

Reassessing Australia's Linguistic Prehistory

by Mark Clendon

The origin of the typological split between the Australian Pama-Nyungan and non-Pama-Nyungan languages is here described by reference to palaeogeography. In the model advanced here these currently contiguous groups are understood to have originated in widely separate regions of Sahul at a time depth about twice that of previous estimates. Australian linguistic diversity is explained in terms of climatic events at the end of the last ice age—those that brought about the evacuation of the central arid zone during it and the evacuation of the Arafuran floodplain after it. The argument advanced here crucially concerns the origin and nature of the Pama-Nyungan and non-Pama-Nyungan (Arafuran) language groups, and the implications of the model for this discussion are addressed. The Pama-Nyungan and non-Pama-Nyungan groups are now understood to represent very ancient *Sprachbünde* rather than the results of phylogenetic spreading from proto-language ancestors.

In a seminar address at the Australian National University in 1975, Michael Silverstein commented upon the striking nature of the contrast between the polysynthetic languages of the Kimberley Plateau, the Northern Territory's Top End, and certain parts of Arnhem Land, on the one hand, and the Pama-Nyungan languages to the south and east of those regions, on the other (fig. 1).¹ In typological terms, the contrast between these two language groups is indeed striking and one for which no plausible explanation has to date been advanced. Yet it is surprising that this should be the case; if the degree of typological divergence manifested in language had been as clearly evident in terms of, say, stone tool assemblages, a considerable body of Australian prehistoriography would doubtless have been predicated upon that divergence. That the significance of this typological disjunction has been largely overlooked is probably due mainly to the way in which prehistoric and linguistic investigations have progressed in Australia. Until recently, relatively few linguists have had intensive first-hand (descriptive) experience with languages from both sides of the Pama-Nyungan–non-Pama-Nyungan (Arafuran) divide, and without such experience it is not always possible to appreciate the extent and significance of that divide. And again, until relatively recently, as Renfrew (1987) has pointed out, linguists and prehistorians have had only indirect experience of each other's field.

1. Although I was not present at this seminar, Silverstein provided me with a photocopy of his handwritten notes for it in 1992.

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In what follows I will argue that a parsimonious account of Australian linguistic prehistory should include the following features: (1) a revised understanding of the origins of the Pama-Nyungan and non-Pama-Nyungan language groups on opposite sides of Sahul during an extended glacial maximum and of their subsequent approach to each other in the course of the ensuing Holocene climatic amelioration; (2) a recognition that very ancient, diffuse, and widespread language groups are likely to constitute *Sprachbünde* rather than phylogenies and that, concomitantly, relatively more recent and localized language groups are more likely to be phylogenetically related; and (3) a recognition that in a scenario involving complex stratigraphic layering over time, both phylogenetic spreading and convergence as a result of diffusion will be important (see, e.g., Dench 2001) but that over very long periods cladistic outcomes will be obscured by the processes of convergence.

Starting with Capell (1962) and O'Grady, Voegelin, and Voegelin (1966), the Pama-Nyungan and non-Pama-Nyungan language groups have been characterized as "suffixing" and "prefixing" types, respectively. Although convenient and widely used, this mnemonic is misleading: it implies a distinction on the basis of relatively trivial configurational differences. As Silverstein pointed out, the difference concerns more specifically the amount of sentence material obligatorily incorporated in or indexed on the verb: the sentence in non-Pama-Nyungan languages is typically an endocentric projection of its verb, or, put another way, non-Pama-Nyungan languages are highly polysynthetic while Pama-Nyungan languages typically display morphosyntactic coding tokens spread more evenly over sentence constituents.² These ty-

2. Dixon (2001; 2002, 48) plays down the significance of the Pama-



Figure 1. Modern Australia, showing distribution of Arafuran (non-Pama-Nyungan) languages (after Dixon 1980, 20). Dashed line indicates extent of arid zone (after Edwards and O'Connell 1995, 770).

pological distinctions are summarized in generalized form in table 1.

This essay attempts to explain the significance of the Pama-Nyungan–non-Pama-Nyungan divide by describing its origins. The description is developed by reference to epistemology, linguistic typology, and palaeogeography and has significance for our understanding of Australian prehistory.

Apart from its contribution to our understanding of European prehistory, Renfrew's (1987) *Archaeology and Language: The Puzzle of Indo-European Origins* altered the manner in which we ought to conceive of linguistic prehistory in a fundamental way. Renfrew's main contribution to historical linguistics has been to demonstrate that models of language change that depend upon the movement of either populations or languages (or both: language "spreads," in Nichols's [1992] terminology) need to be grounded in demography and rather precisely related to the specific geographic, economic, and

Nyungan–non-Pama-Nyungan divide, but his criteria for doing so are mainly lexical and phonological rather than structural. In ignoring prototype effects and the gestalt nature of prototypes, Dixon's position misses some important properties of linguistic types. A relevant discussion of area-specific clustering of grammatical features and their mutual entailment may be found in Aikhenvald and Dixon (2001, 12–13). In decrying a cladistic explanation for the divide Dixon goes on to deny the significance of the divide itself, which is not the same thing at all.

About a dozen or so semantically degraded verbs occur, typically in compounds, across both Arafuran and Pama-Nyungan languages (for examples in Gunwinyguan [Arafuran] languages see Alpher, Evans, and Harvey 2003). Although it is outside the scope of this essay, I suggest that at least part of the explanation for the pan-Australian occurrence of these high-frequency and frequently compounded verbs could lie in an abrupt-replacement scenario such as that advanced by Heath (1997).

social factors influencing demographic change. Consideration of demographic factors must now constitute a fundamental part of the methodology of historical linguistics; it is no longer feasible to ignore the social and demographic preconditions of language spread or, as Nichols (1992, 18) attempts to do, to sideline such considerations as being beyond the scope of the discipline.³

As Renfrew (1987) has shown and others (e.g., Bellwood 1991; 2001) have confirmed, the spread of large language families such as Indo-European, Austronesian, and Bantu appears to be a relatively recent phenomenon, generated initially by a complex of demographic factors including population densities at certain critical levels that were in turn generated by post-Pleistocene changes in the economic strategies adopted by some human populations (grain-based agriculture in the case of Indo-European, agriculture and distance voyaging in the case of Austronesian, and agriculture and iron metallurgy in the case of Bantu). In effect, the European historical background of invasion and migration from the fifth to the tenth centuries AD, which provides the most obvious model from which our modern understanding of population migration in historical contexts has developed, was likely to have involved factors uncharacteristic of prehistoric societies generally—quite specific political and economic factors characterizing catastrophic system collapse that cannot be randomly postulated for other historical situations. The difficulties inherent in employing invasion/migration or language-spread models to account for the origins and nature of the Pama-Nyungan group of languages are by now well known (see, e.g., Bellwood 1997, 132–34).

This essay puts forward a historical-geographical model to account for Australia's linguistic diversity and examines some of the implications of such a model.

Epistemological Background

Cultural discontinuities are the stuff of comparative anthropology. Such discontinuities cannot become information, however, without a theory or model in terms of which they may be made sense of. In pre-contact Aboriginal Australia cultural discontinuities are indeed evident and call for explanation. Warm winter clothing, canoes, carefully worked stone spear-heads, didgeridoos, Wanjurna ancestors, circumcision, subincision, patrimoieties, matrimoieties, and generational moieties (and combinations thereof) all define discontinuities in cultural space as these artefacts were or were not used in particular places by particular people. This cultural geography may be seen as an end point or outcome of certain historical events and processes the nature of which constitutes prehistory's material. Discontinuities define such a geography, which in turn testifies to a particular kind of history.

3. Nichols's allocation of the world's languages to "spread zones" and "residual zones" represents a recent attempt to conceptualize prehistory in terms of anthropologically unmotivated population movements.

Table 1. Typological Distinctions between Arafuran and Pama-Nyungan Languages (Generalized)

Parameter	Arafuran	Pama-Nyungan
Morphological type	Head-marking	Dependent-marking
Core argument coding	Prefixed to verb	Case-marked on nouns; other tokens suffixed to a range of sentence constituents
Grammatical function coding	Arrangement of verbal prefixes	Nominal case marking
Verbal compounds	Noun-incorporating	Verb-incorporating
Noun classification	Exhaustive, five-way	Sporadic, natural two-way
Agreement-class concord	Present, overarching	Absent
Grammatical pivots	Absent	Present
Switch-reference	Absent	Often present
Verbal conjugation	Absent or random	Verbs allocated by transitivity to one of two large, open paradigms (as well as smaller, closed, high-frequency paradigms)

Language is another cultural artefact, a semiotic one, and Australia's linguistic geography also displays significant discontinuities. For example, members of a group of languages around the Lake Eyre Basin have sets of common features while displaying some differences among themselves: these languages and the features that unite them are commonly referred to as Karnic (e.g., Bowern 2001 and references therein). To the north is another group of languages, again with features in common and some differences: these languages are referred to as Arandic (e.g., Koch 1997 and references therein). As it happens, the Karnic and Arandic language groups show significant differences from each other, such that the linguistic boundary between them constitutes a major cultural discontinuity. Other examples of discontinuities of this kind may be observed between the languages of the lower River Murray and those of the Adelaide and Yorke Peninsula regions, between the Yolngu languages of north-eastern Arnhem Land and the languages around them, and between the Kimberley languages and those to the south of them, among many others. Discontinuities such as these pose at once a problem and an opportunity for prehistory in that they invite explanations and potentially constitute a significant body of evidence for prehistoric demography.

There are two important ways of accounting for linguistic diversity in Australia. The most popular approach is cladistic, in which languages are compared for shared characteristics, which are assumed to indicate a common ancestry. Properly applied, comparative methodology seeks out systematic sound correspondences between languages (see, e.g., McConvell and Laughren 2004). When systematic sound correspondences are not sought, however, the resulting methodology of inspection lacks a theoretical basis: in Popper's (1963) terms, its assumptions cannot be falsified and therefore lack theoretical status. Languages possess a finite and rather small set of signs (phonemes) (around 25–30 for most languages) which in the context of historical reconstruction are operated upon by a

reasonably extensive set of diachronic phonological processes. The upshot of this situation is that, provided enough phonological processes are invoked and enough semantic latitude is allowed, it would not be an enormous exaggeration to say that almost any linguistic form can be argued to be related ultimately to almost any other form. This lack of theoretical underpinning has recently come back to haunt inspectional methodology in claims that extremely ancient and inclusive proto-languages ("Nostratic," "proto-world," etc.) may be induced by pursuing cladistic assumptions to their logical conclusion.

Cladistics by its (analytic) nature seeks to arrive at a common ancestry for any given set of languages under inspection. The method compares selected forms from each of the languages in a set and abstracts from each a form claimed to be common to all at a previous historical stage. This form, really a lowest common denominator, is then termed a proto-form, a large enough collection of which may constitute a proto-language.

If cladistics results in a "lowest-common-denominator" solution to diversity, then a second approach may be characterized as the "melting-pot" solution. In this scenario different languages come to resemble each other over time by mutual influence and diffusion (sharing) of forms rather than by diversification from a proto-language. Here similarities between languages are understood to be reflexes of a set of areal features shared by languages whose speakers have been in social contact over an extended period. Such a group of languages sharing features because of mutual influence rather than inheritance is termed a *Sprachbund*. While cladistics accounts for similarities and differences between languages by way of divergence over time from a proto-language, that is, by positing a shared inheritance, areal-feature modelling accounts for the same phenomena by way of convergence over time upon a common inventory, that is, by positing a shared history. The melting-pot solution is no more theoretically

buttressed than is cladistics; instead, notions of *Sprachbünde* or linguistic areas rely on empirical observation of modern sociolinguistic practices and the extrapolation of such observations back into history.

Of course, the approach to history pursued by most practitioners does not polarize these two models in an either-or kind of impasse; it is fairly clear that both apply in different contexts. For example, despite the caution observed above, in proper context phylogenetic models of linguistic prehistory are manifestly practical. The contexts most usually relevant are (1) when spreading from a common cultural source is independently attested by phenomena observed in other disciplines and (2) when systematic sound correspondences are observed among a number of languages.

Systematic sound correspondences occur when in some stable (morpho-) phonological environment phoneme /a/ in language A always corresponds to phoneme /b/ in language B. If this condition applies rigorously enough, then the cladistics involved may become falsifiable and so meet the requirements of theory. As Salmons (1992, 217) warns,

A proposed reconstruction or even genetic relationship can be rejected basically by showing that it fails to fit into a pattern of systematic correspondences developed by comparing apparent cognates across the relevant languages. Ignoring the need for regular correspondences thus removes mass comparison from the realm of science. As Gould (1983: 257) puts it: "Unbeatable systems are dogma, not science."

Properly constituted sets of systematic sound correspondences represent falsifiable structures in epistemology when the correspondences involved are predictable. Such structures can therefore be tested: predictions about correspondences between words can be made and checked against empirical data. Thus, for example, knowing what sound correspondences pertain between Samoan and New Zealand Maori, we can predict that the Samoan place-name Savai'i will be represented in Maori as Hawaiki, as indeed it is.

The Austronesian language family is a paradigm example of the applicability of cladistics to historical linguistics. Here spreading from a cultural source is independently attested (Bellwood 1991; 1997) and systematic sound correspondences are observable throughout the family. In Australia, however, the situation is by no means as clear. First, as Crowley (1997, 275–81) points out, sound correspondences of the sort needed to establish deep-level (ancient) phylogenies have so far not been found in Australia. Crowley's explanation for this is that not enough data have been collected (p. 278). However, in view of the number of recent researchers on Australian historical linguistics with extensive experience and undoubted expertise, this seems unlikely. It would appear more likely to be the case that deep-level sets of systematic sound correspondences have so far not been discovered because the historical situation in Australia is more complex and much older than a model of simple spreading outward from a proto-language will allow.

Prelude

It should be clear that the linguistic geography of Sahul at the end of the Pleistocene was even at that time an extremely ancient one. The current (admittedly controversial) date for the earliest evidence of humans in Sahul is ca. 60,000 BP from Arnhem Land (Mulvaney and Kamminga 1999, 139–42). On the more conservative side, we could expect the first humans to have arrived during a period of low sea level some time before 50,000 BP, possibly from Timor or from Halmahera into modern West Papua's Vogelkop region. Spreading out thence around clement coastlines and up river systems and drawn on by a naïve indigenous fauna (Flannery 1994, 187–94), it probably took only a few centuries more at most for humans to colonize the rest of Sahul. Over subsequent millennia and throughout prehistory there have probably been many entries into Sahul, occasionally adding to and enriching the continent's linguistic diversity. By this reckoning some 50,000 years of linguistic prehistory and development preceded the first phase of the model put forward here.

The Model

The model used here to account for the Pama-Nyungan–non-Pama-Nyungan divide has two historical phases.

The initial phase is located at the end of the final period of Pleistocene glaciation in the Southern Hemisphere, at ca. 13,000 BP. At this time sea levels were over 100 m lower than at present, and New Guinea, Australia, and Tasmania formed a continuous continent that I, following writers such as White and O'Connell (1982) and Taçon and Brockwell (1995), will refer to here as Sahul (fig. 2). The severity and extent of Late Pleistocene desiccation in what is now inland Australia created four geographical provinces or zones in Sahul: (1) a Papuan zone, comprising the forested slopes and uplands of modern Papua New Guinea and West Papua; (2) an Arafuran zone, encompassing the extensive southern lowland regions of modern West Papua, the more northerly reaches of the Top End of Australia, and the Arafuran Plain connecting these two areas; (3) an inland arid zone, much larger and drier than the modern arid zone and sparsely inhabited or uninhabited except for a few isolated refugia (Veth 1989); and (4) a southern and eastern or peripheral zone consisting of the east coast of Australia and the dividing range, the south-eastern corner of the continent centred on southern Victoria and Tasmania, and the south-west of Western Australia. Dodson, Fullagar, and Head (1992) have aptly characterized these regions at this time as "forested crescents."

This geography effectively divided the prehistoric population of Sahul between the northern (Papuan and Arafuran) zones, on the one hand, and the peripheral zone, on the other, with a barrier of desert or arid land between, which was likely to have been impenetrable so far as its effects on demography were concerned. The peripheral zone would have adjoined the Arafuran zone only in the region of the Cape York Pen-



Figure 2. Sahul at 18,000 b.p. Dashed line indicates extent of arid zone (after Smith 1989, 93; Dodson, Fullagar, and Head 1992, 121; Edwards and O'Connell 1995, 770; Morwood and Hobbs 1995, 748).

insula, where the dividing range ends. This would have created in effect a geographical bottleneck for the transmission of cultural artefacts between these zones.

These geographically distinct provinces gave rise to typologically distinct groups of languages. The heavily forested, tropical uplands of New Guinea contained people who spoke languages the descendents of which are now referred to collectively as Papuan languages. The open, sub-tropical savannah-woodlands of the Arafuran Plain contained people who spoke languages the descendents of which are now referred to collectively as Australian non-Pama-Nyungan languages. These languages will be referred to henceforth in this essay as Arafuran languages (see Clendon 1999, 351). As we have seen, they were characterized by a head-marking morphology or polysynthesis. They indexed core sentence arguments on the verb by prefixation, with grammatical functions signalled configurationally or morphologically, and/or by accusative/inverse marking. They were noun-incorporating in verbal compounds, employed an exhaustive system of nominal classification, and relied upon an elaborate agreement-class systematics to bear an important functional load in grammar. They were correspondingly most likely marked by an absence of switch-reference systems, primary voice oppositions, and grammatical pivots. The languages of the temperate-to-cold peripheral zone, the descendents of which are known as the Pama-Nyungan languages, were characterized by dependent-

marking morphologies, an ergative syntax, and exhaustive systems of case marking on nouns to index core sentence arguments. Morphemes attached to both verbs and nouns by suffixation, and verbs were allocated to conjugational paradigms on the basis of transitivity. They were verb-incorporating in verbal compounds, and switch-reference and grammatical pivots could be expected as coding devices.

In the model's second phase post-Pleistocene sea-level rises drowned the Arafuran Plain and created a second division in Sahul between New Guinea in the north and Australia in the south. The effect of this second division could have been either to create separate and distinct Papuan and Arafuran language groups or to reinforce typological distinctions already present. The inhabitants of the Arafuran Plain moved north into New Guinea and south into Australia, where they occupied the Top End, Arnhem Land, and Kimberley regions. Subsequently increasing rainfall reduced the significance of the arid zone for demography and created new opportunities for the northern Australian Arafuran-language-speaking groups and southern Pama-Nyungan-language-speaking groups to influence each other's development. Today the Pama-Nyungan-Arafuran distinction is still the most prominent typological feature of Australia's linguistic geography, but the divide is extremely fuzzy near the edges, as is to be expected of such an ancient division.

These phases in prehistory may be summarized in terms of the effects of shifting geographical barriers on language, namely, a desert barrier in phase 1 and a marine barrier in phase 2.

Tropical Sahul

The area now covered by the Arafura Sea, which I have been calling the "Arafuran Plain," was bounded in the north by the southern slopes of the New Guinea Highlands, in the west by the Banda Sea, in the south by the Kimberley and Arnhem Land massifs, and in the east by a large inland sea where the Gulf of Carpentaria now lies ("Lake Carpentaria," extending some 500 km north to south and 250 km east to west [see Golson 2001, 205]). New Guinea, the Arafuran Plain, and Queensland's Cape York Peninsula would have together constituted a region ("Tropical Sahul") characterized on the whole by a warm-to-hot climate, wetter in the north and seasonally wet in the south (Nix and Kalma 1972), with varying amounts of forest cover, probably grading from open to sparse with latitude and elevation. In the east, North Queensland appears to have been separated from the Arafuran Plain by the Arnhem Land plateau and Lake Carpentaria, and its languages may have had something in common at this time with the languages of eastern New Guinea.

Before about 11,000 years ago, when it was inundated by rising sea levels at the end of the last period of glaciation, the Arafuran Plain was a large and probably reasonably productive area larger than all of non-arid tropical Australia today. The northern part of the plain would have been covered by

expanses of floodplain forest and swamps, perhaps not too dissimilar from but probably drier and more open than those found today in southern West Papua and Papua New Guinea west of the Fly River, which area in effect constitutes the plain's northern extension. In keeping with the drier climate of the last ice age, the region's forests would have been more open than those found in West Papua today, and the more elevated areas would have supported an open savannah regime (see Veth, O'Connor, and Spriggs 1998). The southern part would have been drier and more open, too, with more grassland, similar, perhaps, to the modern Arnhem Land floodplain. Perhaps the floral regime could have been characterized overall as open wetland with savannah and savannah-woodland mixed with closed gallery forest around the probably numerous wetlands and waterways draining off the New Guinea Highlands (see Golson 2001, 205). Threaded with rivers and billabongs and abounding in birds and wildlife of all sorts, as well as, of course, salt-water crocodiles and mosquitoes, it would have been a productive environment for people with a hunting and gathering economy. It could be expected that in such an environment human hunting and gathering groups would have attained a reasonably high population density.

While the split between Papuan and Arafuran languages is clearly to a large extent the result of the floodplain's inundation, it is nevertheless possible that this split was a feature of the region even at the glacial maximum. Mountainous areas (such as the Caucasus and the Pyrenees) sometimes harbour languages typologically distinct from those of adjacent flat lands, and it is not inconceivable that this was the case for the Papuan highlands as well. Papuan languages are typologically very diverse, some having only a limited amount of morphology while others, such as Yimas in the Sepik region (Foley 1991), have quite complex morphologies. Nevertheless, Foley (1986, 9–12) is able to make "a number of significant generalizations . . . about their structural types," and writers such as Dunn, Reesink, and Terrill (2002) are able to list features they consider to be "typically Papuan," especially with reference to the Trans-New Guinea Phylum (pp. 29, 32). The model advanced here does not depend on more detailed consideration of these languages, however, and no more will be said about them.

The model predicts that at least some of the occupants of the Arafuran Plain would have moved north into what is now New Guinea as the inundation proceeded. This being the case, we might expect to find typological affinities between Australian Arafuran languages, on the one hand, and the Asmat and Marind languages which now occupy the vast floodplain of southern New Guinea west of the Fly River, on the other.

It is apparent and not unexpected that almost all traces of lexical ties between Australia and New Guinea have been obliterated over the 10,000 or so years since the arrival of the Arafura Sea. A few lexical similarities have been reported, but whether these are systematic enough to be considered remnants of a shared inheritance is an open question. For ex-

ample, the Asmat verb *yi-* 'say', takes on a general meaning when used in compounds, as 'do' (Foley 1986, 119–20), and the Australian Arafuran language Worora has a verb = *yi* 'do, say' with exactly the same semantics: it is a *verbum dicendi* in simplex constructions and a general classifier in verbal compounds (Clendon et al. 2000, 80):⁴

Asmat:	<i>po yi-</i>	paddle say	'paddle'
		(Foley 1986, 120)	
Worora:	<i>kari kuN [] = yi</i>	paddle say	'paddle'
		(Clendon et al. 2000, 31)	

Although this kind of comparison is intriguing at first glance, the Worora verb *kuN[] = yi* 'say, do' is probably more closely related to the north-western Australian verb *ju-/ji-* 'put, place, cause', found in verbalizing contexts in both Arafuran and Pama-Nyungan languages and only distantly, if at all, related to the Asmat form.

With reference to the Asmat languages, Voorhoeve (1980, 22) states: "As in most languages of the Central and South New Guinea Stock the morphological complexity is entirely found within the verb system. . . . verbs take prefixes as well as suffixes. Prefixes mark a variety of aspects or modes. . . . Suffixes mark aspect or mode, tense, object and subject, generally in this order." The Marind languages Boazi and Zimakani "seem to have a system of three noun classes, involving concord with pronouns, demonstratives and adjectives" (Voorhoeve 1970, 6). And again, morphological complexity in Marind appears to be located mainly in these languages' verbs.

Voorhoeve's Central and South New Guinea stock, the members of which exhibit a good deal of endocentric projection in their verbal morphology, as well as concordant noun classing, are located along West Papua's south coast opposite Australia's Top End (see map in Voorhoeve 1970, vi). This is the region of New Guinea into which the inhabitants of the northern part of the Arafuran Plain would have moved when their land was inundated. Head-marking is apparently quite common in New Guinea languages generally (Foley 1986, 95–101), and so while these observations demonstrate nothing conclusively in and of themselves, the occurrence of this linguistic type on New Guinea's south coast is nevertheless entirely compatible with the historical scenario suggested here.

Golson (2001) makes a compelling case for the existence of a cultural province in Tropical Sahul during the Pleistocene, comprising northern Australia and the lowlands of southern New Guinea. The following discussion draws closely on Golson's observations of these essentially archaeological phenomena (esp. pp. 202–5).

Edge-ground and waisted axes found in New Guinea date back to ca. 40,000 BP. Edge-ground and waisted axes are also found along North Queensland's rain-forest coast, although these are undated. The "waisting" of stone axes was carried

4. The segment /*kuN-*/ here marks the obligatory celestial-class object prefix of all *verba dicendi* in Worora.

out ethnographically in North Queensland to facilitate the attachment of wooden handles or hafts. The North Queensland axes were used to keep clearings in the rain forest open as camping places and dancing floors. In New Guinea they are also associated with clearings in the rain forest, probably to promote vegetable food production. The New Guinea and North Queensland axes also appear to have belonged to the same stylistic/typological population (Golson 2001, 196). Axes of this type from Pleistocene contexts are found in Australia on Cape York and in Arnhem Land and the Kimberley but nowhere farther south with the exception of Kangaroo Island off the South Australian coast.

Rock art styles as well appear to point to a common regional culture in Tropical Sahul during the Pleistocene. Paintings of figures in Arnhem Land dated at ca. 10,000 BP feature male figures with elaborate headdresses, pubic aprons, and bustles carrying boomerangs and barbed spears. Chaloupka (1993, 118) has pointed out close similarities in dress and decoration between these "dynamic"-style figures and the so-called Bradshaw paintings found in the Kimberley massif. This kind of attire is apparently also very similar to the way in which Asmat men of southern New Guinea dressed traditionally (Chaloupka 1993, 110). In West Papua's Vogelkop region, Chaloupka (1993, 246) reports stencils of acutely angled boomerangs ethnographically unknown in New Guinea.

Taken together with the linguistic evidence presented here and in Clendon (1999), Golson's support for the idea of a distinct Pleistocene cultural province in Tropical Sahul is compelling. In this essay I have taken the further step of associating a particular type of language with the Arafuran Plain in that province, extending north into modern West Papua and south into the Arnhem Land and Kimberley massifs. In the absence of compelling social-demographic motivation, it should be assumed that deep-level clades will not be identified in this region, although a number of contributors to Evans (2003a) have argued for regional phylogenies.

The Arid Interior

South of this area and merging with it at its southern edge was another distinct climatic region, the inland arid zone, a good deal larger than it is today. The arid zone was more extensive, more arid, and altogether more hostile to human occupation during the period of glacial maximum than it is today (see, e.g., maps in Dodson 1989, 214; Ross, Donnelly, and Wasson 1992, 99; and Dodson, Fullagar, and Head 1992, 124). Today it consists for the most part of arid shrub- and steppe lands, with a small amount of what geographers refer to as true desert. However, during the ice age there appears to have been considerably less vegetation cover, and correspondingly more of this zone would have been true desert. This was the period during which strong, cold winds blew sand into linear dune patterns which became stabilized only when post-glacial conditions allowed the growth of stabilizing vegetation. The central arid zone appears to have been largely

evacuated during the glacial maximum and was only recolonized after the onset of warmer, wetter conditions in the Holocene period. An interesting assessment of Central Australia's climate during the last glacial maximum is presented in Smith (1989, 94–95).

Throughout the period of the glacial maximum, then (ca. 28,000–15,000 BP), the arid zone constituted a more or less uninhabited barrier between the populations of Tropical Sahul and those of the peripheral zone. A similar barrier exists today between the once contiguous populations of Australia and New Guinea in the form of the Arafura Sea. Clearly, such barriers can have overwhelming effects on language geography. The arrival of the Arafura Sea brought about the divergence of the languages of the once contiguous populations of Tropical Sahul to such an extent that there is now almost no evidence for any link between Australian and Papuan languages (Dixon 1980, 234; Foley 1986, 269). Despite historical contact between people on the two sides of the Torres Strait, the languages of the two regions remained unaffected typologically. The points to be noted here are, first, that a similar barrier was in place between the populations of the peripheral and Arafuran zones during the glacial maximum, and, secondly, that there is no reason not to assume that such a barrier would have had effects comparable to those of other such geographical barriers on language geography. It is possible that the central arid zone was not evacuated in its entirety and that some (small) groups of people there may have provided some kind of link between the populations of the Arafuran and peripheral zones. By the same token, the sea is not impervious to travel across it by ocean voyaging; rather, the question is whether such contact was extensive enough to make a difference. I will claim here that it was not, any more so than was ocean voyaging between New Guinea and Australia. It is also clear that the barrier was not complete; Cape York was probably a region permitting access between Tropical Sahul and the southern peripheral zone. However, this corridor would have been drier than it is today and would probably have supported a relatively small population compared with the region to the north of it. While providing a wider corridor than that provided by the Torres Strait in more recent times, it would nevertheless have restricted the exchange of populations and cultural artefacts (such as language) between the two regions. In any case, it is not necessary to the model advanced here for the barrier to have been impervious; it was only required to impede cultural exchanges of various kinds between the regions up to a certain point.

The Dividing Range

At the height of the last glaciation ca. 28,000–15,000 years ago, south-eastern Australia appears to have been quite dramatically drier than it is today. The vegetation of the peripheral zone at this time is shown in Dodson, Fullagar, and Head (1992, 121, fig. 6.1). The dividing range was apparently forested, as was the west coast of Sahul's Tasmanian Peninsula,

running from the Coorong in South Australia down to the west coast of modern Tasmania. Shrublands and open woodlands extended for about 300 km west of the dividing range, while most of modern southern Victoria, Bass Strait, and Tasmania held semi-arid grasslands. Likewise the far south-western corner of Western Australia was forested, with the vegetation regime changing quite rapidly to heathlands, shrublands, and then desert. Apart from these biogeographic refuges, most of the rest of the continent appears to have been either arid or semi-arid. This means that the range of habitats for humans and the plant and animal species upon which their economy was based was drastically reduced and so was population density in the regions that became arid and semi-arid. In effect, the dividing range and its associated woodlands, the Tasmanian Peninsula, and the south-west corner of Western Australia would have constituted refuges for a population that had once occupied a much larger area. Minute refuges apparently dotted the interior, even at the continent's centre (Smith 1989; Veth 1989), but the super-refuges in the peripheral zone considered here appear to have been the largest and therefore the most significant for linguistic geography. Despite this, it is clear that the peripheral zone was a greatly reduced habitat compared with what was to become available to Aboriginal people in the Holocene.

Habitat reduction in the peripheral zone is likely to have resulted in population decline and almost certainly in a reduction of the number of languages being spoken. Then, when the climate became warmer and wetter after ca. 13,000 BP, the continent was reoccupied from a relatively compact demographic base consisting mainly of people from the dividing range speaking a relatively small number of languages. In this sense, then, the glacial maximum constituted a bottleneck in time through which the Pama-Nyungan languages had to pass before attaining their later diversity. This bottleneck, really an extended period of reduced diversity, may assume the appearance, in a number of respects, of a proto-language such as the putative proto-Pama-Nyungan argued for by Blake (1988), Evans (1988), O'Grady (1990), McConvell (1996), and contributors to Bowern and Koch (2004), among others.

How linguistically diverse the dividing range and the south-east of the continent were cannot now be known, as the salvage studies undertaken recently cannot expect to capture anything like the full intricacy of complete codes. But we do know, for example, that the Adelaide language⁵ was radically different from the languages of the Lower River Murray region 40 km away on the other side of the Mount Lofty Ranges. While the Adelaide language appears to have been typologically comparable to languages farther north (the Thura-Yura languages of Simpson and Hercus [2004], as well as languages in the Lake Eyre Basin and in the Western Desert [see Teichelmann and Schürmann 1840]), Yaraldi and related speech varieties spoken around the Lower River Murray were ty-

pologically and phonologically quite distinct (Meyer 1843; McDonald 2002). It is only the fact that we have two very good accounts of these languages written over 160 years ago that enables us to make these claims: in most other parts of southern and eastern Australia records are very much scantier.

Dividing-range languages of the glacial maximum may have had features in common as well: linguistic areas, constituting a taxonomic level above that of the language, are evident in many parts of Australia (see, e.g., Dixon 2001). Documented examples of linguistic areas include Arnhem Land (Heath 1978) and northern New South Wales (Crowley 1997, 281–89), where there appear to have been at least two linguistic areas comprising numbers of similar languages or speech varieties. If the dividing range at the glacial maximum contained a small enough number of linguistic areas, then it is possible that the languages of eastern Australia displayed a degree of lexical and typological similarity at that time. If this was the case, then the subsequent post-glacial Pama-Nyungan spread westward would have mimicked divergence from a proto-language; indeed, it would have involved divergence from a bottleneck, that is, from a smallish number of possibly rather similar languages rather than from a single ancestor language. Be that as it may, the differences in subsequent historical process are not as great as they first appear.

Models aimed at integrating linguistic comparative methodology and archaeology to arrive at something like an absolute chronology for the more recent phases of Australian prehistory have recently been proposed (e.g., McConvell 1996; Veth 2000). The most recent date to have been suggested for proto-Pama-Nyungan is ca. 6,000 BP (McConvell 1996). Archaeological sites in Australia give evidence of changes in material culture from about 5,000 BP onwards. These changes appear to demonstrate a more elaborate stone tool technology, an increased range of economic exploitation, and advanced art styles, among other signs, and are usually grouped together under the heading "intensification" (see, e.g., Lourandos 1997). Evans and Jones (1997, 405–13) and Layton (1997) have pointed out that the 6,000 BP date for proto-Pama-Nyungan and the 5,000 BP date for the commencement of intensification may not be merely fortuitous—that these two events may be causally connected. There has also been some discussion of whether intensification was the outcome of increasing population density or its cause. A more realistic approach might see increasing population density and intensification as being "structurally coupled" in Maturana and Varela's (1987) terminology, that is, as mutually interdependent components of a looped feedback system in ecology and demography. Contrary to suggestions put forward by McConvell (1996) and Evans and Jones (1997), proto-Pama-Nyungan is probably not an outcome of intensification; it just happens that conventionally applied comparative methodology appears to be effective only about as far back as the mid-Holocene.

The model advanced here does not require the Pama-Nyungan languages to have arisen from a single proto-lan-

5. The Adelaide language was apparently unnamed but is now referred to as Kaurana.

guage ancestor. Relaxing this conventional requirement means that the model does not need to specify precise social and demographic conditions under which a putative proto-Pama-Nyungan spread at the expense of other languages. Pama-Nyungan could be, for example, a clade or set of clades which subsequently converged as a *Sprachbund* in isolation from other language groups.⁶ The essential point is that the Pama-Nyungan group of languages, whether arising from a bottlenecked *Sprachbund* or from a clade, spread westward from their dividing-range *Urheimat* sometime after 13,000 BP.

Anthropological Context

Traditional Australian sociolinguistic practices are likely to have effected language diffusion, and consideration of these factors supports the development of a model of lexical and phonological change and retention able to accommodate time depths greater than those conventionally countenanced in comparative studies. In Australia a number of factors relevant to the discussion can be discerned. Group exogamy, systemic multilingualism, and an ideology of universal (i.e., fictive, classificatory) kinship are all powerful social institutions which produce and support extensive and widespread contacts between local groups.⁷ The dynamics of Australia's notoriously complex kinship systems are all generated by rules of exogamy. Societies are divided into exogamous moieties (patrilineal, matrilineal, or generational splits) which may in turn intersect each other to produce four-way or even eight-way splits. The requirements of exogamy not uncommonly entail marriage between people who speak different languages, and therefore bilingualism and multilingualism appear to have been the norm in traditional Australian societies. The results of these social institutions are that contacts are maintained over quite long distances between people speaking different languages and that it was not uncommon for traditional languages to contain large numbers of synonyms.

Linguistic factors both created *Sprachbünde* and were created by them. The most important of these factors was a comparable set of phonologies. Australia is remarkable for the uniformity of its phonological systems, which are usually described as conservative, that is, non-innovative. There is actually some variation across the continent; phonotactic constraints at morpheme boundaries appear to be generally less rigorous in Arafuran languages, and some languages have developed fricatives in isolation (e.g., Adnyamathanha in South Australia and Marrithiyel and Ngan'gityemerri in the Northern Territory). Innovative phonologies have also been recorded in, for example, the Arandic group in Central Australia and the languages of Cape York. Despite this, the overall

impression produced by Australian phonologies is that they have very much more in common from one end of the continent to the other than they have differences. This situation has obvious advantages in a society practicing systemic multilingualism: second-language learners need to master new lexical inventories and codes (grammars) but are not bothered by radically different articulatory goals. (Contrast this situation with the difficulties experienced by English-speakers attempting to master a set of tones when learning Vietnamese or a set of post-velaric consonants when learning Arabic.)

Necronym prohibitions and distinct mother-in-law languages enable the percolation of lexemes over wide areas. As words are removed from the language by law, new ones are taken up, often from other languages. Once adopted they are not always discarded when the original prohibition is relaxed. By this means words may diffuse over very wide areas, given enough time, and even eventually return "home," as it were, as replacements for prohibited lexemes themselves. Restricted registers such as mother-in-law languages may serve as reservoirs of archaic lexemes. Such reservoirs may yield up their forms from time to time to mundane speech contexts, thus keeping old forms alive for longer periods than might otherwise be the case. If a restricted register is shared by a number of speech varieties, then an item entry into the register at one point renders that item automatically available to users of other languages in the group. By this means lexemes may find currency more widely outside their "home" language.

Cyclic renewal with respect to prohibited lexemes is not the only kind of cyclic change operating in Aboriginal Australia. I have elsewhere (Clendon 1999, 351) illustrated non-linear or cyclic historical processes with respect to the noun-class prefix */(w)u(N)-/* in the Arafuran languages Wubuy (Nunggubuyu) and Worora. The morpheme */-n-/* as well in these languages appears to switch between marking accusative and inverse-hierarchical morphologies over time. Dixon (1997, 93; 2001, 71–83) notes other cyclic historical phenomena, most notably the ongoing derivation of bound pronominal prefixes from free pronouns and syntactic alignments in languages switching between ergative and accusative morphologies. The outcome of processes of cyclic renewal is the retention of forms over much longer periods than is usually considered possible in other contexts. In situations of cyclic renewal no one form is really "ancestral" to any other in a cladistic sense; rather, there is the potential for forms to maintain their formal and functional integrity over rather long time periods.

A common religion in Australia was crucially concerned with the sites, pathways, and monuments created by totemic ancestors and culture heroes. The practice of this religion involved widespread movement between groups taking part in ritual and ceremonial activities of all sorts. This in turn ensured contact across extensive areas and required multilingualism of its practitioners. All these social institutions facilitated the spread of linguistic features (lexicons, phonologies, morphologies) over very wide areas; in other words, social

6. For this suggestion I am grateful to an anonymous reviewer.

7. Speech communities are not themselves exogamous, despite the claim of linguistic exogamy made in Evans and McConvell's (1998, 185) model. Nevertheless, social ideals that require (extended and classificatory) kin-group exogamy obviously increase the likelihood that marriages will be contracted across language boundaries.

institutions contributed to a flattening of the linguistic geographical landscape as languages drew upon each other's resources (converged upon each other) to a greater rather than to a lesser extent.

How does it happen, outside of initial-colonization contexts, that the descendents of speakers of just one language come to occupy a given region? Perhaps the most obvious scenario involves the depopulation of an area and subsequent repopulation from a single source. Depopulation may conceivably come about as a result of drought or protracted feuding. Events of this nature drastic enough to bring about the depopulation of an area probably occurred only once every 200–300 years or so, but when they did occur their consequences may have been far-reaching.

Bowern (2001) has made a case for considering the Karnic group of languages of the Lake Eyre Basin as a phylogenetically related category. As it happens, the Lake Eyre Basin is just the kind of place that could be expected to suffer periodic cataclysmic droughts; it is one of Australia's driest and harshest regions, and it would be reasonable to suppose that the area or large parts of it were depopulated in the past on more than one occasion. If the region was subsequently repopulated by people from one particular language or language group with, say, a prior claim as landowners, then it is conceivable that a linguistic phylogeny in the area would result over time. Sutton (1990) and McConvell (2001) have invoked a cardiac analogy to characterize this kind of climate-driven population movement, consisting of what they refer to as "upstream" phases in which people move into arid or geographically tenuous regions in good times and "downstream" phases in which they retreat from those regions under climatic duress.

Feuding is ethnographically attested in Australia, usually with dispute-resolution mechanisms available. If, however, in the past for any reason these mechanisms failed to apply on any given occasion, feuding may have escalated out of control and compelled people temporarily to vacate their country. Again, subsequent repopulation of the area may have been undertaken by a subset of its original occupants, who could perhaps assert a prior claim as landowners.

Reoccupation of a previously depopulated area may have taken place from more than one source and still have resulted in a phylogenetically constituted set of speech varieties, even though the proto-language at the base of the phylogeny was itself a stabilized pidgin or creole. It is in fact not unusual for cultural artefacts to be historical compounds derived from more than one source. A clear example of this is the subsection system in Central Australia, which McConvell (1985) has shown to be a compound of section systems which operated originally in widely separate parts of northern Australia.

Zones of Convergence

The model yields some quite specific predictions with respect to particular regions in Sahul. I will survey three of these

regions and assess how they may have been affected by the glacial maximum and its aftermath.

The languages of the North Queensland–Cape York region are all unequivocally Pama-Nyungan, although the Cape York languages have undergone some quite dramatic phonological changes when compared with neighbouring languages. The entire dividing range, therefore, was probably occupied by people speaking peripheral-zone languages, although this situation may not have been as straightforward as it now appears.

The region in which contact would have been maintained between Tropical Sahul and the peripheral zone during the glacial maximum was most likely the Cape York–North Queensland area. Here we might expect to find languages displaying both Arafuran and Pama-Nyungan features of some antiquity. One of the typologically diagnostic features of the Arafuran languages is the possession of noun classes, most commonly four in number, although occasionally there is a minor fifth class containing plural or collective references (Sands 1995; Clendon 1999). Arafuran noun-class systems typically have two human (masculine and feminine) and two non-human classes. I have argued elsewhere that the underlying intensions of the non-human classes are either earth and sky (terrestrial and celestial genders) or food and anti-food (Clendon 1999, 340). If any area of Australia could have been substantially isolated from the effects of the glacial maximum and its aftermath, it was surely the rain-forest region of north-eastern Queensland. And it is just here that we find Jirrbal (Dyirbal), an unequivocally Pama-Nyungan language which, however, displays a prototypically Arafuran noun-class system with the non-human gender category partitioned along economic lines (Dixon 1982, 178–83). It is conceivable that this situation reflects very ancient influences from both Tropical Sahul and the peripheral zone up and down the dividing range or perhaps even a typology predating the glacial maximum.

The Pama-Nyungan spread westward from the dividing range after the glacial maximum may even be represented in north-eastern Arnhem Land by the enclave of Yolngu languages there. It is just possible that the people who originally spoke these language varieties moved westward into Arnhem Land as the Gulf of Carpentaria was inundated. It is of course also possible that the Yolngu language varieties represent much more recent demographic shifts. McConvell and Alpher (2002) examine linguistic cognates of kinship terms found on Cape York, in Arnhem Land, and elsewhere in north-central Australia. Their evidence supports the idea that Pama-Nyungan-speaking people colonized the northern arid zone from the dividing range in North Queensland.

The model outlined here also predicts that during the Holocene period, as speakers of Pama-Nyungan languages moved westward from the dividing range, they would have come into contact with speakers of Arafuran languages, possibly also moving south from the Kimberley and Arnhem Land escarpments. To a large extent, however, the movement of Arafuran-language-speakers southward would have been barred by the cen-

tral Australian desert, still a formidable and marginal habitat for some time after the Pleistocene as it remains today (see Gould 1969). In the areas where speakers of these two kinds of language did meet and intermingle, it is to be expected that the typological and lexical boundaries between the types would become blurred and fuzzy, and this is indeed what we find.

South of the Arnhem Land escarpment and immediately south-west of the Gulf of Carpentaria are a number of languages that appear to be typologically intermediate between the Arafuran and Pama-Nyungan groups: these include Garrrwa, Wanyi, Kayardild, Yukulta, Lardil, Yangkaal, and Yanyuwa (see map in Evans and Jones 1997, 386). Various of these languages have in the past been classified in and out of the Pama-Nyungan group (for details see Evans and Jones 1997, 394–95). Other Arafuran languages as well have come closer to the Pama-Nyungan type in this border zone: to the west is Jingulu, a language whose phylogenetic affiliation was once considered problematic. As Pensalfini (1999, 227) remarks: “With both obligatory case marking and verbal argument marking, Jingulu looks like a combination of head and dependent marking.” Again, the most parsimonious explanation for these typologically ambivalent languages is that they occur in an area where people speaking both Arafuran and Pama-Nyungan types of languages have been in close social contact for some considerable time.

Conclusion

The model proposed here contains an obvious epistemological advantage: it is parsimonious. The model is essentially a default one; it applies unless and until some other, independently motivated historical process or event can be discerned to replace it. Drastic biogeographic upheavals in Sahul during the terminal Pleistocene are independently attested from palaeontology and inevitably had a correspondingly drastic effect on demography. Demographic motivations of this sort are of just the kind required to explain modern linguistic geographical configurations. A model such as this cannot be disproven, in the short term at least, in the same way that a theory can be falsified. All such structures in epistemology do is create a framework upon which evidence can be hung and tested for goodness of fit. If the model explains enough observed facts and reconciles hitherto irreconcilable observations, then it will serve a useful function until the accumulation of fresh evidence either distorts it out of recognition or falsifies it completely. The model has been drawn up in response to my concerns about the perceived absence of a plausible social/demographic motivation for alleged language spreads in prehistoric Australia.

The model as proposed is incompatible with the historical scenario recently put forward by Evans and Jones (1997) and Evans and McConvell (1998). In this hypothesis proto-Pama-Nyungan spread outwards from the Gulf of Carpentaria about 5,000 years ago as the medium in which new ceremonies were conveyed and promoted abroad. This scenario certainly has, as its authors claim, “at least *prima facie* archaeological, an-

thropological, and sociolinguistic plausibility” (Evans and Jones 1997, 412–13). However, the hypothesis is also expensive, as it depends upon a reconstruction of prehistoric social institutions in many ways quite different from those observed ethnographically. In Australia some ethnographically observed social practices appear to be extremely ancient. In 1859 Joseph Milligan (cited in Plomley 1976, 30–31) described the custom of necronym prohibition in Tasmania in the following terms:

It was a settled custom in every tribe, upon the death of any individual, most scrupulously to abstain ever after from mentioning the name of the deceased, a rule the infraction of which they considered would be followed by some dire calamities. They therefore used great circumlocution in referring to a dead person so as to avoid pronunciation of the name.

This practice as described here is identical to that found on the Australian mainland, although Tasmania has been physically separate for some 8,000 years.⁸

In order to propel proto-Pama-Nyungan across the continent, Evans, Jones, and McConvell require the now almost universally exogamous Australian kinship system to have once been endogamous (Evans and McConvell 1998, 184). Without independent support, this is a very expensive position to maintain. The evidence of intensification referred to above also sees Australian society becoming increasingly more complex and interconnected over the course of the Holocene, but this process is likely to have been linked to increasing population density, which is manifested archaeologically, rather than to altered marriage customs, which are not.

If proto-Pama-Nyungan was originally just one language among many in a continentwide mosaic, how does it happen that it is typologically so very different from the others in the mosaic? Is it not fortuitous indeed that the typological odd-man-out just happened also to be the one associated with ceremonial innovation? If proto-Pama-Nyungan served initially as a lingua franca among ceremonial participants, would it not have been more likely to be a composite of the head-marking, polysynthetic languages around it?

Another major difficulty with that model is that it crucially involves language change as ceremonies spread from one place to another. While the authors admit multilingualism in ancient society, there is little to suggest why systemic multilingualism should have been overridden by obligatory monolingualism at that time. Today cultural artefacts (including ceremonies) spread across language boundaries without requiring language change. More than that, language is associated with country to the extent that knowledge of language is emblematic of land ownership (see Dench 2001, 109, with regard to the situation in the Pilbara region). Alan Rumsey, among others, observes

8. Necronym prohibition in Australia is therefore at least 8,000 years old, predating Stonehenge, the Pyramids, etc., and at least coeval with Pre-Proto-Indo-European, for instance.

that in Australia “languages are directly associated with tracts of country, in which they are said to have been installed during the Dreamtime” (2000, 36). Linguistic evidence tendered during the course of native title claims testifies to this. To cite just one example, Bagshaw (2000, 42–43) describes how ancestral Dreamtime *pukarrikarra* beings travelled up the Eighty Mile Beach in northern Western Australia:

[They] entered Karajarri country from the south at Pirijiti. . . . Immediately prior to arriving at this location they had spoken the Nyangumarta language. At Pirijiti they changed languages . . . and henceforth spoke Karajarri. . . . Upon later entering Yawuru, Nyikina, and Mangarla territorial domains, the same *pukarrikarra* beings also adopted and introduced each of the languages now spoken by those peoples.

Here we see a religious mandate linking land and language: such mandates are common across northern Australia, at least, and there is today a positive social expectation that visitors to a country should speak the language of their hosts if they can. This makes it all the more difficult to see why the spread of a particular ceremony 5,000 years ago should have ignored or by-passed these institutions to insist on linking ceremony to a new language. This is not to say that it could not possibly have happened, but it does add to the scenario another layer of expense.

Although the model presented here is incompatible with that of Evans, Jones, and McConvell, it is not proposed primarily to exclude or replace it. As it stands, their discussion constitutes a highly plausible, structured model for the spread of the set of currently held religious customs and practices collectively referred to as “the law.” I am not at all sure, however, that a specific language or even specific lithic technologies can be linked to it. Nevertheless, as Mao Dze-dong once said, “Let a thousand flowers bloom, let a thousand schools of thought contend.” Reality is usually far more interesting and intricate than our predictions of it can ever be. The issues under discussion are ultimately unknowable, and it would be foolish indeed to try to exclude any reasonable and plausible hypothesis. Rather, having a number of testable models available as explanatory vehicles in prehistory helps to retard our natural inclination toward the safety of dogma.

Comments

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Claims about the relatedness of languages and about the places in which they were spoken and the development of their form are matters of historical inference. “Proof” of such claims is of a different order from mathematical proof or proof of, say, the

chemical composition of something and, absent written records, cannot even attain the value of any eyewitness account. This said, there are nonetheless levels of speculativeness in historical inference, and Clendon’s scenario for the nature of Pama-Nyungan and non-Pama-Nyungan structural dissimilarities and for the external history of these languages leans heavily on speculation and neglects well-supported claims of phylogenetic relatedness.

There are good reasons to believe that the Pama-Nyungan languages are genetically related (see Alpher 2004 and Evans 2005 and references therein). There are good reasons to believe that the non-Pama-Nyungan languages or at least most of them are genetically related (Evans 2005, 273–75; 2003*b*). And there is good support for a claim that Pama-Nyungan and non-Pama-Nyungan are genetically related (Evans 2005, 276), quite possibly as part to whole. The support for any claim that either of these groups or a genetic group encompassing both or any individual language in either of them is genetically related to any New Guinea language for which records exist is much weaker.

Clendon holds that similarities among Pama-Nyungan languages and similarities among non-Pama-Nyungan languages are synthetic (diffusional). This contention is based on an undemonstrated assumption of stability of typological parameters and on conjectures for which no evidence has been published such as that “restricted registers such as mother-in-law languages may serve as reservoirs of archaic lexemes.” Concomitantly, he almost completely neglects historical-comparative evidence that strongly suggests descent from a common ancestor. His account of genetic-historical inference is a caricature, and he makes no mention of the study of diffusion as an essential part of the comparative method.

Citing Renfrew, Clendon endorses the use of nonlinguistic information in inferences about genetic relationships of languages: “In the absence of compelling social-demographic motivation, it should be assumed that deep-level clades will not be identified in this region”; phylogenetic reasoning is “manifestly practical . . . when spreading from a common cultural source is independently attested by phenomena observed in other disciplines.” This is an invitation to circularity of reasoning in discussions of the physical ancestry of populations. Clendon and Renfrew to the contrary, there is a strict obligation to avoid nonlinguistic inference in determinations of linguistic relatedness (I except here the considerations of geographic distribution that are essential in the validation of cognate sets). With a number of Australian Aboriginal languages—Limilngan (Harvey 2001) is an example—assessments of their similarities to their neighbors (the “social-demographic” context) seem to be possible on grounds that are mostly or entirely typological: it is absolutely proper to withhold judgment on linguistic phylogeny in such cases.

Clendon’s comparison of scenarios based on considerations of parsimony is, I think, idle, since the models compared are based on radically different methodologies. However, within

his model it seems that certain simplicities are purchased at the cost of neglect of some obvious questions: What happened to the people in the southwestern "crescent"? Did these people's languages evolve precisely as the Eastern ones did? Are we talking of yet another hypothetical creolization, or did these people—whose apparent descendants speak perfectly normal Pama-Nyungan languages—simply change languages (a possibility that Clendon discounts)? The Carpentaria basin contained a huge freshwater lake. Surely this supported a relatively dense population over a long shoreline, quite far west of the dividing range. If the phylogenetic picture is as I have suggested here (for Pama-Nyungan, Clendon admits this as a possibility, albeit in a back-door sort of way), it does not seem likely that the separation of populations hypothesized by Clendon obtained and led to the results observed today.

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Clendon proposes a new model for the expansion across Australia of the languages known as "Pama-Nyungan." From the starting point of fundamental typological differences between Pama-Nyungan and non-Pama-Nyungan languages, he argues that modern Pama-Nyungan is the result of population spread into mostly uninhabited areas following climate amelioration at the end of the last ice age. For Clendon, Pama-Nyungan represents a large-scale linguistic area formed after 13,000 BP. The standard view amongst most historical linguists is that Pama-Nyungan is a genetic family which spread from somewhere in the area of the Barkly Tablelands after 6,000 years ago and is linked with Holocene intensification (see Evans and Jones 1997; Evans and McConvell 1998; Bowern and Koch 2004). Clendon's model contradicts known facts about language change and requires assumptions which are implausible. Here I concentrate on the linguistic arguments.

Clendon has trivialized differences among non-Pama-Nyungan languages. They *are* mostly head-marking, but that marking is manifested in different ways. Nyulnyulan languages have prefixal subject agreement but object, oblique, and possessive enclitic (suffixal) agreement (as well as extensive case marking [Bowern 2004]); Wambaya and Nunggubuyu are both non-Pama-Nyungan but rather different in their complex predicate formation both from each other and from Nyulnyulan (see McGregor 2002). Burrara has a tense system rather unlike Worrorra's. To call all the languages head-marking (or, to use Clendon's nonsensical phrase, to state that they show a "good deal of endocentric verb projection") is to overlook fundamental differences. After all, a large number of the world's languages, from Africa to Amazonia, are head-marking.

The similarities Clendon adduces between Northern Australian languages and Papuan are highly tenuous. Asmat and Marind have complex predicates formed with a verb meaning

"do" or "say." This is unremarkable. The linguistic features he uses are shared also by Mongolian, Mohawk, Korean, and Udi, amongst others. Such features are not diagnostic of shared history, whether areal or phylogenetic. Furthermore, it is simply not the case that reconstruction represents the "lowest common denominator" among a group of languages. Laryngeals in Indo-European could hardly be said to be the "lowest common denominator" of the languages of Western Europe and South Asia, since the only subgroup to preserve any of them is Anatolian, although Greek and Sanskrit have vocalic reflexes and their effects can be seen in Germanic, Slavic, and Italic. These errors serve to illustrate a serious problem with Clendon's conception of classification/reconstruction.

Clendon's arguments against the genetic status of Pama-Nyungan are highly questionable. He makes no attempt to address the considerable evidence presented by Alpher (2004) for Proto-Pama-Nyungan and he does not consider other reasons that Pama-Nyungan classification and reconstruction have been problematic, as outlined in Bowern and Koch (2004), including (but not limited to) absence of phonologically reliable data, absence of scholars with in-depth experience in multiple languages, and reliance on lexicostatistics (see Bowern n.d.).

The alternative that similarities among Pama-Nyungan are primarily due to convergence requires us to make a number of assumptions contrary to known facts about how languages change. If we follow Clendon's model (and Clendon follows Dixon [1997, 2002] in this) we have to assume widespread borrowing of multiple features that are not normally borrowed, including suppletion. Clendon makes an argument for "cyclic change," but such change is unattested outside Australia. For Clendon to be correct, language acquisition and speech interaction in Aboriginal Australia would have to be unique in the world; this is unsupported (see Anderson 2005).

The linguistic evidence which Clendon adduces in support of his model is vague. He says that Kurna is "radically different" from the surrounding languages but does not address Simpson and Hercus's (2004) evidence for a Thura-Yura subgroup of Pama-Nyungan, which includes Kurna. Phenetic characters such as these are not useful for linguistic reconstruction. After all, French is "radically different" from Spanish in phonology, but both can still be shown to be Romance languages.

Clendon argues that Australian languages are phonologically similar because of widespread multilingualism, on the grounds that it is easier to learn languages with similar phoneme inventories than languages with radically different inventories. But in his scenario of multiple exogamous groups and widespread multilingualism, people would be learning the languages as children. Arguments about ease of learning do not apply to children; they can learn anything. If he is right, how are we to explain Kabardian (with over 60 phonemes) next to Ossetic (with 36) or the borrowing of the phonetically "difficult" clicks of Khoi-San languages by speak-

ers of “phonologically easier” Bantu languages? Yet again, Australia would have to be a special case, the only continent in the world where children are limited by adult-like ease of second-language acquisition.

Clendon’s model makes predictions about population movements which could be tested with genetic data. In fact, his model does not appear to be well correlated with the genetic evidence for Australia and New Guinea. For example, we do not appear to find an Arafuran gene pool versus a Pama-Nyungan gene pool, whereas in fact Australia seems, from the evidence of Friedlaender et al. (2005), to be genetically distinct from Papua New Guinea.

Thus, in summary, Clendon’s model cannot be considered a serious alternative to the current standard.

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Since Clendon has mainly addressed the linguistic data, I will restrict my comments to the archaeological and other kinds of evidence for Late Pleistocene social and demographic changes of relevance to the model. The likely emptying of large parts of central Australia finds some support in the large number of sites with stratigraphic breaks, depositional hiatuses, or very low discard rates during the Last Glacial Maximum (Hiscock 1988; O’Connor, Veth, and Hubbard 1993). Whether these sequences represent a hiatus or just low-density occupation is debatable, with no sign of final resolution of the issue in sight. The restriction of permanent occupation to the outer periphery of Sahul and the Arafuran Plain also seems plausible given continuous or even intensified occupation throughout the Last Glacial Maximum at several sites in the extreme north, east, south-east, and south-west of the continent. However, if distinct refugia existed in each of these pockets, then we should expect multiple recolonization events from various locations rather than simply from the eastern dividing range.

The drowning of the continental shelf probably resulted in very large land losses in north-western Australia. When this drowned area is directly superimposed on the contemporary Australian landmass, an area roughly the size of the non-Pama-Nyungan language area is noticeable. It would take only small demographic adjustments for retracting populations to avoid entering the arid zone proper, and a distribution much like that of the non-Pama-Nyungan languages today would arise, suggesting that linguistic discontinuities might be expected simply as a reflection of sea-level rise in some areas. Interestingly, it is in this north-western zone of likely greatest demographic compaction that we also see evidence for large-scale warfare depicted in the dynamic-style rock art of Arnhem Land at ca.10,000 BP (Taçon and Chippendale 1994).

The isolation of northern and southern populations finds

some support in other kinds of evidence too. Genetic studies suggest that Australian and New Guinean populations have had separate histories for quite some time, reflecting long-term isolation both before and after the Last Glacial Maximum (Kayser et al. 2001; Redd and Stoneking 1999). Preliminary analysis of Australian mtDNA also suggests that deep divisions could exist within the Australian Aboriginal population (Huoponen et al. 2001; van Holst Pellekaan et al. 1998), possibly along north-south lines. Pardoe’s (1984, 1990) study of non-metric cranial traits also found a major division between northern and southern populations. Claims for a northern rock-art province have been mirrored in claims for a pervasive southern Panaramitee style. Layton (1997, 384) proposed a model similar to Clendon’s to explain such geographic divisions: “the simplest model might conceive of two refuge areas during the last Glacial, the southeast (home of geometric art) and the north coast (home of large silhouettes).”

Clendon rightly disassociates the lithic from the linguistic evidence in his model. The different elements of the “small tool tradition” have very different distributions, and recent dating suggests that they likely arose at different times in different places (e.g., backed artefacts in eastern Australia 9,000–15,000 years ago, points in northern Australia ca. 6,000 years ago, tulas in central Australia ca. 5,000 years ago) (Hiscock 1993; Hiscock and Attenbrow 1998; Slack et al. 2004). This complex geographic and temporal distribution appears to represent local adaptations to increased risk and mobility in the mid-Holocene (Clarkson and Wallis 2003; Hiscock 1994), with individual technologies spreading to surrounding regions long after they were first developed (Clarkson 2004).

As Clendon states, this model is going to be difficult to test. Perhaps the biggest problem for reconstructing post-Last Glacial Maximum recolonization is the mismatched distribution of various kinds of evidence. Linguistic data are limited by poor knowledge of many southern Australian languages that disappeared before they could be adequately recorded. Dated prehistoric skeletal evidence is sparse for all but the south-eastern part of Australia. While genetic information is available for northern and central Australia, it is currently almost non-existent for southern Australia (except the Darling River). Furthermore, genetic analysis has so far revealed no evidence of a population expansion in Australia after the Last Glacial Maximum which might be associated with reoccupation of the interior. Occupational sequences and intensities tell us something about broad demographic patterns but little about where people came from or went during demographic reconfigurations. Stone artefacts are by and large our only geographically and temporally continuous record of cultural change, but these are notoriously difficult to interpret in terms of human movements and do not seem to support any existing linguistic models.

In short, I find Clendon’s model compelling, but the non-linguistic evidence does not offer unequivocal support at this stage. This is no reason to discount the model; rather, it should

be considered one of a number of plausible but still largely untestable contenders in the explanation of Australian linguistic anomalies. Much more comprehensive genetic and skeletal analyses, coupled with more precise modelling of the nature of arid-zone settlement, probably offer our greatest hope of reconstructing the demographic adjustments of this period. The post-glacial human story is undoubtedly a complex one, and there is much room for alternative models and new evidence at this time.

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Clendon offers us a fresh and provocative hypothesis about how the shared human prehistory of Sahul may have given rise to certain characteristics of the contemporary linguistic situation in Australia and southern New Guinea. He correctly focuses on the need to give ancient geography full weight in our reconstructions, raising the possibility that we can still discern, in the languages spoken on both sides of the Arafura Sea, typological traces of the Late Holocene geography, when a single savannah occupied areas that are now submerged. He reminds us that the major typological discrepancies between non-Pama-Nyungan and Pama-Nyungan languages in Australia need more explanation than they have been given. And he argues that we should not simply accept the standardly assumed dates of 5,000–6,000 years ago for the expansion of Pama-Nyungan. If we go back to the drawing board regarding assumed dates, one possibility is that the Pama-Nyungan family is much older than previously thought, going back to the late Pleistocene, and Clendon's model has the virtue of putting this hypothesis on the table.

Though he claims that southern New Guinea languages exhibit typological similarities to non-Pama-Nyungan, his evidence is rather weak. To begin with, the languages he says most about—Asmat, Marind, Boazi, and Zimakani—are all members of the Trans-New Guinea Phylum (TNGP) (Pawley 1998) and are likely to be relatively recent arrivals in southern New Guinea from the New Guinea Highlands. More promising candidates for Arafuran comparison would be the non-TNGP languages spoken along the southern New Guinea coast. We know little about these languages, but preliminary accounts of Kanum by Donohue (2002) portray it as a dependent-marking, suffixing language much more like Pama-Nyungan than non-Pama-Nyungan and even exhibiting some tantalizing formal resemblances such as allative *-ngke* (cf. Pama-Nyungan locative *-ngka*) and ablative *-mpa* (cf. Pama-Nyungan genitive *-mpa[n]-*). Clendon never gets onto detailed record with what the claimed typological profile of the “New Guinea Arafuran” languages is, contenting himself with suggestive remarks such as “a good deal of endocentric projection in their verbal morphology.” Exactly what languages

are we talking about? What is the exact set of grammatical traits found in each of them? Without knowing this we cannot tell whether there really are significant typological resemblances between these languages and those of northern Australia. What the model needs is a systematic comparison of a well-chosen set of typological characteristics for a sample of languages from each of the regions under discussion.

A further problem has to do with the “westward recolonization” step. Here the challenge is to account for the distribution of internal genealogical diversity within Pama-Nyungan; their homeland should be the area of greatest genealogical diversity. On Clendon's model, this should be the Australian east coast, but this is strikingly homogeneous. Elsewhere (Evans and Jones 1997) I have argued that the greatest diversity within Pama-Nyungan is around the south-western Gulf of Carpentaria—suggestively close to its nearest non-Pama-Nyungan neighbours. Other scholars have argued for higher levels of diversity in the Murray-Darling River system, but I know of no account which regards the east coast as a serious contender. To accommodate this, Clendon's model would need to modify its assumptions about where Pama-Nyungan-speakers were as they developed their distinctive typological characteristics.

To conclude: This article introduces some important and welcome new ideas into the still fledgling enterprise of trying to piece together Australia's human past by integrating linguistic, archaeological, palaeoclimatic, and other data. Clendon's arguments about the need to take a different geographical approach to the problem, from a time when Australia and New Guinea were a single landmass, about possible non-random clusterings of typological similarities between non-Pama-Nyungan and southern New Guinea languages, and about the need to account for the striking divergences between Pama-Nyungan and non-Pama-Nyungan languages all strike home. Two enigmas are better than one, since one may end up yielding the secret to the other, so, although I do not find his overall model convincing, it adds a welcome alternative to a field in which we are still a long way from having any clear picture of the almost unimaginably long human occupation of Sahul.

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Far from being overlooked by scholars, the puzzle of why Pama-Nyungan and non-Pama-Nyungan languages are so different in type has been addressed with different hypotheses, from Dixon's implausible diffusion of “prefixing” through the North (2002, 693) to the model of the Pama-Nyungan group's being a subgroup within non-Pama-Nyungan, the split having been accompanied by typological innovations (Evans 2003a; 2005, 276–77). Clendon's solution—that the two sets of lan-

guages are unconnected and arose from different geographical regions in Pleistocene Sahul—is novel in some ways (although the “Arafuran” origin of non-Pama-Nyungan was proposed speculatively in McConvell 1990). It does not explain the similarities in *form of elements* between the two and the absence of such similarities between non-Pama-Nyungan and Papuan. This, rather than typological similarities, is the crucial evidence used in diachronic linguistics. The new elements are mainly the following: (1) The Pama-Nyungan languages are said to have developed from areal convergence rather than a single proto-language, and some similar scenario is also claimed for the non-Pama-Nyungan languages. (2) Late Pleistocene/early Holocene dates for Pama-Nyungan origin are advanced as against mid-Holocene. (3) The origin of the Pama-Nyungan languages is linked to the “dividing-range” languages close to the east coast. (4) The Pama-Nyungan expansion is said to have arisen from a “bottleneck” situation.

No evidence is presented supporting the origin of Pama-Nyungan in an areal grouping, perhaps because Clendon believes (contrary to what I know of informed linguistic opinion) that this is now “understood.” In any case, if the languages converged prior to splitting, then the hypothesis of a single proto-language is still valid, and whether it resulted from convergence or not is probably untestable.

The mid-Holocene estimates for proto-Pama-Nyungan unity—4,000 BP (O’Grady 1979) and 6,000 BP (McConvell 1996)—may well be too late. Early Holocene is a reasonable guess for initial spreads, but we need to move beyond guesses. Clendon offers no way forward here. McConvell and Smith (2003) offer a different approach to chronology in the idea of “archaeolinguistic stratigraphy,” in which artefact horizons can potentially be linked to strata produced by linguistic changes.

The notion of a “dividing-range” *Urheimat* for Pama-Nyungan is worth investigating as an alternative to suggestions of a homeland around the western Gulf of Carpentaria. We should not, as Clendon suggests, “relax the requirement” that we look for a single proto-language but tighten it to garner differential evidence for the different hypotheses.

The concept of “bottleneck” may contain a key to explaining why Pama-Nyungan developed from within non-Pama-Nyungan as a typologically aberrant language expanding into the arid interior. McConvell (2004) suggests how at least one of the features of Pama-Nyungan, case marking, may have developed from pronominal enclitics.

Clendon’s treatment of the comparative method in linguistics contains serious misunderstandings, for instance, that the method is “cladistic” (study of diffusion is an essential part of it); that only regular sound changes can establish linguo-genetic classification (other criteria, such as corresponding irregular morphology, have been widely used in establishing families); that the Austronesian family is a paradigm example of well-behaved sound correspondences (there are many exceptions in some subgroups, leading Ross [1988] to propose much more constrained modifications to the “sin-

gle proto-language” principle within the comparative method); that regular sound change is absent or rare in Australia (the papers in Bower and Koch [2004], including McConvell and Laughren [2004], establish many such changes for many subgroups, and this work goes on); and that the comparative method is unfalsifiable (since he argues that the evidence for language families is lacking, this is having it both ways).

Clendon’s claim that the “difficulties” of “migration and language expansion” scenarios for Pama-Nyungan are “well-known” is mystifying, since while migration is only part of the story, this is certainly a case of “language expansion” even on my reading of his hypothesis. Clendon’s parting shot is at the scenarios of Pama-Nyungan spread outlined in Evans and Jones (1997) and Evans and McConvell (1998). The hypothesis that the language expansion was driven by ritual movements is speculative and not one which I would support without further evidence that ritual vocabulary, artefacts, etc., moved along with Pama-Nyungan languages. However, the more general model of upstream and downstream language spread correlating with less and more language shift is testable in terms of bio-genetic results (McConvell 2001; Hill 2002) and in relation to types of kinship and marriage system (McConvell and Alpher 2002). Particular arid-zone adaptations such as the seed-grinding economy could be linked to Pama-Nyungan expansions and can be dated using linguistics and archaeology together (McConvell and Smith 2003). These works rely crucially on a “linguistic stratigraphy” method which essentially flows from the comparative method. Departures from that towards extreme diffusionism will not provide us with the basis for productive research (see Evans 2005).

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How can we decide between competing proposals for the location of a prehistoric “homeland”? Clendon covers some of the types of evidence within the larger range laid out, for instance, in the introduction to McConvell and Evans (1997); there is more evidence of a linguistic kind which Clendon could discuss within his model. The further linguistic evidence is indicative rather than conclusive (as is the kind of evidence Clendon does discuss) but strikes me as less speculative.

One body of evidence is the correlation between greatest linguistic differentiation and the homeland location proposed by Sapir (1949 [1916]). Evans and Jones’s (1997, 393–94) summary of the geographical pattern of subgroups of Pama-Nyungan, which could be augmented by findings in Bower and Koch (2004), is that Sapir’s principle points to south-eastern Australia, roughly the Barkly Tableland.

Another kind of evidence is from reconstructions of geo-

graphically sensitive vocabulary (flora and fauna vocabulary and other environmental terms), which can indicate likely (and unlikely) homeland regions. So far there are few such Pama-Nyungan reconstructions (see Alpher 2004). The terminology for flora so far investigated shows next to no terms common to Pama-Nyungan (Nash 1997) and so is inconclusive. The dingo evidence—both linguistic and distributional (its absence from Tasmania)—needs to be considered.

A further potential source of linguistic evidence could be substratum effects (if localizable); such evidence cannot be expected to be detected until there are more Pama-Nyungan reconstructions.

Linguistic evidence bears more directly on the quest for the homeland of a language group, while the other kinds of evidence which Clendon canvasses are indirect. Also, the type of linguistic evidence Clendon says he adduces is from linguistic typology; but typological similarities and differences can crosscut genetic classifications, and the latter, when available, are a much sounder basis for prehistory. Generally, we want to avoid parsimony when gathering all kinds of evidence to constrain the prehistory of the split (a split more genetic than typological [see Alpher 2004]) between Pama-Nyungan and other Australian languages.

In addition, among the particular points in Clendon's model which call for discussion, I here raise two: (1) Clendon suggests that Dyirbal noun classes persist from an ancient sharing with Arafuran noun-class systems. But why is this sharing confined to Dyirbal? Dyirbal, in the rain forest of north-eastern Queensland, is in the region from which Clendon hypothesizes that Pama-Nyungan spread, so why were the noun classes not carried in this spread? (The equally plausible alternative is that Dyirbal noun classes are an independent and more recent development.) (2) Clendon's peripheral zone has an alternative region which would be expected to have been a major contributor to reoccupation of the arid zone: the south-west of Western Australia. Clendon does not discuss this, but the mild linguistic diversity in that sector of the continent does not support this. If the south-west of Western Australia was continuously occupied through the arid phase, then how has its linguistic signature been swamped by the subsequent Pama-Nyungan spread into that area?

Reply

I thank all respondents for their contributions. Some objections to the model cannot be taken seriously, such as Bower's claim that I consider a phonologically flattened landscape a requirement for second-language learning. Instead I will address some issues in linguistic geography, including the problem for the model posed by the swath of languages running down the western side of the Gulf of Carpentaria and its hinterland. First, however, I need to clarify the issue of the

significance of the relationship between the Arafuran and Papuan language groups. All that is claimed here is that the linguistic geography on the two sides of the Arafura Sea is compatible with the model presented here. The typological status of South Papuan languages in particular is not essential to the argument; it would be naïve to imagine that random population and language shifts in the ten millennia since the inundation of the Arafuran Plain would not have affected the Papuan languages. If Asmat and Marind turn out to be members of the Trans-New Guinea phylum and their speakers really are relatively recent arrivals from the Highlands, that does not necessarily mean that the languages themselves have arrived from the Highlands: languages and populations can shift independently, and genes could be expected to flow downhill from the densely populated agricultural Highlands to the thinly populated, mosquito- and crocodile-infested swamps and jungles of the southern plains. Even if the South Papuan languages are recent in that area, New Guinea contains languages that are typologically congruent with the Arafuran languages and therefore with the model advanced here.

The Pama-Nyungan languages on the western and south-western sides of the Gulf of Carpentaria south of Arnhem Land adjoin the south-eastern end of the Arafuran-language region. These "Gulf" languages show a mixture of Arafuran and Pama-Nyungan features which results from contact and mutual influence over an enormous length of time, and this accounts for both their occasional similarity to adjacent Arafuran languages and their typological diversity. North of the Gulf languages lie languages of the Arafuran Gunwinyguan language family. Alpher, Evans, and Harvey (2003) have abstracted a set of 13 proto-Gunwinyguan verb stems which show similarities in form and function to Pama-Nyungan verbs. Their list includes verbs that are typically high-frequency, frequently compounded, and frequently both semantically and phonologically degraded—verbs translatable as "hit," "get/take," and "do/say," as well as the experiencer verbs "see" and "hear," the stance verbs "sit" and "stand," and other high-frequency verbs such as "give," "spear," "cry," and "follow." Such verbs appear to be particularly susceptible to being replaced abruptly when they become so phonologically and/or semantically degraded that they lose contrastive functionality (see Heath 1997). An example of this may possibly be found in a comparison of Worora and its apparently related neighbours in the west Kimberley, Ungarinyin, Wunambal, and Gunin. The root of the Worora word meaning "do/say" is *-yi*, a form so phonologically degraded that its shape can only be obtained by abstraction; it never appears in this shape at surface level, and its forms are therefore prone to homophony. The root *-yi* is also semantically degraded, acting as a pro-verb in verbal compounds in both transitive and intransitive constructions. This verb is, in short, a good candidate for abrupt replacement, and this may be what has happened in Ungarinyin (Rumsey 1982), Wunambal (Carr 2000), and Gunin (McGregor 1993), where the equivalent

form (“do/say”) is *-ma*.¹ It is possible that either Worora has replaced an earlier **-ma* ‘do/say’ with a form *-yi* borrowed from Pama-Nyungan languages to the south which have *ju-/ji-* in this function or Ungarinyin, Wunambal, and Gunin have replaced **-yi* with *-ma*, common in both Pama-Nyungan and Arafuran languages. Both scenarios imply loss of functionality of the morphemes due for replacement.

Other diffusional processes are synchronically observable. The Western Desert language is Australia’s most widespread language, with a number of regional dialectal variants. In the north it shares sets of bound pronouns with its neighbours, while in the south bound forms are rare or absent, just as they are in neighbouring languages. Its south-eastern varieties, like the neighbouring Arandic languages, allow vowels word-initially, while elsewhere in the Western Desert word-initial vowels are illegal, as is the case in neighbouring languages. In the south-west the Western Desert language employs a lamino-dental consonant series in common with neighbouring Kartu languages, while lamino-dentals are prohibited elsewhere. These features have regional distributions; it is improbable that they were all inherited from an ancestral variety of Western Desert and subsequently diffused differentially to the Marrngu, Ngumpin-Yapa, Kartu, and Arandic language groups. Again, the most parsimonious explanation sees them as areal features taken up by the Western Desert language from its neighbours. Therefore, while it is possible that the distribution of the verbs listed by Alpher, Evans, and Harvey (2003, 348–49) bears witness to phylogenetic spreading from a proto-language near the Gulf of Carpentaria, it is at least equally plausible that it results from quite different processes.

Pama-Nyungan verbs are structured root-stem-tense/aspect/mood, with the root being the lexeme base and the stem being one or other of a lexically determined set of allomorphs that allocate the lexeme to a particular conjugation. For this reason stems are sometimes referred to as “conjugation markers.” Conjugation in turn determines other lexical features such as transitivity. Typically, Pama-Nyungan languages have a large, open conjugation of intransitive verbs, another of transitive verbs, and two or three small, closed conjugations containing a few high-frequency verbs which may also be frequently used in compounds. Alpher, Evans, and Harvey (2003) contend that some of the past and present tense suffixes in proto-Gunwinyguan gave rise to the verbal stem allomorphs of proto-Pama-Nyungan. The phonemes in question are peripheral /m/ and /ng/ from past-tense Gunwinyguan verb forms and apical /r/ and /n/ from present-tense verb forms. Sonorants generally and nasals in particular are iconic of continuous aspect, especially when placed word-finally, and there is a universal tendency for languages to use them to denote the semantics of time reference on verbs. This observation apart, Alpher, Evans, and Harvey’s proposition does not address how “tense” came to be reanalysed as a

lexical category marking transitivity status, how the four tense morphemes developed into the array of stem shapes observable in Pama-Nyungan, or how the seven lexically undifferentiated conjugations of proto-Gunwinyguan (p. 346) gave rise to the two-open-and-three-closed pattern observed in modern Pama-Nyungan languages.

Conjugational morphologies in Pama-Nyungan are not merely bits of heritable phonology; they constitute lexicalized, productive systems and as such are as amenable to borrowing as are other lexical items. The contrastive functionality of conjugation in the Western Desert language is demonstrated by the following minimal pairs, differentiated by intransitive (/ -ngul/, Ø-conjugation) and transitive (/ -rnul/, L-conjugation) tense allomorphs: *kampangu/kamparnu* ‘burned’; *yintingu/yintirnu* ‘poured’; *warlangu/warlarnu* ‘broke’; *jutingu/jutirnu* ‘dripped’. In ergative case-marking systems the transitivity status of verbs serves participant tracking functions and is commonly granted morphological cueing:

- (1) *Maya Kampangu.*
house burned (intr.)
The house burned down.
- (2) *Maya Kamparnu.*
house burned (tr.)
S/he burned the house down.

Here the shapes of the verbs shows that *maya* ‘house’ is the subject in 1 but not in 2. Conjugations in Pama-Nyungan languages constitute morpholexical systems with clear semantic and syntactic functions. Systems like these may have been shared by the languages of the dividing range and carried with those languages’ speakers into other regions during the Holocene. This proposal is at least as plausible as the notion that a set of tense markers in Gunwinyguan developed into a morpholexical coding system in proto-Pama-Nyungan.

The languages of the dividing range may not have been as homogeneous as is sometimes claimed. Aside from the fact that most of them are both extinct and poorly recorded if at all, the linguistic geography of the forested crescents may have shown significant discontinuities in some areas. Yaraldi (Ngarinyeri) on the Lower Murray is an example, and archival material on the Garnai (Kurnai) languages of East Gippsland indicates that they may also have differed markedly from other Pama-Nyungan languages generally. It is in any case misguided to imagine that Sapir’s age-area hypothesis must always apply in all situations. Eastern Anatolia is likely to have been where proto-Indo-European evolved (see, e.g., Renfrew 2001), if only because languages are still found there with the kind of phonological diversity that must have characterized proto-Indo-European, but by the second millennium BC its Indo-European languages showed only moderate diversity and today of course there are none.

Is Australia likely to have been unique in some aspects of its linguistic prehistory? It almost certainly was; no other region on earth has undergone the same kinds of developmental processes, effectively isolated from significant outside lin-

1. Worora has an identical form *-ma* ‘get/take’, never used for “do/say.”

guistic influences for 60 millennia and moulded over that time by gradually shifting climatic and ecological barriers. Harvey's (2003) argument for the priority of prefixes over free pronouns in this region is supported (in a minor way) by the fact that Worora free pronouns are transparently composed of pronominal prefixes attached to stems. It will be unsurprising if Australia is shown to have been unique in other historical processes as well.

While sceptical about inspectional approaches to historical linguistics, by insisting upon its rigorous application I have sought to promote comparative methodology rather than to underestimate it as some respondents have claimed. I have not denied the existence of clades in Australia, but I have pointed out that no deep-level clades have been demonstrated there. Some respondents have questioned my use of the phrase "lowest common denominator" with regard to the outputs of the comparative method—presumably to the word "lowest" in particular, as denoting a level of simplicity. In this metaphor, however, "lowest" refers to a degree of abstraction: proto-languages are the end-points of operations which abstract the features of observable languages. If the properties of two sets are abstracted, one set containing salient features AB and the other salient features BC, then the set of salient features abstracted from the two sets must include ABC²—that is, the abstraction must contain more features than either of the input sets. This is a logically necessary outcome of this kind of operation generally and not a virtue of comparative methodology in particular. Moreover, in some circumstances this outcome ought to be of concern to historical linguistics, as proto-languages are typically more complex phonologically than their daughters. If uniformitarian principles were to apply we would not expect this to be the case. Proto-Indo-European needs not only a set of laryngeals but also sets of ablaut grades to accommodate its array of daughter languages. As a consequence, words reconstructed for proto-Indo-European are frequently so phonologically abstract that they would have been unpronounceable (see Jasanoff 2003).

Clarkson's summary of the results of physical anthropological and archaeological findings offers encouraging support to the model. These results do not fit the model precisely, but at the level of resolution currently available to us this could not be expected.

The model contains two logically independent propositions: that the Pama-Nyungan–Arafuran divide is the result of geography and that phylogenetic spreading from proto-languages is not a requirement. If it could be demonstrated that Pama-Nyungan is a clade or that both Pama-Nyungan and Arafuran are clades, the validity of the model's primary claim would not be affected. The model would be falsified, however, if it could be demonstrated that Arafuran and Pama-Nyungan are branches of the same clade. However, in the

light of the genetic evidence referred to by Clarkson, this is looking more and more unlikely.

None of the respondents has addressed the model's main motivation—to stress the need to link historical linguistics with anthropology and prehistory. The diffusion of linguistic features is contemporaneously and empirically observable in sociolinguistics. Phylogenetic reconstruction investigates phenomena of a quite different order; here historical events are induced, events often opaque to any other investigative procedure. As a result of these differences the comparative method, when properly applied, has the potential to be a much more precise tool than diffusionist modelling, and the criteria for its falsifiability must accordingly be much more precise. Despite Alpher's claim to the contrary, historical linguistics ignores information from other disciplines at its peril. A methodology practiced in isolation is unable to evaluate its premises. If comparative methodology demonstrates a phylogeny, linguists ignore the need to explore its anthropological, historical, and demographic motivation at the risk of undermining their discipline's epistemological foundations.

—Mark Clendon

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2. Unless it can be argued that, for example, A and C are equivalent at some level; for an example of this kind of analysis see Harvey (2003).

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