of this much more recent conference? Certainly even as the dates oscillate like the vibrating electrons that many of them are based on, nothing substantive has changed in the fossil record of the region. The Tabun skeleton still has too many dates, and if anything, more than ever before. Systematic anatomical and archaeological comparisons of the several remaining contenders for the earliest modern human (Qafzeh, Klasies, and Omo Kibish) are yet to be made, and the whole idea that some humans 100,000 years ago can be interpreted as more primitive than others because of their anatomy or archaeology is yet to be reconciled with the implications of making such statements to describe the equivalent range of anatomy and behavior today.

But the genetics—ah, that is another story. In the 4 years since this conference, the landscape of genetic interpretations has changed quite considerably (Bower, 1999; Strauss, 1999). Pilbeam, in his postconference afterword, notes that "the Neandertal nonmodern population was genetically quite different from modern humans, at least as different as the central and eastern subspecies of chimpanzee are from each other" (p. 252). On the one hand this is more different than modern human races are from each other (Templeton, 1998), but on the other hand, as Pilbeam notes, it is like variation *within* a higher primate species, in particular the one most closely related to us, and in chimpanzees this variation is expressed across a limited portion of Africa. This does not support the contention, repeated several times in this volume, that Neandertals must be interpreted as a separate species. It is not clear what it *does* support, though, as the genetic comparisons are between a single ancient sample and many existing ones, and not between the ancient sample and its contemporaries, and this makes a difference (Nordborg, 1998).

Pilbeam also wrote, "most [modern human origins models] are clustered toward the 'replacement' end of the spectrum. . . . We have evidence for the youth and unity of living humans" (p. 526). Apart from confusing the fact that there are only two modern human origins models, replacement and multiregional evolution (so they can hardly cluster at different ends of a spectrum), the years have not treated the contention of youth for the human species particularly well. Comparisons of autosomal and haploid systems in the same individuals (Hey, 1997; Jorde et al., 1995) rule out the recent single population size bottleneck interpretation of genetic variation that Stringer posits, and that the replacement explanation and youthful species interpretation requires. If modern humans only lately began as a new species evolving from a small isolated population, we would expect autosomal and haploid variation to have been reset to a very low magnitude at that time. The haploid variation should have recovered much more quickly and reached equilibrium sooner. But it is the autosomal genes that are variable and in mutation-drift equilibrium today, while nonrecombining systems, including the haploid mtDNA, lack variation and are out of equilibrium.

Trying to account for this, the long-necked bottle theory of Harpending et al. (1998) solves some of these problems, although by positing a long period of very small population for human ancestors. This would allow autosomal and haploid systems to coalesce at different times; the haploid coalescence is a quarter the time of the autosomal coalescence, since haploid population size is one quarter. But this interpretation is incompatible with a recent bottleneck. Furthermore, it requires that for a million years the ancestral population of humans lived in isolation "in an African area the size of Rhode Island or Swaziland" (p. 1967). Such an Eden began a long time ago. It would have to be small and African and *isolated*, and is unlikely to have been located in the Levant, at the African gateway to the rest of the world, which the volume editors describe as a "crossroads." In other words, if such an Eden existed, it was not in Western Asia.

It is always possible that what we need is another conference.

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