

However, I am uneasy with what seems to me to be a rather hasty dismissal of the open-model camp. As Howells found with craniometrics, teeth also show a close relation between Tasmanians and some Melanesians. There are, in fact, 131 worldwide dental groups more like Tasmanians than are southern Australians (131/239 series). Four of my seven Melanesian series are more like Tasmanians than are northern Australians. Admittedly, some two dozen of these closer groups have some missing data, the Australian series has such poor provenience that it can only be grouped into north and south subsets, and the Tasmanian sample is very small. Some of the other close dental similarities with Tasmania are best viewed as erroneous or microevolutionary convergence. Still, my dental anthropology system has usually worked as expected elsewhere, even with small samples, so one cannot help but wonder why it seems to be so out of line with Pardoe's findings. If Pardoe's northern Australian divergence is set as equal to mine relative to Tasmania, then my southern Australian group is about four times more divergent from Tasmania than any of his southern series. Scaling may be the problem, and therefore it would have been helpful had he included a comparative outlier.

A West African series is included in the dental MMD matrix to provide geogenetic scale and to explore the possibility that the problem may be not with my small Tasmanian series but with our understanding of Australasian prehistory. Howells (1973*b*), Brace et al. (1989), and others have also found unexpectedly close relations between Africans and Australo-Melanesians. Some of this similarity can be dismissed as "tropical convergence," relatively high retention of "primitive" traits, and sampling error, but not all. Is it possible that we are not reading Australasian prehistory correctly and that Australians and Tasmanians do not have close biological ancestors? Could there have been a multiple early peopling of Sahulland (perhaps one wave from India and another from more northern Sundaland) with later sharing of cultural features and some genes and occupation of similar environments that led Tasmanian and Australian groups to evolve similar early tool assemblages convergently? While this notion is possible, it hardly constitutes a parsimonious scenario. At the same time, as Pardoe says, some workers find great similarities between Tasmanians and Australians and others find great differences, both of which depend on the traits studied and how they are interpreted. Might there be some middle ground?

Until such time as I can additionally sample Australian and Tasmanian teeth, dental morphology seems a better fit with the findings of Howells et al. Average worldwide dental microevolution is about 0.01 MMD/1,000 years \pm 30% (Turner 1986), suggesting that the Tasmanians and Australians have been separated about 20,000 to 30,000 years. Since this dentochronological "date" does not conform well with the assumption of only 8,000 years of oceanic isolation, perhaps the clustering results that Pardoe presents for Tasmania and Australia are due not to evolution after isolation but to

admixture in southeastern Australia before and even after higher sea level by two groups that lacked a relatively recent common ancestor. (Tasmanians and Australians had watercraft about the size of Aleut-Eskimo kayaks, which were used in some of the most dangerous waters in the world.)

Which traits provide a stronger phylogenetic signal, those of the cranium or those of the teeth? Given that Ossenberg (1989) has shown a substantial correlation between MMDs based on cranial and dental nonmetric traits, how do we resolve the difference here? Since Australian teeth are quite similar to the key dental features of Southeast Asian Sundadonty (Turner 1990) whereas crania from these two regions are very different, it is possible that teeth and skulls in this part of the world have not evolved with the strong correlation that they have in northeastern Asia and the Americas. Following Birdsell (1977) to some degree, I suggest that a third scenario be considered, namely, a dual origin for Tasmanians and Australians, with late Pleistocene and Holocene admixture in the southeast—in other words, branching that began perhaps in Sundaland or farther east. The Americas were not populated by a single migration, and the temporal window for colonization was open for a much shorter period than for Australia.

Pardoe has done Pacific Basin bioarchaeology a real service by introducing and ably demonstrating that yet another model from population genetics can be applied to the complex problems of this vast region. The model provides a powerful means for predicting expected results given certain assumptions. My chief concern is that Pardoe seems to have been a bit hasty in dismissing findings contrary to his own. I strongly suspect that he can marshal a vast body of facts to deal with this concern and readily dismiss my speculations.

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I wish to comment on one point raised in this very thoughtful paper:

We expect that as time, distance, and barriers increase, so will the difference between groups. Thus the finding of such limited differentiation in a group isolated for the longest time in recent world history must lead to a reinterpretation of the evolutionary constraints and factors responsible for variation of non-metric traits.

The multiregional-evolution hypothesis is based in part on nonmetric variation, and its authors have argued from its inception that the problem to be explained is why human populations differ at all in a geographically systematic way over long periods of evolutionary time (Wolpoff, Wu, and Thorne 1984). Most of our colleagues seem to regard isolation as the main driving force in accounting for patterns of variation and to believe that

virtually any amount of systematic gene flow would be sufficient to cause homogenization. It is this perception that leads to the idea that the broad spectrum of human variation is recent; if it were ancient, the reasoning goes, speciation would have resulted. If instead there was enough gene flow to prevent speciation, homogenization rather than patterned differences would be expected. This reasoning underlies the acceptance of the "Garden of Eden" hypothesis, in which all living populations are said to have had a single, unique, *recent* African origin.

To the contrary, the multiregional-evolution hypothesis proposes that the pattern of systematic variation in our polytypic species results from a *balance* of gene flow and local selection and/or drift and that *virtually any magnitude of gene flow would be sufficient to create this balance*. Pardoe's data give our ideas considerable support. He shows that morphological differentiation is not like motion in a Newtonian system, proceeding continually unless there is an opposing force to stop it. Instead, even with complete isolation, differentiation slows down of its own accord. Thus continued differentiation is not a necessary consequence of isolation. The other side of this coin is that differences do not seem to accumulate linearly with time, even with isolation. An attempt to estimate divergence dates from the extant pattern of nonmetric variation would provide an *underestimate*. The fact is that when the magnitudes of differences are controlled by a balance between opposing forces, the differences do not increase with time in a clockwise manner.

Reply

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Brace has succinctly pointed out two causes for uneasiness in examining human variation: size biases and sample composition. Since the distinction between measurements and non-metric observations is only methodological, our studies of variation must address evolutionary processes and historical reconstructions of human evolution. As Brace has shown and as I have tried to demonstrate, the data of biological prehistory are demanding, and incongruencies of results are a starting point for more insightful analysis.

Where Brace has emphasised variation to extrapolate historical relationships, Wolpoff addresses human evolution in a geographical context. By setting the results of this study within his multiregional-evolution hypothesis he highlights the role of gene flow in differentiation and foreshadows my own impression that gene flow is a causal process of diversification, *not* of "homogenisation." Since gene flow in human societies can be seen to be highly structured along social and geographical lines and virtually none of the variation we see is unique to a given population, diversity must arise at least in part from the structuring agency of gene flow.

Turner notes that Tasmanians appear closer to a number of other populations in the world than their nearest neighbours and uses this in his ongoing examination of modern origins and population relationships. As the results of the isolation-by-distance model show, structured relationships in excess of roughly 2,000 km are absent. I suggested that non-metric trait analysis will not show population relationships at a world level, and the fact that, in Turner's analysis, many non-Australian populations are closer to Tasmania than those in Australia might indicate the essential randomness of trait expression at that level. Positing migration models for Australia has been in vogue for many years, and I am as unwilling to accept an Indian or African origin visible in skeletal morphology as a Chinese one. It simply isn't there. Turner has already pointed out the problems he has with parsimony in his conjectures, and setting up a separate migrational homeland for Tasmanians ignores the deep-seated similarities of biology and material culture with the mainland.

My point about the length and degree of isolation was that Tasmania is one case in which gene flow with the mainland can be totally discounted for 8,000 years. Appealing to later Holocene canoeing or rafting as an agent of gene flow is simply not appropriate in Bass Strait. Complete isolation is not an assumption of an isolation-by-distance model but a question that has been examined in great detail. Bass Strait is a formidable barrier, and evidence from mammalian and avian biogeography, ethnographic observations on watercraft capabilities, and archaeological investigations all support this. Isolation is definitely not the case in Japan, where sailing was an early innovation and where distances between islands and mainland are small. Aikens, Ames, and Sanger (1986) review the extensive marine adaptations of Jōmon peoples before 5,000 B.P., which include exploitation of 17 marine fish species and dolphins and "such fishing gear as net floats of bark or pumice, girdled and notched stone net weights, and bone or antler harpoon points and leister prongs" (p. 16). They further note that "watercraft capable of quite formidable journeying were in use long before Jōmon times" (p. 16). Similarly, North America, from whatever time of settlement, has never been completely isolated from Asia.

Turner's "dento-chronology" is probably more accurate than he thinks. With a "date" of 20,000 to 30,000 years separating Tasmania and Australia, this reflects a realistic state of affairs. Colonisation of Tasmania at about 30,000 years would have immediately set up the potential for divergence. However, as Wolpoff elaborates, differentiation is in part a function of gene flow, and when that ceased across the strait differentiation slowed down. Finally, I have been at pains to show that with model-free methods differing results may arise and that these may be reconciled with model-bound methods.

Past population sizes are of fundamental importance to archaeological interpretation and reconstruction, as Plomley and Bowdler point out. The various methods devised within archaeology as well as historical observations are riddled with problems. More important, these

methods of population estimation within archaeology need to be tested against independent ones. Population genetics does not proffer the holy grail but is currently the only independent assessment available. My preliminary estimates of Tasmanian deme size are an order of magnitude greater than those Morton (1982) recorded for Australian mainland groups.

Bowdler, Plomley, and Brace have touched on an important part of the study of human remains: ownership and control by indigenous peoples. Given the power basis of "colonial" archaeology in Australia, as in North America and elsewhere, it is necessary to set out an agenda for ethical research (see Pardoe n.d.). My own research on burial archaeology and skeletal biology on the Darling and central Murray Rivers has been predicated on Aboriginal ownership of their ancestors' remains and permission from the communities to do the work, with whatever restrictions that may entail. This is no different from what a Chinese (say) archaeologist would do if he or she were to excavate a cemetery in England, no matter of what age. This is not politics; it is good manners.

Archaeology is not just stones and bones. We must be aware of our responsibilities in a wider society. Our interpretations have real effects on real people. When Brace commends a "nonracial and noninvidious assessment of human biological similarities and differences," let us extend this to a collaborative approach in archaeology that includes indigenous peoples whose ancestors we study. After all, Aboriginal people are as interested in their past as I am.

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