

colonization of Australia more than 40,000 years ago as the earliest indication of language-based skills. Recent results appear to put the event before 53,000 years^{5,6}. Theoretical and empirical evidence shows that radiocarbon substantially underestimates ages even beyond tree-ring calibrations^{7,8}. Paired radiocarbon and other radiometric ages (see figure) show a consistent trend, predicting that conventional radiocarbon estimates of 40,000 years for first colonization of Australia approach the current claim, by thermoluminescence, of 53,000 years.

Archaeology is the only means of calibrating rates of hominid evolution. There are no independent criteria for estimating the time gap between modern human morphology and modern human behaviour. We need a clear perception of what lies on either side of the gap.

IAIN DAVIDSON

Department of Archaeology and
Palaeoanthropology,

WILLIAM NOBLE

Department of Psychology,
University of New England, Armidale,
New South Wales 2351, Australia

SIR — There is a striking correspondence between 'phylogenetic' trees of human linguistics and those of human genetics¹¹. This has been used in a News and Views article¹ to support the view that both trees reflect the historical divergence of human populations. Leaving aside the many general problems associated with the Hennigian reconstruction of phylogeny¹², there is a particular problem here: the method assumes that there is no interchange between branches of the tree once they have separated, yet human populations that are very different both genetically and linguistically may exchange both genes and language components. Knowing insufficient about language to be sure, I wonder whether such exchange may not be a major reason why the so-called phylogenetic trees are so similar.

But I am quite sure that the graph in Foley's article¹ used to illustrate an aspect of the correspondence between language and genetics presents nothing more than a statistical artefact. It shows a strong correlation between the number of languages in human groups and the between-subgroup genetic diversity within those groups. But only four of the eight groups presented are statistically independent; the others are formed by successive hierarchical grouping of these (and of a ninth group for which no data are shown). Unless two groups do not differ in the variation within them, a supergroup formed by combining them will show more variation than either of them in all characteristics, so the inclusion of supergroups (and supersuper-

groups) on the same graph as groups when looking at the relationship between two sorts of variation is bound to produce an apparent correlation. In this case, if one eliminates the four supergroups from the graph, the apparent correlation is entirely eliminated.

JEREMY J. D. GREENWOOD

British Trust for Ornithology,
The Nunnery, Nunnery Place,
Thetford, Norfolk IP24 2PU, UK

FOLEY REPLIES — One of the clearest points made by Noble and Davidson in their paper² was that the presence of language should be directly rather than indirectly inferred, hence their stress on the 40,000-year datum. Their arguments relating to Australian colonization seem at variance with this position. Either Noble and Davidson are recoupling behavioural and anatomical evolution, arguing that because we know only anatomically modern humans reached Australia, and that this event was at least 53,000 years ago, therefore language must be older still; or else that communication is essential for such a journey, which is special pleading for humans given the occurrence of colonization events such as playrrhine monkeys in the New World in the Oligocene. Such special pleading is closer to the traditional approaches that they were at pains to resist.

Personally, I am perfectly happy to believe that hominids have been making both remarkable and unremarkable utterances for the past 100,000 years. Definitions of language that are not simply tautologically self-referential to humans remain a problem, but such definitional nuances do not circumvent the problem of what earlier and contemporary (Neanderthal) hominids with very high encephalization quotients were doing with their brains. My suggestion was that distinguishing language as the basis for thought from language as communication might provide a way forward that is consistent with both ethological and archaeological data. Far from bypassing the issue of proto-language, this implies that the structure of what became communicated language evolved as systems of thought that were then grafted on to animal communication systems through the evolution of speech.

Turning to Greenwood's letter, it is perhaps a truism to state that the problem in investigating the biology of human evolution is that there is only one species. This imposes major methodological problems, particularly when we attempt to extend analyses into extrasomatic patterns such as language, and Greenwood rightly draws attention to some of these in my News and Views article. In particular they arise from applying methods developed for inter-

specific analysis to within-species variation. In considering the relationship between linguistic and genetic variation there are no clear-cut clades, even at the terminal twigs of any branching model, so that even a plot of independent data points, as opposed to the hierarchically organized ones that were used, is unlikely to be truly independent. The purpose of the plot was not to read too much into the significance levels but to examine at a broad level the comparative rates of genetic and linguistic divergence. Whether there is a linear relationship in such data is a matter of probability, not inevitability, which affects the interpretation of the statistics but not the validity of the general observation. As I stated in News and Views¹, there are good historical and geographical reasons why more 'recent' clades should show greater variation. Over time these may be evened out. This effect can be seen by plotting the limited data on genetic distance and language diversity^{11,13} at higher (continental) through to lower (regional) levels; the higher the level, the greater the relationship, the lower the greater the scatter. Regrettably, of course, the higher the level the smaller the sample size and hence the lower the significance. There is, though, some evidence in microevolutionary studies¹⁴ for such a positive relationship to exist.

In the end we are left with the observation that there is some relationship between genetic and linguistic divergence. To determine whether this relationship is the product of common ancestry as I implied or subsequent population interaction, as Greenwood favours, requires more formal models that might show how these two interpretations would differ empirically. In the absence of such formal models all we have at the moment are the observations and some *ad hoc* generalizations (my own included).

ROBERT FOLEY

Department of Biological Anthropology,
University of Cambridge,
Cambridge CB2 3DZ, UK

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