

DISCUSSION AND CRITICISM¹

On the Family and Change in the Middle East

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The papers on the Muslim family today (CA 26:555–80) offer a comprehensive view of the subject and of its difficulties. The authors more or less agree upon certain facts which contradict the familiar Western representations of the Muslim family: polygamy is uncommon, divorce rare; the conjugal unit is important; the traditional family institutions exhibit relative stability despite the pressure of the Western model; and so on. On the other hand, they all seem to run into the same methodological difficulties and, at times, to fall into the same theoretical traps.

While Western models are rejected, representations of the Muslim family are taken literally when they are “Eastern.” Thus, Al-Thakeb and Nassehi-Behnam do not recognize that the Middle Eastern preference for father’s-brother’s-daughter marriage is seldom realized in more than 10% of alliances (see, e.g., Granqvist, Rosenfeld, Ayoub, Peters, Barth, Khuri, Antoun, Cresswell). Similarly, an extremely high incidence of “endogamous” marriage is claimed, but the social unit of reference is not specified. Patrilineality is emphasized even though matrilineal kin and even affines may actually be extremely

More on Zhoukoudian

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Binford and Ho (CA 26:413–29) have provided a review of the evidence for human activities in the Zhoukoudian lower cave that is interesting and provocative. Much of what they write should have been said long ago, and the review itself is an important contribution to our understanding of the depositional circumstances surrounding the burial of hominid remains in this cave. Yet they raise many points that I do not agree with, and I feel that their conclusions do not follow from any of the evidence they discuss.

A number of the authors’ contentions about the Zhoukoudian hominid remains are in my view absolutely correct. These points should be emphasized, and, lest any worker should argue that they cannot be determined from the literature alone, in my case they are based on conclusions drawn while examining the remaining specimens in Beijing and visiting the site. For instance, Binford and Ho argue that the evidence for fire at Zhoukoudian precludes the interpretation that hominids were able either to make or to control it. While this conclusion is drawn “at a distance,” my observations of the stratigraphy at the cave and of the extent and thickness of the ash layers fundamentally support it. Also, there is no evidence of burnt human bone, even among the bones from the postliberation excavations. There is no evidence of hearths in the deposits I examined, and Binford and Ho’s ash-layer interpretation

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important in economic and social matters. Certainly one must take into account the actors’ representations of the family and its functions, but this is not enough. These authors tend to forget that, above all, the family incorporates society’s reproductive relationships, which are governed by cultural norms as well as by individual and collective strategies that may themselves induce changes in the norms.

Like marriage institutions, academic discussions have their norms and codes. Sociologists and anthropologists are so accustomed to them that they sometimes forget to reflect on the meaning of the words they use. What is the meaning of these typologies in which the conjugal family and the extended family appear as two distinct types? We are treated to long discussions about whether the Muslim family belongs to the conjugal or to the extended type, but in fact these polar types may rather be aspects of one and the same phenomenon. Even in the West, every conjugal family is part of a larger, more or less dense network of kin and affines. And in the Middle East, every extended family is a more or less integrated, more or less structured whole made up of allied conjugal units.

From inventing typologies it is but a step to constructing evolutionary transformations, yet those who preach “the golden mean” between the extended family (identified with tradition) and the conjugal family believe that they are protected from such a transition to the Western model. This approach is theoretically unsound and cannot be a guide for policy.

How, then, should one analyze changes in family patterns in the Middle East? As everywhere else: by identifying the new factors, and new actors, and the new strategies. Whether such knowledge will facilitate the guidance of social change is another story.

seems to me absolutely correct. The discussion of the H3 (Cranium V) vault, pointing out that Weidenreich in his description had probably confused breakage with an inflicted mark, is particularly appropriate in that the discovery of most of the remaining portions of the vault has made it clear that it was indeed broken and not eaten. Binford and Ho’s further interpretation of animal gnawing as the cause of breakage in this specimen must, however, be rejected as a consequence of these finds.

Some of the points raised are more problematic. I am surprised to find a lack of elementary statistical procedures in a work at least in part written by Binford. For instance, using three femora, a clavicle, and a humerus (actually small fragments of these bones) to show a higher percentage of postcranial remains (in a sample that also included a total of 25 teeth and 5 small craniognathic remains) when compared with the percentage for the rest of the site would seem to require an elementary statistical test to ascertain whether the difference was significant. While this point should not be belabored, this case is not unique but one of many.

Generally, Binford and Ho pay little or no attention to what the hominid specimens discussed are actually comprised of. For instance, what they refer to as Skull XIII of Locus O is actually a maxilla. Again, in discussing the Locus L crania they conclude that because the specimens were deep within the cave, and given the absence of associated tools, “these remains were transported by either animals or water.” This discussion would have benefited from a knowledge of the remains themselves. The crania are the three most complete from the site. Binford and Ho seem unaware that Weidenreich associated numerous facial bones with these crania, and consequently their count of skeletal remains does not include PA 98 (the frontal process of a maxilla and a left zygomatic bone) and PA 99 (much of a maxilla with P3-M3 and half of an edentulous palate). Moreover, in writing of Skull X, Locus L, Weidenreich (1943:13–14) stated:

The skull was badly crushed, but the individual fragments were found close together within loose soil so that their assemblage and readjustment was not too difficult. However a large number of small particles remain unplaced since they fail to show any connections either with one another or with the larger fragments. Most of them apparently belong to the facial skeleton. . . . The conditions were furthermore complicated by the recovery of skull XI, likewise crushed, at a distance of not more than 50 cm. Fragments of facial bones belonging to this skull were found intermingled with those of skull X.

This is hardly the description one would expect for remains far from the cave entrance if "hominid carcasses or parts thereof were introduced to the active, entrance area of the cave . . . then further dispersed within the cave, most likely by bone-carrying animals such as hyena or wolf" (p. 428).

Criticizing our Chinese colleagues for not showing an up-to-date knowledge of the (ever changing) taphonomic literature ignores the consequences of the recent hiatus in the development of Chinese paleoanthropology, one of which was a hiatus in access to most Western literature. Moreover, Pei, who was the actual excavator for most of the preliberation discoveries and who is preparing an illustrated history of the Zhoukoudian excavations, seems quite aware of these interpretive problems in private conversation and tends towards a conservative view of the evidence for hominid activities at the site. Also, in fairness to other Chinese colleagues, one should note that many if not most of the interpretive statements attributed to them were actually quoted by them directly or indirectly from the Western literature.

My greatest difficulty with this paper concerns its attempts to dismiss with a taphonomic explanation what may well be the only important evidence of hominid behavior at Zhoukoudian. I refer to the lack of faces and cranial bases associated with the vaults, described (p. 414) as "a striking characteristic" of the Zhoukoudian hominids. Binford and Ho ascribe this condition to "a combination of movement and pressure in geological contexts," citing in support of this contention the "fact" that "essentially all the fossil hominid skulls from Indonesia lack faces and have broken bases." Yet one wonders why Indonesia should be chosen for this comparison, since almost all of its hominids are in gravel deposits "and other contexts showing they had been rolled and tumbled in streams prior to being deposited." The South African australopithecine caves are a much more reasonable basis for comparison, especially since Binford and Ho admit that they are "geologically very similar to the cave at Zhoukoudian" and since they make some of the comparisons themselves.

Using data I have collected (up-to-date except for the Sterkfontein discoveries of the past two years), table 1 shows the numbers of specimens with at least portions of both the frontal and of the face (since the face is adjacent to the frontal, this is a useful means of examining the question of whether or not the face has been detached without the potential confusion with whether the vault's *whole* anterior is missing). Also enumerated are specimens represented by frontal portions but no facial portions, specimens represented by facial portions but no frontal portions, and specimens represented by postcranial remains at each site. The four samples compared are from Members 4 and 5 of the Sterkfontein cave, Member 1 of the Swartkrans cave, the entire Zhoukoudian lower-cave collection known to date, and the Middle Pleistocene hominids from Indonesia (including Ngandong).

It is quite evident that the proportions (and in fact the actual numbers) of frontofacial associations in the two South African caves are virtually identical (as one might expect from the taphonomic and geologic analyses) and glaringly different from the Zhoukoudian figures. Expressed as proportions of the total number of specimens represented by frontofacial remains (for instance, 10 for Zhoukoudian), there are one and a half to two times as many associations of frontal and face in the South African caves as there are in the Zhoukoudian sample (the chi-

squares comparing either South African sample distribution to Zhoukoudian are significant at the .05 level). This is not the only difference; isolated portions of the face are very much rarer (virtually nonexistent) in the two Asian samples, while frontals without any facial elements are about ten times more common. All these differences are also significant at the .05 level. For these three comparisons the Indonesian sample is exactly the same as the Zhoukoudian sample (no doubt by chance, but it surely suggests that the underlying sample percentages are very similar). This indicates in the strongest possible way that the differences in parts represented between these African australopithecine samples and Asian samples of Middle Pleistocene hominids are not attributable to taphonomic factors.

Therefore, the fact that the two Asian samples are so similar in preserved body parts at the very least provides grounds for suggesting that there is *something* in the behavior of the hominids that resulted in the very similar body part and association proportions. And these data are not the only sources of suspicion that the Middle Pleistocene hominids of Asia behaved differently from the australopithecines, particularly with regard to the crania of their own species. For example, there is the almost certainly related absence of cranial bases in the Zhoukoudian and Indonesian samples, contrasting with the many bases preserved in the australopithecine caves. Another example is found in the healed cranial fractures that are virtually unknown among the Lower Pleistocene hominids of Africa and ubiquitous in the Middle Pleistocene humans of Asia.

In contrast to these craniofacial data, it is interesting that the number of limb fragments, also taken as a proportion of the frontofacial sample size described above, is similar in the three cave sites and in all three caves many times more frequent than the proportion of limbs in the Indonesian sample. In this comparison there is the potential for a purely taphonomic explanation, and (in the converse comparison) the comments made by Binford and Ho regarding the similarity in proportions of craniodental materials in these three cave samples are essentially correct.

Finally, it bothers me that in concluding Binford and Ho can completely, and almost certainly accurately, discount all previously claimed archaeological and geological evidence for human activities in the cave and then render a broad conclusion about the evolution of human behavior with the most wide-ranging implications. To the question of what life in the cave was like, they reply, "We do not know." To the question of what the diet of the Zhoukoudian hominids was like, they reply, "We do not know." Then, in the same paragraph, they assert: "our conclusion is consistent with the growing body of evidence showing that early hominids were not predators. . . . hunting seems to have been a very late modification in the hominid niche. . . . it seems to have played little if any role in conditioning the anatomical or distributional changes of the mid-Pleistocene." I consider this a striking non sequitur that detracts from the value of what is otherwise at least in part a potentially useful review.

TABLE 1
DISTRIBUTIONS AND ASSOCIATIONS OF SOME HOMINID
SKELETAL REMAINS

	NUMBER OF SPECIMENS REPRESENTED BY			
	Frontals Only	Faces Only	Frontals and Faces	Postcranials Only
Sterkfontein	2	14	7	16
Swartkrans	1	13	9	13
Zhoukoudian	7	1	2	9
Indonesian	14	2	4	4