Current events

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Wright for the wrong reasons

I appreciate the opportunity to further discuss the role of multivariate analysis in assessing the affinities of Shandingdong (the Upper Cave at Zhoukoudian) 101. Here is a skull that most scholars have regarded as East Asian in affinity, beginning first with Weidenreich (1938/1939), the only one who was able to study the original specimens. Although often cited as questioning the Mongoloid affinities of UC 101, Weidenreich (1938/1939) actually questioned their *Chinese* affinities, "in so far as it is permissible to use this designation in determining a race". It was his contention that the three Shandingdong crania "show certain common features" of an Asian sort, but typify "three different racial elements, best to be classified as primitive Mongoloid, Melanesoid and Eskimoid". In particular, he wrote ". . . recent North Chinese may be considered as more advanced types, but traceable to ancestors like those represented by the Upper Cave man". Weidenreich's analysis (1938/1939) was addressing the general question of racial origins, whether there once were pure races that subsequently mixed to form today's hybridized populations. He took the Shandingdong specimens as proof that there never were. Weidenreich (1938/1939) reasoned that if races had been "pure" in the past, and mixed with each other more and more over time, until achieving their present state in which no pure races are left, we would expect that variation of a past sample should be less than today's, as there was less intermixture in the past. But Shandingdong showed, if anything, *more* variation. Weidenreich (1938/1939) suggested the variable Shandingdong sample may be ancestral to native Americans, and if so it would account for their variation without positing separate migrations from different populational sources (as some were doing at the time).

Neumann (1956) strongly supported the idea that the Shandingdong specimens were generally Mongoloid ancestors of Native Americans, and Oschinsky (1964) detailed additional resemblances to "New World Mongoloids". Coon (1962, pp. 474–475) wrote that UC 101:

> does not conform strictly to a Mongoloid model, but neither do all Chinese alive today. . . . [With the other crania it] bear[s] the same kind of relationship to the modern Chinese that the Upper Paleolithic skulls of Europe do to modern Europeans.

Finally, in their review of Late Pleistocene human evolution in China, Wu Xinzhi & Wu Maolin (1985) of the Institute for Vertebrate Paleontology and Paleoanthropology confirmed the Mongoloid affinity of the three crania, and argued that in comparison with modern populations, and taking age and sex into account, they were relatively homogeneous. In an earlier, more detailed analysis, Wu Xinzhi (1961) had already concluded:

> there is no reason to consider the Upper Cave fossils as representing anything other than a Mongoloid population entirely consistent with what is known about the development of modern *H. sapiens sapiens* in North China [Wu's translation (1961)].

With so long a pedigree, UC 101 should have been a good test case for different analytical procedures. But three multivariate analyses have contradicted these anatomical studies and

concluded the specimen is not Mongoloid—Howells (1989) and two others based on his dataset: Van Vark & Dijkema (1988), and Kamminga & Wright (1988) and Kamminga (1992). Those who conducted these analyses continue to vouch for their validity, indeed for their ascendance over more traditional means of analysis by scholars studying the material (or in this case the casts, as the original specimens disappeared with the Lower Cave remains). The latest example is Wright's continued defence of the notion that his multivariate analysis provides morphological information that UC 101 is non-Mongoloid.

Wright raises a valid objection to my statement about the Giles-Elliot (1962) discriminant function. I checked the measurements I made on the primary UC 101 cast in Beijing. They are close to those he reported. The cast in our osteology laboratory is an old, secondary one which measures somewhat higher and shorter. I accept Wright's analysis as being the correct application of the function. But Wright's being right has not dispelled my scepticism about the validity of the multivariate approach. The sensitivity of the results to rather minor measurement differences does not make me more confident that this is the best way to proceed.

The nub of it is that a multivariate analysis of measurements is not an anatomical analysis, as I argued in this journal almost 20 years ago (1976). Multivariate clustering UC 101 with Australian, Melanesian and modern African nearest neighbours based on 33 measurements does not necessarily mean "close morphological resemblance" (Wright, 1992). Nothing could demonstrate this better than Wright's determination that UC 103, the most Mongoloid-like of the three, a female specimen whose Mongoloid affinities have never been questioned to the best of my memory, clustered away from the Mongoloids and even more closely with the Africans in the CRANID [his multivariate computer program (Wright 1992)] analysis.

When Thorne and I (1992) wrote the point Wright criticized, "our examinations of the Chinese specimens found no *anatomic evidence* that typically African features ever replaced those of the ancient Chinese in these regions" [italics mine], we meant exactly what we said. We said it "without [what Wright regards as] analytic support" because multivariate analyses based on living populations are not going to resolve this issue. Wright is welcome to continue using this multivariate approach to show relationships, but we are not obligated to accept their validity. CRANID is arguably better at this than the Giles-Elliot (1962) discriminant function, when used appropriately [although the results of its testing on new samples from the base populations, as has often been done for the Giles-Elliot (1962) function, are yet to be published]. But it is also not appropriate to use discriminant functions to determine the identity of specimens from populations not included in the sample the function is based on, without first independently testing whether it works (Giles, 1966), an unlikely prospect for Shandingdong or any other fossil sample whose affinities are questioned.

Functions often do not work even for modern forensic specimens (Ayers *et al.*, 1990). Fisher & Gill (1990, p. 62) had enough trouble discriminating Northwestern Plains Indians as Indians with the Giles-Elliot (1962) function, and concluded much more generally

> often metric methods [such as those developed by Howells (1973)] will prove less successful when applied to populations not included in their formulation. This is an inherent weakness of metric approaches and should be kept utmost in mind if one attempts to extend such methods beyond original samples.

If these comments are valid for modern samples, they are all the more so for Pleistocene ones.

This kind of approach, i.e. applying a forensic analysis to Pleistocene fossils, is an exercise in typology. At the first meeting of the Paleoanthropology Society, in 1992, I read a paper with David Frayer in which we used forensic analysis to show the Mladeč remains were similar to

Europeans and not Africans as the Eve theory would predict. But although it supported Multiregional Evolution, we decided not to publish it, and for just this reason. The multivariate forensic analysis of fossils rests on a typological, unchanging approach to geographic variation that we could not support. So unless one believes that Weidenreich (1938/1939), and so many that followed him, are wrong, and that there actually was a Mongoloid race living in North China 30 000 years ago that is the same race as a living one, unless one is also willing to further believe that this long-lasting race is part of the sample used by Giles (1966) or Howells (1989) (the source of Wright's data) for calculating the coefficients of the discriminant functions or other multivariate techniques, I see no reason to withdraw the contention that the multivariate analysis of UC 101 is an example of the fact that "the dependence of the results on everything from the programming package to the reference sample badly muddies the waters" (Wolpoff, 1994, p. 186).

Acknowledgements

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