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Some Notes on the Proto-Austroasiatic Words for ‘Water’

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Dempwolff’s reconstruction of *wayR ‘water’ is reviewed. The meaning ‘fresh water’ is suggested. The initial *w may have been dialectally distributed with a probably older *kw, Chomsky’s treatment of *w is formulated and its relevance to the evidence for *kw is challenged. The articulation of *R may have not been a velar spirant in all PAw dialects in view of the agreement of Bonil, Palaun, and Yopese on sibilance. In some dialects there may have been a paragogic vowel after the *R and that vowel itself may have been followed by a glottal stop. Finally, evidence for a medial *S has been found.

1. INTRODUCTION. Proto-Austroasiatic exhibits at least three different words for ‘water’: (1) *Cak, with reflexes usually associated with ‘sea water’ and thus also with ‘salt’; (2) *DaNam, usually associated with ‘drinking water’ and thus often with ‘fresh water’; and (3) one reflected by, for example, Ifugao wayl ‘water’, with cognates often associated with meanings like ‘fresh water’, reconstructed by Dempwolff as (in contemporary transcription) *wayR, the main subject of this discussion.

Dempwolff’s reconstruction of *c in *wayR was dependent on his citation of an archaic Malay oya, a form that could hardly have been the antecedent of the standard form bat (orthog. air), no doubt from an earlier *bar. Dempwolff’s oya might rather have been a form that occurred in a dialect in which *i before a final consonant broke to ia—much like the breaking that produced Minangkabau aia ‘water’ (but with the Malay retention of *a). That ia might then have become ya after a vowel. Regardless of this concern, his reconstruction is justified by the finding of Timuray wayq ‘water’.

In listing the cognates of members of West Central Papuan Network that he assigned to *wayR, Ross (1994:458 f.) included among the latter the following words: Hula we ‘river’, Motu tin ‘river, water’, Gahadi Lila, Doura, Kiri wi, Roro be, bel, East Melko fei ‘water’. The vowel e in the words of this collection (hereafter referred to as the e-words) is presumably taken to reflect the *a of the reconstruction, which was (perhaps fronted and) raised in a partial assimilation to the following *y or to what may have become a following *i by way of *i > *yi > *ye, this *i could have continued into the i of the e-words above. The same second alternative change sequence could have led to the *i that yielded the a in Malay air (usually written aic, and sometimes aie) after the regular change of *i > Malay e before a final consonant.

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Review Article

Australian Languages Reconsidered:

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Is the Australian linguistic area, because of its unique history, one in which the established methods of historical and comparative linguistics have limited appropriateness? Do neighboring languages in this situation come to share an "equilibrium level" of 50 percent basic vocabulary regardless of their degree of genetic relatedness? Is the Pama-Nyungan grouping totally without foundation and something that must be discarded if any progress is to be made in studying the nature of the linguistic situation in Australia? Are Australian scholars more biased than scholars elsewhere to criticize the work of colleagues? These and other "deliberately unorthodox" views of R. M. W. Dixon set forth in Australian Languages (Cambridge Univ. Press, 2002) are countered, while conceding that the book brings together an enormous amount of historically and typologically relevant material in one place.

1. INTRODUCTION.1 Bob Dixon concludes his recent book, Australian Languages (2002) (henceforth AL), with the following provocative claim: "The Australian linguistic area poses problems of investigation and analysis unlike those found anywhere else in the world. The established methods of historical and comparative linguistics, which can be applied so successfully elsewhere, have limited appropriateness in Australia. The special nature of the Australian situation must be acknowledged for real progress to be made in describing the nature of this linguistic situation, and for an understanding to be attained concerning the nature of interrelations between its constituent languages" (699). On this characterization, Australian languages are not just one more family that will yield a reconstructed proto-language and family tree, once enough descriptive materials have been gathered, and analyzed by enough well-trained comparativists. "The language situation in Australia is simply unlike that of Austroasiatic; or of Indo-European or Uralic or Uto-Aztecan. It is unique." (xx). Rather, its special characteristics, in Dixon's view, mark it as the ultimate result of an unimaginably long state of "equilibrium"—the likely result of around forty millennia of undisturbed and exclusive coexistence of a whole continent by people who were in regular mutual contact and were united by cultural similarities that deny any one group significantly greater status or power.

Not only would this make the Australian situation an extreme test case for the comparative method, but it would demand that the efforts of those scholars imperceptive enough to apply old methodologies in a situation whose special features render them barren and inappropriate, so that "a discipline becomes enmeshed in a cul-de-sac of its own making. One needs to make a sidestep in order than to continue to move forward" (xx).

This is, of course, not the first time that Australia has been declared a special case. In a sagacious article, the late Terry Crowley (1997:276) quotes another giant of Australian linguistics, A. Capell, who had made many similar claims half a century earlier: "the observed sound laws in Australia operate only within rather unpredictable limits. . . . There does not appear to be the Indo-European consistency of laws" (Capell 1956:83–84). As Crowley puts it, "Capell was effectively declaring Australia exempt from the comparative method, making it a continent [that] was somehow off limits to principles of language change and reconstruction that applied in all other parts of the world. Those, like Capell and Dixon, who wish to treat one part of the world as a special case, face the challenge of giving a coherent account without using the standard methods that they claim not to be appropriate. To do this, they need to invent new theoretical frameworks—in Dixon's case, his theory of "punctuated equilibrium", developed largely to deal with the difficulties posed by the Australian situation. At the same time, they need to justify the "special case" status they are pleading for, by demonstrating that the established methods fail rigorous and competent attempts at application. But this special pleading brings severe risks. As Alpher (1990:152) puts it: "Any supposition that for the Aboriginal languages of Australia are somehow special in regard to regularity of sound change or frequency of borrowing would appear . . . to work against a sound heuristic procedure—in fact, to open the way for confusion."

In his preface (xx), Dixon laments what he perceives as a lack of robust debate on these topics: "It is a convention in academic society today (and perhaps slightly more in Australia than anywhere else in the world) that one should hesitate to criticise the work of colleagues." This review answers his call to join battle, taking issue with many of the book's arguments and, most centrally, with the issue just outlined. In most respects, I will argue for a diametrically opposing view: that a renewed interest in applying the comparative method by a number of Australianist scholars is actually succeeding in giving a more coherent account than that argued for in AL.

Inevitably, this means I will "accentuate the negative"—it would be demeaning the high stakes laid down by the author for his major claims if I were to fail to engage with them. As a result, in this review I will say disproportionately little about the many useful and worthy parts of AL. To get an idea of how good these can be, the reader should sample such sections as those on the consequences that Australian languages' lack of interest in voicing, and in their relation to the many types of negation (83–86) and on the organization of affixed subject and object pronouns in double-agreement systems (370–401 and 437–47). Dixon's enthusiasm throws up many interesting surprises, even for the purist, such as his nice quote from the pioneer linguist

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and doctor W. E. Smythe on the multipurpose interrogative / indefinite pronoun in Gambyangpirir (328), or his discussion (663) of how the distinctive system of tenses found in the Munggpira languages (non-Pama-Nyungan) has diffused into an adjoining Yolngu language, Djirang. And the over thirty detailed maps provide an invaluable distillation of huge amounts of specific information about linguistic features and their distribution across the continent.

The book’s first and last pages define its main focus as being historical and comparative issues, with the concomitant goal of developing alternative models to better account for the Australian data, and show why they are needed—and the outer chapters directly address these issues. Chapter 1 discusses the “language situation in Australia” and chapter 2 “Modelling the language situation” (understood here as modelling the forces driving the differentiation and convergence of language varieties), while chapter 13 focuses on particular “genetic subgroups and linguistic areas” and chapter 14 closes the book with a “summary and conclusion” whose two sections give a clear idea of its intended diachronic focus: “outline of development” (section 14.1) and “dispersal patterns and cyclical changes” (section 14.2).

The overall organization of most of the inner chapters is on typological rather than comparative principles. Basically the book proceeds by taking various grammatical and phonological topics, a chapter or two at a time, and summarizing in great detail their patterning and distribution across the continent. Thus chapter 3 gives a structural overview of Australian languages, chapter 4 looks at “Vocabulary” chapter 5 at “Case and other nominal suffixes,” chapter 6 at “Verbs,” chapter 7 at “Pronouns,” chapter 8 at “Bound Pronouns,” chapter 9 at “Articulations and fusions,” chapter 10 at “Genetic nouns, classifiers, genders and noun classes,” chapter 11 at “Frequentative/accusative morphological and syntactic profiles,” and chapter 12 at “Phonology.”

As is inevitable in a book that attempts to cover more than two hundred languages, the emphasis reflects the author’s personal focus of interest and in this sense does not always mirror topics that have seen enormous activity among researchers in the field. Perhaps as a result, it conveys a certain lack of comprehensibility about the issues, a feeling that all of the parameters are in place, and that future descriptive linguists will find only minor variations on old themes. For example, AL omits discussion of many issues in the syntax of Australian languages that have claimed center stage in the syntactic literature in recent years. Warlpiri syntax, at the center of so much discussion about the nature of nonconfigurationality (see Hale, 1982; Lafreniere, and Simpson 1995 for key references), receives just a single mention (88) in connection with its generalized subordinate clause structures. Nor, among many other comparable omissions, is there any mention of other important work on nonconfigurationality, such as that by Austin and Bresnan (1995).

Though much more could be said about these matters, in this review I will concentrate on those aspects of the book that are relevant to comparativists and those whose apprises for the “Australia as a special case” view were whetted by the widely cited but programmatic claims in Dixon’s earlier (1997) book. The rise and fall of languages.

To do so with the thoroughness it deserves I will have to go into a level of nitpicky detail about Australian languages that some readers may wish to skip. So it is

2. See page 346 in section 2. for an explanation of the number sign or crosshatch (#) as used here.

2. Likewise, the book ignores some major changes in the analysis of case that have taken place among most Australians since LoA, maintaining the practice of setting up a “split case” system based on surface forms (nonconfigurative/accusative on pronouns vs., ergative/absolutive on nouns), rather than following the more recent practice, more in line with the logic used to establish case in other parts of the world, of using an integrated “bipartite” analysis into three cases (ergative, nominative, accusative) such that all words in a NP agree in case. See Blake (2001:24-26) for a recent overview and #Meadk (1979) and Goddard (1982) for the original arguments.

Given the bipartite analysis—which is simply a return to the traditional logic for determining the number of cases in a paradigm in Indo-European languages (see Blake 2001:24-26)—has been widely adopted by Australianists writing grammars of languages with such systems in the last two decades, the failure to adopt it (or even mention it) in AL is puzzling at odds with other aspects of Dixon’s recent work, such as his goal of promoting a “Basic Linguistic Theory” that consistently aims to draw together and codify a unified body of analytic practice across the descriptive traditions employed for different parts of the world.
with Barry Blake, of the five volumes of the *Handbook of Australian languages* (henceforth *HAL*), a sixth volume is still to appear, brought many fine grammars to the linguistic public, and his teaching at the Australian National University in the 1970s and 1980s attracted dreams of young linguists into the field (including this reviewer). The phenomenon of this book *The language of Australia* (*LOA*), sometimes affectionately known as the "green bible," then succeeded in setting the agenda for work on Australian languages by an ever-growing number of linguists in language description, typology, historical linguistics, and many other fields. But, as he states in the introduction to his new book (xxvii), *LOA* was in some ways premature: in 1960 there were still fewer than a dozen really good grammars of Australian languages, no more than a couple of reasonable dictionaries, and the language map of Australia was still covered with huge blanks when it came to most descriptive and historical questions.

The work under review here is conceived as an updating of *LOA* in the light of the enormous body of work that has been unleashed in the two decades that followed—in large part due to Dixon's foundational efforts—with the added perspective that comes from his work in the Amazon through the 1990s. Though longer than *LOA* (734 as against 547 pages), it is not parallel in structure. No attempt has been made to repeat or update the first four chapters of *LOA*, which were more aimed at a general readership interested in such questions as the relation of language to social units, special speech styles, Aboriginal English, and bilingual education. Dixon (xxvii) refers readers interested in such questions back to *LOA* (now, unfortunately, out of print), with the odd statement that they have dated very little: in fact a great deal has appeared since (see, e.g., Walsh and Yallop 1993, *SSASBA* 1996, *Langxen* 2000).

In compensation, there is a greater focus on reconstructive and historical issues, and a lexicography of the book, as already indicated, is its detailed discussion of Dixon's "punctuated equilibrium" model as a response to what he regarded as the unsuitability of the comparative method to the Australian situation. The book aims to flesh out the empirical basis for this rejection with more extensive data than could be provided in his shorter and widely publicized 1997 book *The rise and fall of languages*.

Even at more than 700 pages, though, and even with the excision of the more anthropologically and sociolinguistically oriented sections, *AL* seems truncated. The author plans a companion volume, *Australian languages: a complete catalogue, described (xxix) as "in preparation (but still some way from complete)," and many parts of *AL* feel as if the job of giving evidence (e.g., for subgrouping) and of providing full scholarly referencing for its assertions has been delegated to this unavailable other half. Throughout this review I shall adopt the practice of placing a "∗" before papers I cite that had appeared before 2000 but are not referenced in *AL*. This omission of references is a shame, both because the book would have benefited from drawing more on the community of Australianist scholars, and because many of the revisions Dixon proposes to the classification of Australian languages appear to come out of nowhere—more on this below. The lack of referencing makes it virtually impossible for the nonspecialist reader to check on the specific claims made in the book, assess the grounds for them, or to pick up on the sorts of errors that inevitably occur in a book of this size and scope. It is therefore imperative that the companion volume appear as soon as possible.

There are far too many of these to list them all, but I will take one language, Dalabon, as a yardstick for the accuracy of claims made in the book about individual languages. In its indexed references to Dalabon in the book I include the following incorrect or dubious claims, some minor, others systemic: (a) that its applicative prefix is *ma-ri* (677): I am aware of no recording of this form by any scholar who has worked on Dalabon; (b) work by Frances Morlin and myself we have recorded it as *ma-ru*—or more (see Evans, Morlin, and Yallop 2004), with the (b) (found in a by some speakers; Capell 1964) writes it as *ma-ru* using a different orthographic representation for the high mid-vowel, and ignoring the metathesis on the ə, earlier work by Alkire wrote the same vowel 'orthographically as u (sometimes a) but still recognized its status as a high, high central vowel; the surrounding discussion in *AL*, imaging the form i to "assistination of a i to be before ni and then loss of final ni" makes it clear that this is not simply a type, leaving a word form explained from any source against which it can be checked; (b) that its reflexive-reciprocal form is *weu* (325): in fact, it is *weu* (Evans and Morlin 2003: 270), though in this case there is at least an old source (Capell tran. 66) giving a form with which Capell writes as *weu* (c) that there is no reflexive suffix *gu* (165), a form that is crucial to Dixon's claim that Dalabon is a prefixing language lacking the *-gu*—auxiliary alignment of the prefixative and locative (see section 5); but retaining locative-*gu*: no source is given for this claim; in fact, Capell (1964:119) mentions such a form, though all his examples give it attached to the interrogative he writes as *ma-ri* "where"; no other investigators have published on Dalabon mention this form—indeed the locative is "*a*" (where *a* represents a glottal stop in Dalabon orthography) and the word for "where" is just *ma*; it is likely that Capell mistakenly pasted the following second person prefix *di*- which may be historically related to the preceding word is (d) that, in its basic pronoun system, the form appears is a prefix, but the object is a predicate; the analysis of the object pronoun is a controversial point (b) (in this case) I agree with Dixon's characterization: it was analyzed as a free pronoun by Capell (1964:190), as a prefix by Alkire (1983), and as a pronominal by Evans, Brown, and Corbett (2001:151); no reference is made to any of these sources here; (e) strange claims are also made about the genetic status of Dalabon: I discount these in 9.1 of the main text.;
3. CLASSIFICATION. The section of the book that differs the most from existing scholarship is the part concerned with classification. This contains many radical departures—sometimes overtly justified, but often not—from established classifications such as those by O'Grady, Wurm, and Hale (1966) and Walsh (1981). Accepted scholarly names for subgroups are replaced with an idiosyncratic alphanumeric code, and even the spelling of language names often creates grammatical neologistic forms; as a result even the experienced Australianist reader needs to keep referring back to the language coding scheme on xxxviii-xxxlii to see which languages are being talked about. Though some of these innovations are justified attempts to brush away old cobwebs, others—such as the reclassification of Arrernte Land languages to be discussed below—seem ad hoc and unfounded. The net effect, I would predict, is that scholars will continue to rely on the preexisting complete classifications, rather than employing the highly idiosyncratic classification presented in AL. Imperfect as they are, they at least have the merit of internal consistency, and can be patched up where relevant by more modest studies of particular subgroups.

Pages xxviii-xxxl present a map, plus a complete classification of all Australian languages except those of Tasmania. A letter-based classification system coherently recognizes the distinction between the dozen large-scale groupings of non-Pama-Nyungan languages, represented by binomials of the form NX, e.g., NC for the “Mindil” group. What most other scholars refer to as the “Pama-Nyungan” family (on which more in section 5) is represented by letters A-WM, though confusingly those starting just with N (without a preceding capital letter, e.g., NC “Central inland New Guinean”) are what has been considered Pama-Nyungan, and the A-WM scales includes, as group X, the Garrwa family that is often considered non-Pama-Nyungan and is most likely the nearest relative of Pama-Nyungan. Though sources on individual languages are referred to, there are no attempts to harmonize the various schemes. Neither are there any attempts to present an overall classification of all Australian languages, even though this is what scholars would like to see. Instead, a series of maps and charts are presented, each with a different classification scheme. The net result is a confusing array of possibilities, none of which are completely satisfactory.

5. In 1966 O'Grady published two (essentially identical) versions of his classification, each time with two colleagues: one, with Carl and Florence Vogelins, as a special fascicale of Anthropological Linguistics (this is referenced in AL), and a second, with Kim Hale and Stephen Wurm, as a wall map. The latter is referenced in AL. Since the two are essentially identical in their classification, I will in places use the phrase “the O'Grady et al. classification” to refer to both.

6. To be sure, the spelling of language names is always going to be problematic, because they are regularly tagged in two directions by the competing orthographic principles of English and the vernacular orthography. The general sociolinguistic trend by indigenous communities has not been to follow standardized orthographic principles, but rather to use distinct orthographies in the same language—distinguishing that one is used on the same road sign where differences were served traditionally, so that in many cases (Arrente, Ilbilbil, Gun-wok) three or more orthographies are used across the various dialects, despite a greater tendency to use them in a single dialect. Nonetheless, the unnecessary introduction of new spellings in AL adds to the orthographic confusion rather than diminishing it, e.g., the spelling Nyangumara (xxxviii) for Nyangumara, or the phonemically incorrect spellings Mawang and Sawerja (see footnote 11 on page 214). To avoid undue confusion among readers who know many of the languages being discussed, in general I will keep to the versions used by the major published primary sources on the language in this review; sometimes this will necessitate using two versions where I quote directly from AL or other sources.

7. Indeed, on p. xxxvi “avid readers” are recommended to photocopy the master map ... and perhaps also the list of languages and language groups—and to keep these on the side when studying the volume.” Had more standard terminology been used for subgroups, this stratagem would not have been necessary, at least for Australianist readers.

8. Dixon gives the rationale for this as “to enable the reader to see that not only one ingroup ... runs along the division between groups NA-NL, on the one hand, and the remaining groups (labelled A-Y and WA-WM), on the other hand.

9. Ironically, the O’Grady et al. classification is criticized on p. 46 for precisely this reason: “[A]ll that was published was the classification. The data on which it was based were not specified, nor were the cognate densities between languages.” In fact, this criticism is hardly warranted. O’Grady and Edmiston (1966) described the methodology used in the O’Grady et al. classification, give a source for word list for eight languages (and cognate densities with Wirinda), and cite (1969:299, 9.2) three publications providing phonotactics of cognate densities; O’Grady (1966) for 27 West Australian languages, Hare (1966) for 13 southern Puman languages, and Hale (1962) for 20 Araric languages; only the first of these last three are acknowledged in AL. In addition, O’Grady’s typescript of Cape’s well-known “Vocabulary of 500 words in 40 north Australian languages’” has been available in the Mitchell Library (Z64599 [CY2353]), and since 1953 in digital form in the ASIADE archive. (I am grateful to David Nash for bringing these sources to my attention.)
that is spoken to the west of Arnhem Land). Interesting as they are, Green’s proposals are still programmatic and do not go beyond a few superficial categories on the verb.

The preceding paragraph summarizes the state of play for the languages of Arnhem Land. When the dust settles, we now have around nine families plus Yolngu, with the main current arguments concerning the composition of Gunwinyguan, and in particular whether it includes Manggarai, Anindilyakwa, and Wrenda/Wangman.

Now let us return to the classification in A.L. Surprisingly, given the author’s general skepticism of high-level groupings, he sets up a single “Arnhem Land group” for all of the above languages except Wurrumiyanga and Yolngu. Though, as mentioned above, preliminary proposals for this move have recently been published by Rebecca Green (2003). AL neither justifies nor references the reasons for this highly adventurous lancing, which is about comparable to Austric in terms of the number of proven cognate morphemes involved. More disturbing though, are the many unjustified splittings and lumpings that have been carried out within this new “Arnhem Land group.” Murray is split into two, Alawa going into a new isolate branch (“NBb”), and Mara and Wurdangari into a different branch (“NBB”), against the clear views expressed by all linguists who have worked on Murray languages that they form a clear group,11 and without any arguments for this new classification being given. On a comparison of 152 basic vocabulary items, these two languages share 41 percent of their vocabulary, but have less than 8 percent shared vocabulary with any of the following other languages of the region (Wurdangari was not included in the comparison): Mawg 7.6 percent with Ngandi, 6.7 percent with Gun-djém, 4.1 percent with Dalabon, 4.7 percent with Rërhmarr, and 2 percent with Mawg, while Alawa has 7.5 percent with Gun-djém, 6.4 percent with Ngandi, 3.7 percent with Rërhmarr, 3.0 percent with Dalabon, and 1.5 percent with Mawg.

Languages that all previous published classifications have placed within Gunwinyguan are now scattered across five branches of the new “Ambient” grouping:

- NBb, containing Rembangara and Ngalakian, which are certainly more closely related to each other than to anything else (Baker 2004); and
- NBB, containing Ngandi, Nunggubuyu, and Anindilyakwa (Anindilyakwa). As mentioned above, no solid evidence has yet been adduced to confirm the gut feeling many linguists have, on the basis of structural parallelisms, that Anindilyakwa is

10. Dixon introduces the term “North-west Arnhem Land subgroup” to replace the term “Iwaidja,” used by scholars since the 1960s (see Evans 2000), but without change to the internal content of the group. There is a judicious discussion of the evidence for and against this particular grouping on 65-68. O’Daly, he also introduces the unwarmedly xenologi language Bongga and Marung for the two languages of this group, once conventionally spelled Iwaidja and Marung since work by Capell in the 1960s and 1970s. Certainly neither of these latter spellings is satisfactory from a phonemic point of view, but the new spellings in AL do not resolve these problems, and in fact create new inaccuracies, because phonetically the first is *foolar* and the second *manayi*. A practical orthographic spelling, Mawg, is now being used by that speech community, and there are current discussions under way about using *Iwaidja* when writing that language’s name in its own orthography.

11. The term “North-west Arnhem Land subgroup” is the term used by Dixon in his 1972 work *Australian languages reconsidered* to replace the term “Iwaidja,” and was subsequently adopted by scholars. However, it is now clear that this term is not accurate, as contemporary research has shown that the languages in this subgroup are not closely related. The term “North-west Arnhem Land subgroup” is used by Dixon in his work to refer to the languages that are now known to be a part of the Gunwinyguan language family.
related to any other language, and AL itself does not have any discussion of the grounds for this grouping in its 20 mentions of Anindilyakwa.

• NB, containing only Dalabon, despite the far-reaching similarities between it and Bininj Gun-wok (Kunwinjku); it is more than this below.

• NBg, containing Bininj Gun-wok (Kunwinjku) and Gunbarlang. The grouping of these two together goes back to O'Grady et al., though a close consideration of verbal morphology (prefixal prefixes and TAM suffixes) reveals them to be far more distinct from each other than generally thought.

• NBh, the "Jawoyn-Warny" group. The insight that Warny belongs within Gunwinjku, and specifically with Jawoyn, is due to Harvey (2003b), though this is not referenced here. Grouping Jawoyn and Warny together at subgroup level is uncontroversial, but the substantial evidence for placing Jawoyn and Warny together within Gunwinjku (see Harvey 2003b) is ignored.

These four groupings are, according to ALs system of numbering, regarded as no closer to one another than they are to the other eight groupings within the "Amerind branch," a manifestly strange claim. To give some idea of how counter-intuitive this is, consider the following translations, in (1a-c), of the sentence 'the man hit the woman' in Dalabon (Nbe), Bininj Gun-wok (NBg), and Nara (NBh). I also include parallel verbal words in Dalabon and Bininj Gun-wok to show the extent of similarity (in both structure and form) between these latter languages, differing (in this example) only in the presence of an additional prefix marking assertiveness in Dalabon (which in any case has a cognate form in BGW, where it marks 'immediate aspect').

(a) Mara
Na-kiyi-mar n-g-agil 44 53 n-qari-b-mar, 3MAS-ORI-MAN-NPL 3MSK3Q3Q-3Q-TKE 9PE-3QDEM 9PE-3QDEM-WOMEN-NPL.
'The man hit the woman.' (Heath 1981b:81)

(b) Dalabon
Biy-yibik haka-bb-o-ng kahnun kiridkird, mar(ERG) 3MSK3Q3QHUM-ORI-3Q-MGR-3Q-SPP EEM WOMEN.
'The man hit the woman.'

(c) Bininj Gun-wok
Bilinji hi-bb-o-n ngal-eke daluk, MAN 3MSK3Q3QHUM-ORI-3Q-SPP EEM WOMEN.
'The man hit the woman.'

14. A common terminological problem facing Australianists is what name to use for dialect chains; in this case the term Bininj Gun-wok has recently been coined as a solution; Kunwinjku, properly speaking, refers to just one dialect of this chain; see Evans (2003c).

15. Though Harvey's paper appeared a year after AL, it had been widely circulated in draft form for a number of years.

16. Word order is free in BGW and Dalabon but has already been put in the order of the Mara example and its English translation to facilitate comparison. BGW and Dalabon examples are constructed on the basis of my own work on these languages; see Evans (2003c) for a grammar of BGW and Evans, Morling, and Tulkomba (2004) for a dictionary of Dalabon.
ing of Anindilyakwa mentioned above, and the failure to group Gurungi more closely to Burarra than to the other Mainingrida languages. We are left—and without any justification for the reasons—with a misleading and idiosyncratic classification.

3.2 The Languages of the Torres Strait ("AL") The Torres Strait separates Australia from New Guinea and contains what is normally regarded as the only "non-Australian" language spoken within Australia's political borders: Merym, spoken on the island of Mer (Murray Island) in the eastern part of the Strait, which is a Papuan language of the poorly known Trans-Fly family. On the western islands are various dialects (Kala Lagaw Ya, Kala Kawar) of a language that has traditionally been regarded as Australian (this is the view given in LoA) and, for many authors since O'Grady et al. (1966), as belonging to the Pama-Nyungan family. Its speakers, however, are culturally and physically very different from peoples on the Australian mainland, and the language itself has significant "un-Australian" features such as /s/ and /t/ phonemes, as well as other signs of mutual convergence with Papuan languages that we discuss below.

The AL classification, however, takes a strange further step, including both Merym and the Western Torres Strait languages together under "A. Torres Strait Group" (see AL:125-30 for discussion). In the classificatory terminology of AL (see p. xxv), "group" is used "simply for languages grouped together on a geographical basis," that is, without any implication of genetic relatedness. However, as p. xxx it is stated, of Merym and the Western Torres Strait language (Dixon's "A"), "that these are Papuan languages, not closely related to each other. AL has a significant Australian substratum."

An initial idea of the "Australoasian" of the Western Torres Strait languages compared to Merym can be gained from table 1, which compares the pronoun system of Western Torres Strait (Kala Lagaw Ya dialect) with that of Merym and of another Trans-Fly language, Gizen (data for the latter two from Piper 2001). Alongside, I give forms that have been proposed for Proto-Pama-Nyungan by Blake (1988) and Alpher (2004).

The resemblance of form between the Merym and Gizen pronouns is striking. On the other hand, many of the KLY forms have straightforward Pama-Nyungan cognates. Most obvious is the way of the pronoun ngay, and even more propositive in the proportion ngay: ngari: nangoa between the nominative, ergative, and accusative forms. Once we allow for the descent of "ngari" in Western Torres Strait, this clearly mirrors that reconstructed for Proto-Pama-Nyungan between nom ngay, erg ngari, and acc nangoa (cf. Dyer 1981, 1992, and Stock, which shows the latter two forms clearly).

Note further 2PL nigel as a cognate of NHampa+hula; the lack of a laminar nasal in Western Torres Strait (and consequent descent of NH as n) is a good candidate for Papuan influence. Another two clearly Pama-Nyungan pronouns are 1PL paku and 2PL nthi.

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20. This is not to say that the genetic relationship of BGW and Dalabon is completely straightforward, because Dalabon has been influenced by some forms with BGW and in others with Ramarumba, which belongs to a different group within Gwionyan. However, this point I am making is that, whatever their exact internal position, both clearly belong within the Gwionyan family.

21. It is currently unclear whether p-nil had an inclusive/exclusive contrast, and if so whether this included both dual and plural forms, and there are still unsolved problems with the initial nasal contrast that I avoid here by using the zeroomission representation NH to represent n-nil and TH for c-nil. See Alpher (2004) for a more sophisticated discussion of this problem.

22. I thank Barry Alpher for pointing out this particular parallelism.

Table 1: Free Pronouns in Western Torres Strait (Kala Lagaw Ya Dialect) and Merym, Compared With Gizen (Papuan) and Proto-Pama-Nyungan

<table>
<thead>
<tr>
<th></th>
<th>Proto-Pama-Nyungan</th>
<th>Kala Lagaw Ya</th>
<th>Merym</th>
<th>Gizen</th>
</tr>
</thead>
<tbody>
<tr>
<td>1SG</td>
<td>ngay</td>
<td>ngay (cbl. ngau)</td>
<td>ngay (ngg)</td>
<td>ngay</td>
</tr>
<tr>
<td>1PL EXCL</td>
<td>ngari</td>
<td>ngaug</td>
<td>ngaug</td>
<td>ngaug</td>
</tr>
<tr>
<td>1PL INCL</td>
<td>ngara</td>
<td>ngaug</td>
<td>ngaug</td>
<td>ngaug</td>
</tr>
<tr>
<td>2SG</td>
<td>nangoa (Alpher 2004)</td>
<td>ni (ngg, acc. nin)</td>
<td>ni</td>
<td>nangoa</td>
</tr>
<tr>
<td>2DU</td>
<td>NHampa+hula</td>
<td>nigel</td>
<td>nthi</td>
<td>nthi</td>
</tr>
<tr>
<td>2PL</td>
<td>NHampa+hula</td>
<td>nthi</td>
<td>nthi</td>
<td>nthi</td>
</tr>
<tr>
<td>3MASC SG</td>
<td>NHampa+hula</td>
<td>nthi</td>
<td>nthi</td>
<td>nthi</td>
</tr>
<tr>
<td>3F SG</td>
<td>NHampa+hula</td>
<td>nthi</td>
<td>nthi</td>
<td>nthi</td>
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<td>nthi</td>
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</tr>
</tbody>
</table>
Papuan language that it is supposed to be related to. The whole area is ripe for a proper typological study of convergence, complemented by an interdisciplinary approach that would begin to give clearer dates for the recolonization of the Torres Strait Islands from north and south.

3.3 OTHER ISSUES IN CLASSIFICATION. Fortunately, not all aspects of the classification are as strange as the two cases discussed above. Many other groupings are reliable, either incorporating recent advances (as is the case with Mindi, Ngirna, and Tngakic) or following long-established classifications: this is true for all the Kimberley families (Dixon’s ND, NE, NT, and NG) though in the last three he introduces new nomenclature: Fitzroy River Subgroup instead of Nunbinyali, South Kimberley Subgroup instead of Banuran, and North Kimberley Areal Group instead of Wurmran.

There is a further overall problem with the classification, however, relating to its lack of any explicit proportionality in terms of degrees of embedding, at whatever level one considers. I shall term this “phylogenetic scale distortion.”

First consider two examples at the highest level: Yolngu vs. Anhemb, and Tiwi vs. Umphithama.

First, Y (Yolngu subgroup), a group of languages about as closely related as Romance, is treated as equivalent, in terms of taxonomic depth, to NB (Anhemb), which hasn’t yet even been shown to comprise related languages and which, as mentioned above, is about as internally diverse as Austic.

In the case of Yolngu and Anhemb we are comparing two groups with complex internal structure. But we also encounter problems with languages treated as isolates at this highest level; we will exemplify this with Tiwi (NL) and Umphithama (UM). Tiwi has long been recognized as difficult to relate to other Australian languages: spoken on Bathurst and Melville islands, it is likely to have been isolated from the Australian mainland for around 8,000 years. In LoA, it was explicitly mentioned as one of the languages that cannot be shown to be related to other languages of the continent, a view also held by other scholars who disagree with Dixon on many other points of classification (e.g., O’Grady 1979). I am not aware of any Australianist scholar who would argue with the decision to maintain its isolate status. Umphithama, however, is quite another case. In the O’Grady et al. classification it was placed in a highly embedded position: within Lamanic, within Pama-Nyungan. Rigby (1997), the foremost scholar of the Lamanic languages, suggests that the similarities Umphithama shares with the Lamanic languages may reflect convergence rather than inheritance, but certainly does not suggest it should be treated as an isolate. Most recently, Verstraete (2005) has pointed out striking similarities in the pronoun paradigms of Umphithama and Ayapathu (Ayahdalu), a language located in AL as Bcf, within the Wil subgroup (Bc) of the North Cape York subgroup (B). Once one allows for the fact that Umphithama has dropped initial consonants, and that Ayapathu appears

Interestingly, the other language he mentions, Jingale (Djingale), has since been brought into the fold through work by Hanks, Chippendale, L. Green, and Nielling, and is now considered to be a member of the Eastern Mindi group, within the larger Mindi group, a reclassification reflected in AL, which classifies it as NCb.


25. Ian Green (pers. comm.) points out that one could always retort that initial dropping itself could be a diffused areal feature, which it may well be. But my point is that without assuming some form of the comparative method it is not clear how one is justified in impose an earlier stage kulu to the Umphithama data, unless “I” here means “is borrowed and in the process undergoing the following sound change.”


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28. This is why we are seeing the spread of loanwords such as “yes” or “no” from other languages, which can affect the classification of Australian languages.
other hand Nah, Dixon’s “Kayardild/Yukulta subgroup” contains languages that are mutually intelligible despite their different grammatical structures, had earlier been described as dialects of a single language, have cognate forms for the majority of grammatical morphemes, and share around 80 percent of basic vocabulary, rising to 92 percent for verbs (Evans 1995:11).

The fact that groupings ostensibly at the same level of taxonomic embedding can differ so dramatically in their amount of similarity—in this case, 80 percent (Southern Daly) common vocabulary, and no-brainer grammatical relatedness (Kayardild/Yukulta) vs. the need to overcome reader incredulity (Southern Daly) makes it impossible to use the AL classification for any purpose that requires that proportionate degrees of relatedness be preserved in the scheme, for example, for purposes of typological sampling. It also masks the strikingly nonhomogeneous distribution of deep-level diversity across the continent, because the fifty or so primary groupings in AL vary so greatly in their degree of internal homogeneity.

4. THE MYTH OF THE 50 PERCENT EQUILIBRIUM LEVEL. A possible motive for Dixon to disregard shared-vocabulary figures in his classification is his oft-voiced view that, over time, neighboring languages come to share an “equilibrium level” of 50 percent basic vocabulary, regardless of their degree of genetic relatedness, thanks to a combination of factors that includes word taboo following death, and borrowing from neighboring languages to replace the tabooed word. This model, first published in Dixon (1972:331–36), has been repeated in a number of his subsequent publications (e.g., LRA:256, Dixon 1995:76–77, Dixon 2001:86) and gets another run here as well, in essentially unchanged form (AL:27–29).

Curiously, this claim, though widely cited, has never been based on clear published evidence that goes beyond the geographically circumscribed group of languages in the Cairns rainforest region which, pace Dixon, are all relatively closely related by continental standards. In the original discussion the closest reasonable case is the 45 percent shared between Warunggurl (Maric subgroup) and Gurrinyiyguy (in the Herbert River group that includes Dyirbal). But even though this figure has been bolstered by borrowing. Maric and the Herbert River group are not particularly distant genetically; at the very least they are both Pama–Nyungan (according to those scholars who accept it) and may additionally share closer ancestry. Warunggurl, which shares 60 percent with its neighbor Gurrinyiyguy, was given as an example of shared vocabulary “at the upper end of the equilibrium range” but in fact the two languages are closely related, and are now placed in the same “Herbert River Group” in AL:200.83

If we look at some figures from other parts of the continent, where we have better evidence for the genetic distinctness of the languages involved, the figures we find between neighboring languages drop dramatically. Consider the boundary between languages of the Nyulanyul family and its Pama–Nyungan neighbors to the south, tabulated in Stokes and McGregor (2003), where “only two pairs involving a [Nyulanyul] and another language share 30 percent or more core vocabulary” (Stokes and McGregor 2003:36), namely Nyunumalt and Karajarri (30 percent) and Yawarra and Karajarri (37 percent) (35). Here are some further examples:

- In the Budly Tablelands, Wanyi (from the Garraw family) shares only 21 percent with its neighbor Wambaya, despite sharing many grammatical morphemes suggestive of either long-term contact or even shared group membership, and with Wolayua, a Pama–Nyungan language adjoining it to the south, it shares only 12 percent (Breen 2003:430).
- Muninj-Pata and Ngnj’gymere both have all available evidence always been neighbors. Ian Green (2003), as mentioned above, has recently shown them to be related on the basis of scores of common irregularities in their complex verb paradigms, but they share only eight percent of basic vocabulary, and in fact Muninj-Pata doesn’t share more than 10 percent with any of its neighbors.
- In the Torres Strait, Mabuqan (a dialect of the Western Torres Strait language) and Memyar Mi (a Pama language with relatives like Grazy in the Flax River region) share only 11 percent of vocabulary, despite substantial mutual influence in grammatical structure, such that the AL classification assigns them to a single “group” (cf. 5.2 above).
- Maru (Marar) and Yanyuwa (Wuwa branch of Pama–Nyungan), according to Hade’s calculations, share only two percent of basic vocabulary (Alpher and Nash 1999:27), though Blake (penn. comm.) calculates 11 percent.

The only case known to me where neighboring languages that are not closely related genetically approach anywhere near the 50 percent figure is the pairing Nganandi (Gunwinyguan) and Ritharrang (Yolngu), which have particularly close social relationships and an assaymmetry in size in precociously times (around 60–70 Nganandi but around 200 Ritharrang). This pair is described by Heath (1979) as sharing “nearly 50 percent”26; it shows that it is possible to get this high, but that normally special sociolinguistic circumstances are involved. Even here, though, the figure is likely to be lower: Heath did not, as far as I know, publish his figures, and a recent recount I performed on the same 152 word list mentioned above yielded the somewhat lower figure of 41 percent.27

The 50 percent equilibrium model has been refused in an important paper by Alpher and Nash (1999), whose careful study shows that (a) the effects of word taboo in Australia are far less significant than had previously been thought, owing to the frequent “return” of tabooed words and that (b) the equilibrium percentage is half of the proportion of lexical replacement due to borrowing, with empirical evidence suggesting that the equilibrium level between adjoining languages is unlikely to exceed 25 percent (and even this lower level requires us to assume that borrowings can never be identified, for example). While not denying the effects of borrowing, they conclude

26. On p. 46 of AL, Dixon states that “between the ‘Nyulanyul phylic family’ [my NTL] and the ‘Maric subgroup of the South-West group of the Pama–Nyungan phyllic family’ [YD] there is a c. 40 per cent cognate density.” It is not stated where these figures come from, and they seem to be significantly inflated: the figures in Stokes and McGregor (2003:34) range from p. 10 to 37 percent, with a median of 11 percent.
27. AL accepts the reclassification of Yanyuwa into Ngarr (his WM) but not the reclassification of Ngarr into Pama–Nyungan.
28. Harvey (1997) states they “share 50 per cent,” but this appears to be simply an inflationary restatement of Heath’s original claim.
29. Though not published until 1999, this paper was originally presented at a meeting of the Australian Linguistics Society at Alice Springs in 1984, so its findings have been known to Australianists for some time.
that lexicostatistics remains a "useful and useful instrument" in studies of Australian languages (48). See also #Black (1997) for similar arguments; his paper demonstrates that in Australia core vocabulary is, in fact, less prone to borrowing than other domains.

Though AL briefly mentions Alpher and Nash's paper on p. 30, the author's response is simply to reassert his own views: "A high proportion of long-term contact situations do show 40-60 per cent shared vocabulary, that is, the empirical facts support the model given above." However, he does not actually adduce any new examples illustrating the "40-60 percent" case, let alone consider enough cases to demonstrate that these do in fact constitute "a high proportion." The failure to give any new supporting evidence is striking given the panoply of facts and studies from across the continent that are marshalled in this book.

Lest I be misunderstood here, I certainly do not want to claim that borrowing is insignificant in the Australian context. Rather, I would stress that

(a) the extent of borrowing between neighboring languages varies significantly with the region and with the nature of the relationships between them, with high levels of borrowing being a special case (as with Ngandi-Ritharrangu), rather than the norm; both sociopolitical factors (particularly the closeness of relations between any pair of groups) and linguistic factors (the phonological proximity of the two languages, favoring or disfavoring the ready adoption of icons) are likely to play a role.30

(b) methodologically, the assertion of higher-than-average levels of borrowing needs to be based on cases where there is clear evidence from the grammatical morphology that the languages aren't closely related—ideally, between languages of demonstrably different subgroups or families. It is not enough to simply assert that languages sharing close to 90 per cent vocabulary are "a small linguistic area," because it may well be true that they are in fact a genetic group. This is the problem with several of the "small linguistic areas" considered in the final chapter of AL:

i. Arraric, where Arrernte and Kaytej share about 44 percent vocabulary; as against rates between 2 and 24 per cent with their other neighbors. Though Dixon states on p. 672 that "the two languages cannot be shown to constitute a low-level subgroup," Koch (2004) provides a detailed and convincing reconstruction that confirms the O'Grady et al. classification of Arraric as a subgroup. The fact that Arrernte and Kaytej share about 44 percent is thus largely due to inheritance rather than borrowing.

ii. the "North Kimberley small linguistic area," where shared lexical similarities between neighbors run to 40-60 percent, has again elsewhere been considered to be a family (Worrornan). Dixon mentions some of the grammatical similarities between them (672) but claims the evidence is insufficient to consider them an innovation-defined subgroup. Since the appearance of AL, McGregor and Russey (2003) have presented convincing evidence for the relatedness of the languages of this group, so here again the percentage of shared vocabulary is at least in large part due to inheritance.

iii. Kalkatunga and Yalanngga share about 43 percent core vocabulary and 10 per cent of their verbs. Again, conventional classifications treat these as members of one subgroup, whereas Dixon argues in AL (679) that the evidence for subgrouping is insufficient, and that "few grammatical forms are similar." Among the distinctive

30. This point is due to Peter Sutton (pers. comm.).

shared similarities of these two languages, however, we can cite the following (drawn from Riaco and Been forthcoming): third person possessor suffix (Y-yangai; K-anji - anji - unji), the 'other' suffix (yarrar in both languages), shared irregular inflections in the verbs 'to see' (present *nanyi in both, past Y yanga, K nayyi, future yangi in both) and 'to give' (present Y ngayni, K anji; past Y yanga, K anya - unja; future Y ngangai, K anji), the construction of nominalized verb inflections: agent-noun (Y-yarji; K-nyirri - idjirre dative applicative (Y-nyama; K nyama - jama), participial (Y-rama - yama; K -rmir - fira), and the 'hence' suffix (-nha in both). These are also regular correspondences between the two, with Kalkatunga showing a number of innovations such as loss of a number of consonants between identical vowels, the weakening of i to u also between vowels, and the raising of t to i when flanked by a laminal consonant with a in the following syllable. Though it is always possible to claim that all the above morphological similarities are due to borrowings, the levels of shared patterning—particularly in the paradigms for nominalizations and the irregular verbs cited above—certainly do not rule out the interpretation that the similarities in these languages are simply due to shared inheritance;

(c) the levels of borrowing one finds in Australia are in general no higher than in such familiar cases as French-English, so that it is unclear why Australia needs to be treated as a "special case"; borrowing poses no greater problems to lexicostatistic classifications, or to the comparative method, than does Indo-European or Austronesian, for example.31

It seems time to reassess the 50 percent equilibrium claim, and downgrade its status from dogma to apocrypha. The heuristic and cautious reinterpretation of lexicostatistics—obviously in conjunction with the other tools that the comparative method provides, as well as other little-used quantitative methods such as factor analysis32—can help overcome some of the problems of "phylogenetic scale distortion" that are so damaging to the AL classification.

5. THE PAMA-NYUNGAN DEBATE. No other issue in comparative Australian linguistics has been as hotly disputed as the question of whether there exists a proveable high-level subgroup along the lines of 'Pama-Nyungan'. The Pama-Nyungan grouping that Hale came to recognize in the early 1960s and which first appeared in print, as far as I know, in O'Grady, Wurm, and Hale (1966),33 spans seven-eighths of the continent. It is named after the words for 'person' in the northeastern (pama) and southwestern (nyungan) extremes of the continent.

An idea of just how polarized the Pama-Nyungan issue is among Australianist scholars can be obtained by comparing the following quotes:

31. I would add the caveat that the similarity of Australian phonological systems makes borrowing harder to detect on phonological grounds. But many studies have been able to overcome this through careful reasoning from sound changes, and in any case other lines of argument remain, e.g., from the presence of morphemes etymologizable in one language but not another. See Nash (1997) for a nice example.

32. For a rare application to Australian languages, see Zoé (1976).

33. Though Wurm (1965:156f) reports Hale's proposing the names 'Pama-Nyungan'.
The "Pama-Nyungan" idea is a pervasive one, used both by Australianists and by linguists at large. However, it is totally without foundation and must be discarded if any progress is to be made in studying the nature of the linguistic situation in Australia. [AL xx]

For decade after decade, Dixon (1970a, 1980, 1991, 2001, 2003) has persisted in the same wrong-headed assessment of the phylogenetic status of the large Pama-Nyungan group of Australian Aboriginal languages. His claim, which is extravagant and spectacularly erroneous, is that it has no genetic significance in the wider Australian linguistic context. Moreover he denies that the Comparative Method can be applied to Australian languages.

This approach is so bizarrely faulty, and such an insult to the eminently successful practitioners of Comparative Method linguistics in Australia, that it positively demands a decisive riposte. (O'Grady and Hale 2004:69)

Before continuing, we note that work by Blake (1988) and Evans (1988) has led to some revisions of the classification, as it affects languages in the southwestern Gulf of Carpentaria. These two publications remove Tangkic from Pama-Nyungan and treat it as a separate non-Pama-Nyungan group, and in exchange reclassify Yanyuwa, originally treated as a non–Pama-Nyungan isolate, into Pama-Nyungan, and more specifically into the subgroup known as Warlovaric (Dixon uses the term Ngarna for this subgroup). The two languages of the Garwan family have also been the subject of some debate, and have borderline status, but will here be regarded as the non–Pama-Nyungan group most closely related to Pama-Nyungan (see Evans and Jones 1997). In what follows, "Pama-Nyungan" should be taken to designate this revised grouping. Note that there are two important discontinuous blocks in addition to its main continental distribution, and that both adjoin the western half of the Gulf of Carpentaria: the Yolngu languages of Northeast Arnhem Land, and Yanyuwa along the southwestern Carpentaria coast. Note also that scholars who accept Pama-Nyungan as a construct assume that the Western Torres Strait language is a Pama-Nyungan language (see 3.3 above).

When one attempts to integrate the findings of historical linguistics with other disciplines concerned with the deep past, such as archaeology and genetics, the proposed Pama-Nyungan grouping poses many explanatory challenges, such as how to explain massive language expansion among small-scale, egalitarian socis groups, when and how this expansion occurred, and whether there is any corroborating evidence in the archaeological record or in the genetics of modern populations. Though some ideas have been advanced (Evans and Jones 1997, Evans and McConville 1998), these certainly do not constitute anything like a well-worked-out integrated scenario, and in this sense Dixon is correct in saying that we still lack any definitive answer to these questions: "no plausible cause presents itself for a wide-scale punctuation of the sort needed" (53). But the same criticism may be made, in equal measure, of the diffusion-based scenario advocated in AL, which requires us to scale up by a huge factor from the well-studied local linguistic areas to an immense zone occupying most of the continent.

34. So consistently does Dixon follow through the process of discarding the idea that the term "Pama-Nyungan" does not even appear in the index at the back of the book, though the primary section discussing it (434-54), entitled "The "Pama-Nyungan" idea," does appear in the table of contents. See also xvii, 21, 276-83.

35. Sands (1996), in a monograph-length discussion of this problem, proposes a number of changes to this scenario, heather and extended by Dixon in AL. Although some of her points are valid—in particular, her evidence for including an alien loan in the earliest alternation set at the level of Pama-Nyungan—many of her other arguments are not persuasive. For example, her proposal that *-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-ku/*-k

36. Dative *-ku, unlike the others, has a significant number of non-Pama-Nyungan reflexes.
The free pronoun ngaid was first proposed as a diagnostic of Pama-Nyungan—and therefore as "a quite modern innovation on the Australian linguistic scene"—by O'Grady (1981a). As Dixon (2001:277) conceives, it "is found in the great majority of languages from groups A-Y, WA-WM [i.e., the languages considered Pama-Nyungan by many scholars, plus Garnawa and Wayi, their nearest relatives, though there as an exclusive rather than an inclusive pronoun—N.E.] and in none at all from NA-NL."  

### MAP 1. MAJOR AUSTRALIAN LANGUAGE FAMILIES

![Map of Australian Language Families](image)

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31. This reflects a difference in methodological emphasis between Dixon and Alpher: Dixon's (1978) reconstruction posited combinations of invariant affixes with invariable conjugation markers (thus requiring the assumption of some irregular sound changes to get from the purported combinations to attested modern forms), whereas Alpher's (1990) reconstruction works back from inflected words, trying to maximize regularity of sound change, at the cost of producing a less tidy propositional that is not completely segmentable into conjugation marker plus TAM istence.

32. The problem of how to adjudicate on unfamiliar semantic change—on what O'Grady (1979:129) called "the tangled web of Pama-Nyungan diachronic semantics"—has given rise to a literature of its own, approaching plausibility from the angle of continent-wide typologies of lexical polysemy that has a range of semantic systems including special speech registers and sign language: see particularly Wilkins (1996) and Evans and Wilkins (2001).

33. We should mention, though, that Kayardild and Yukulta, in the non-Pama-Nyungan group Taakic, have a form ngaid for the first person plural exclusive (and therefore different from ngaid in both number and exclusivity). Comparison with its Larri equivalent naidi, however (which has pluralized = plural cognate further affixed, such as Dalabon naaidi), suggests the Kayardild/Yukulta form is innovative, with initial e- analogized from other first person bilabials such as ngaidi T; I therefore follow Dixon here in not considering it a cognate.
The shading on map 2 shows the distribution of this etymon, which can be seen to align very clearly with the Pama-Nyungan / non-Pama-Nyungan boundary, except for the presence of cognates of *ngali (though with first exclusive meaning) in Gurrwa/Wanyi, the family most closely related to Pama-Nyungan.49

Examining the evidence, Dixon mentions two possible hypotheses: the Pama-Nyungan hypothesis, which would see it as an inherited form from Proto-Pama-Nyungan (retained in most descendants but lost in a few), and the 'diffusional hypothesis', that it 'has simply diffused over a continuous area'. Its presence in noncontiguous Yolngu is acknowledged as a problem for the diffusional hypothesis, to be resolved by the assumption that 'it did form part of this diffusion zone at some time in the past'. Yanyuwa, not mentioned in AL, constitutes a similarly problematic case, though here the discontinuity is likely to be more recent. In favor of the diffusional hypothesis, Dixon points out that the

MAP 2. DISTRIBUTION OF 1ST PERSON INCLUSIVE DUAL PRONOUN
*NGALI AND ITS REFLEXES (ADAPTED FROM DIXON 2002:278)

49. The corresponding map in AL (278) shows the distribution of PN languages lacking the etymon on this mode of presentation, while maps can have two interpretations: (a) they are non-PN (and, in fact, a *ngali reflex is absent) or (b) they are PN, and a clear *ngali reflex is present. In my version of this map shading indicates presence only. I have included here from his type (i) (containing map pronouns with ngal- plus an increment) but not the other types of absence.
version, on the basis of Alpher's (2006) recent arguments that ergative -gsu in Yolngu demonstratives is cognate, with the simplification of -gaju to -gsu being regular. Though Blake (1988) took this to imply a Pama-Nyungan-level innovation, Dixon (1984) rejects this on the grounds that, because it is "attested for more than a third of the putative Pama-Nyungan languages," "[t]here is no justification for taking the innovation of -gsu as an ergative marker" to be a defining feature for all languages in a high-level genetic subgroup." Here, again, the argument turns on assumptions about how widespread attestation needs to be before the reconstruction of an ancestral innovation is warranted. But because, in the application of the comparative method elsewhere in the world, the independent occurrence of a form in two or three daughter groups is normally deemed sufficient to attribute it to the common ancestor, I do not see this as a strong objection; cf. "Meillet's principle," a.k.a the three-witness principle, "that apparently cognate words must be present in at least three different languages before the words can serve as the basis for a proposed reconstruction." (Trask 2000:209). If we followed similar principles for Indo-European to those being invoked in AL, we would not be licensed to attribute the lexeme ngsu 'king' to the proto-language, because it is only attested in Italic (Latin regn), Celtic (Germanic nith is borrowed from Celtic), and Indo-Iranian (Sanskrit rṣaṃ-) (cf. Benveniste 1965:307). Nor would we be licensed to reconstruct hieboveler consonants for Proto-Indo-European, because they are found only in Anatolian (Hittite hett 'what'), Latin quidque, and Germanic (English what, queen). Reflexes of -nggə occur in around sixteen distinct Pama-Nyungan subgroups, according to the present lower-level subgrouping, and even according to an updated version of the O'Grady et al. classification, which would put several of the western subgroups together under the Nyungar branch, it is still attested in around ten distinct subgroups, in five separate geographical blocks. On any interpretation, this clearly satisfies the three-witness principle, so that to reject it on the grounds that it occurs in less than a third of the putative Pama-Nyungan family is to set the bar of evidence way higher than it is set for applications of the comparative method elsewhere.41

We now add in three more pronoun forms: 2PL ጥሱ maneu in map 4 (AL map 7:3, p. 256), 2SU ወስቴ maneu/N in map 5 (AL map 7.4, p. 270), and 1PL excl ngase in map 6 (AL map 7.6, p. 275). I include the forms where it has shifted its semantics to 1PL excl. In each case we get essentially the same effect: it is found over most parts of the Pama-Nyungan area, taking in both Pacific and Indian Ocean coasts, the discontinuous Yanyuwa in each case, and discontinuous Yolngu in two out of three (nharri and ngara but not ruddja). Garawan, again, is the only non-Pama-Nyungan family to witness a somewhat related form, niriruha, though without an initial laminal.

41. See Koch (2000) for similar arguments regarding the reconstruction of Proto-Pama-Nyungan nominal morphology.
42. I deviate from Dixon in including Yolngu in the attestations, on the basis of its reflex is the 2nd non-inclusive definite stem nhumwdas (see e.g., Morphy 1985:37).
43. I follow Dixon in including the Western Torres Strait language, whose form is nhul in KLY (though nhul in KKY, probably an innovation), but deviate in including Yolngu on the basis of attestations in Dhulwa/Ogup, Dhulwa, Ritharrku, Djipju, Djiru, and Yawuru, even though it does not allow for the effects of regular loss of the stop from homorganic nasal + stop clusters (Alpher 2004:122-125; e.g., the Djipju 2N:16 stem is nhumwani- (Morphy 1985:37).

MAP 4. DISTRIBUTION OF 2ND PLURAL *NHURRA AND ITS REFLEXESES (ADAPTED FROM DIXON 2002:269)

44. Among the many types of apparently diffused phenomena that do not align with the Pama-Nyungan boundary are the structure of the verb lexeme (single stem vs. light verb + covert), the geographical partitioning of a whole range of phonological features (e.g., phonoautics, phoneme inventory), and the distribution of many lexemes such as those for artifacts.
If our question, then, is what the actual boundaries of the phenomena are in each case, then Dixon is literally correct when he says that the isoglosses do not coincide, because each of the above individual phenomena within Pama-Nyungan shows a geographically distinct pattern of retention. But if our question is why the relevant forms are always penned up inside and against the Pama-Nyungan boundary, with a wide distribution across the Pama-Nyungan zone, and a number of distinctively Pama-Nyungan forms repeatedly occurring in three discontinuous areas, then we have a very significant clustering indeed.

We could repeat the same arguments by mapping many other forms, such as third person plural pana, the other ergative case allomorph -la, locative allomorphs -ngka -la and accusative -nha* mentioned above, and of course for many lexical items. In

MAP 5. DISTRIBUTION OF 2ND DUAL *NHU(M)PALA AND ITS REFLEXES
(ADAPTED FROM DIXON 2002:276)

45. It is sometimes said that this appears in non-Pama-Nyungan, in the form of the object-marking element -n within the pronoun prefix system (e.g., Kumawingk ngan- "1SG subject", ngan- "3SG object [e.g., with 3rd singular subject]"; see further examples in section 5). Do not regard this as a persuasive argument, because we are computing an element within a bound prefix system with suffix found with free pronouns (both pronouns and nouns), and the two should be kept apart. If, as the evidence appears to us, the -n in the prefix system is more ancient in the non-Pama-Nyungan languages, it is invalid to derive the former from the latter.

MAP 6. DISTRIBUTION OF 1SG PERSON PLURAL EXCLUSIVE *NGANA
AND ITS VARIANTS (ADAPTED FROM DIXON 2002:275)
to Perth, and down much of eastern Australia, seems much more homogeneous. But such intuitive generalizations, though often voiced informally, have yet to receive support from more principled studies using quantitative techniques.

Overall, though, the arguments given in AL against Pama-Nyungan do not strike home, in this reviewer's opinion. They rest, on the one hand, on some unsustainable assumptions about the relationship between survival rates (in terms of intensification in descendant languages) and reconstructability, and on the other, on the assumption that diffusion can simply be asserted to have occurred, escaping the burden of evidence that is needed by inheritance accounts.

6. THE PROBLEM OF PRONOMINAL PREFIXES. Bound pronouns are found in a large proportion of Australian languages, in various guises—prefixed or suffixed, proclitic or enclitic, to the verb or to an auxiliary element. Together with case morphology, noun-class prefixes, verbal suffixation for TAM, and verbal suffixation for valence-changing operations like reflexive, reciprocal, causative, and comitative, they constitute one of the major domains of grammar where we can use organized paradigmatic systems for evidence of historical relatedness.

AL devotes one and a half chapters to the topic (all of chapter 8, and the first half of chapter 9). In many cases Dixon is undoubtedly correct in seeing patterns of bound pronoun use as subject to social influence, oblique and inflected or inflected to a variable extent, and systems dwindle and are replaced by new ones, often at a different grammatical site, such as the well-known shift from prefixation in the western Mindi languages to suffixation in Jangala as an erstwhile prefixed verb reduces to an auxiliary that is then suffixed to a newly developed set of verb roots, which were originally unreflexing covers—see Blake (1990), Ian Green (1995), and Green and Nordlinger (2004).

However, there is one point where I would like to propose quite a different account to that given in AL, concerning the issue of pronominal prefixes to the verb "he" in (most) non-Pama-Nyungan languages, and the antiquity and relatedness of these systems. On p. 354, Dixon states: "It is clear that at an earlier stage Australian languages simply had free pronouns." As bound pronouns in non-Pama-Nyungan languages, as Dixon maintains, simply convergent but independent developments, spread from one group to another by diffusion, or do they represent a complex inherited system? This question is crucial to arguments about whether all Australian languages are related and relevant also to higher-level groupings of non-Pama-Nyungan languages and to the relationship between Pama-Nyungan and non-Pama-Nyungan. In fact, the amount of shared, paradigmatically organized morphology across non-Pama-Nyungan pronominal prefixing systems is sufficient that deeper-level reconstructions are likely to prove rewarding.

I will now give a taste of the sort of evidence that points to the relatedness of nearby all non-Pama-Nyungan languages, and allows a reconstruction of this domain of verbal morphology. I say "nearly all" because there remain a few languages of the Durbin region—Limilirig and Larakuya—as well as many languages of the Daly River, in which the reflexes of the forms to be assembled below barely appear. To avoid clutter I will select a few representative morphemes from half-a-dozen witness languages that represent most of the diversity found among the non-PN languages, as well as possessing a good geographical spread.

(3) Verb: S/O/IRR - Root - RR - TAM

The pronominal system encompasses prefixes for subject, object, and indirect (sometimes manifested as futurity), as well as a suffix -a that mostly follows the object morpheme, if present. The ordering of the pronominal elements is complex: a constraint on the order of subject and object is rivaled by another, stronger constraint that first and second person morphemes precede others, while the placement of the intransitive morpheme %en%u% (often reducing to pa, na, or ana) depends on the syllability of the other pronominal elements and the placement of morpheme boundaries between them. An idea of the similarities between different non-PN languages can be gained by comparing the following six forms of the verb "hit" in Mawng (Iwaidja) and Nunggubuyu (Durwinygaaan), the former spoken on the north Arunta coast of Amherst Land, and the latter also in Amherst Land, but more than five hundred kilometers away on its eastern coast. They belong to quite distinct non-PN families, and no investigator yet has proposed any genetic or areal link between them; even Dixon's far-reaching "Amherst Group" does not include Mawng. The forms for "he hit me," and 'she hit me' are given, in the future/irrealis and two reflexive forms (m: masculine; f: feminine).

(4) 'he > me'
Mawng nganu-wang nganu-wun nganu-wun
Nunggubuyu nganu-wang nganu-wun nganu-wun

(5) 'she > me'
Mawng ngangga-wang ngangga-wun ngangga-wun
Nunggubuyu ngangga-wang ngangga-wun ngangga-wun

The root for 'hit,' a reflex of pu in most Australian languages, has (probably independently) lent itself to ws in both of these languages, although pu is still found in certain environments, not shown here, such as after nasals. And two of the three tense/spect/ mood suffixes are cognate: past 1 -ng and past 2 -ni; the two forms basically express a perfective vs. imperfective contrast in both languages. In Nunggubuyu the form for 'hit' has been somewhat disguised by vowel changes in the two past tenses, but the original is preserved in the future as well as in other tenses not shown here (nonpast 3 ws and evitative wamangan).
As can be seen, the pronominal morphemes are arranged with first person (ego-) preceding third person (su if masculine, sage- [Mx] or ugil- [Nu] if feminine). An object marker "n" is placed after the first person prefix, which is in object function here. In Mawang this "n" disappears before another "n," and in Nunggabuyu, where the morphophonemics is more complex, assimilates to before "n" and is lost before any other nasal; wherever it occurs, it is shown in bold for ease of identification. Finally, a future marker pe- (identical in both languages, at least in this environment) is placed in this case between the object marker and the third person subject. In Nunggabuyu this future marker has been extended to a general nonpast marker and is also used in some nonfuture tenses, e.g., the past 2 which typically has an imperfective meaning. As a result of these changes, Mawang reuses prefixes from the first column in the third, while Nunggabuyu reuses prefixes from the second column instead. Despite these language-specific changes, the complex inflected words considered here show clear similarities between Mawang and Nunggabuyu in form, word structure, and the combinatorics of specific morphemes.

These forms are only a tiny fraction of the paradigm. The complete transitive paradigms may contain upwards of fifty elements in those languages with inclusive vs. exclusive plus a three-way number distinction in both subject and object, and some of the cells, such as the "you"/"you" and "you"/"you" combinations, are notoriously unstable thanks to the effects of "pronominal disguise" first identified by Heath (1991). Once we compare these across fifty or more languages it is easy to lose the thread and no one has undertaken this mammoth task.

However, to show that the resemblances are not simply restricted to a well-chosen pair of languages, I now give a slightly broader sample of eight languages (from six non-Pama-Nyungan families) and seven paradigmatic values (table 3).

**Table 3. Selected Prefix Forms in Eight Non-Pama Languages**

<table>
<thead>
<tr>
<th>Language</th>
<th>(PWA)</th>
<th>(TTW)</th>
<th>(GUN)</th>
<th>(WBR)</th>
<th>(MNDN)</th>
<th>(WBR)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mawang</td>
<td>100n</td>
<td>210n</td>
<td>330n</td>
<td>150n</td>
<td>330n</td>
<td>150n</td>
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<tr>
<td>Tiwi</td>
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<tr>
<td>Nunggabuyu</td>
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<td>Wambaya</td>
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<td>Kwiala</td>
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<td>Unga-</td>
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<td>150a</td>
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</tbody>
</table>

Notes:
- In the paper on Proto-Guuguwaynu, the verb inflections appear in the past perfective ("ekeem- yungap"). The Nunggabuyu form "ekeem-yungap" represents the verbalization of a and d that occurred in Nunggabuyu phonology; we do not yet know enough about Tiwi historical phonology to know whether Mawang "ekeem" derives from a merger of "a" and "e" or contains an original "a".

Only the first combination mentioned above ("he > me") is included, in the fifth row, and in most languages this has the form ngau- rather than ngau-, possibly because in most languages the third person masculine pronoun is encoded by if rather than ni-; nonetheless, its cognacy in all languages except Tiwi and Wambaya is clear.

In this diagram I have bolded elements that, in a given language, appear to be ancestral and derivable from the postulated protoforms given in the right-hand column (I label this "pX"). Proto-X, to emphasize that this may not go all the way back to "Proto-Austral", through this certainty is one possibility. Without going into the details, it should be clear that there is substantial comparability of forms across the seven representative languages, which were chosen to span the full non-Pama range.

For a fuller consideration of the intrinsic prefix forms, the reader is referred to Harvey (2003c), which postulates an eight-valued system, with four persons (first inclusive, first exclusive, second, and third) and two-valued number system of the minimal-augmented type (so that he would gloss my 1sG, 2sG, and 3sG above as 1 minimal, 2 augmented, and 3 augmented). The two forms karr- and nivnr- each have such widespread reflexes across non-Pama-Nyungan that Harvey argues both should be reconstructed as a second person plural prefix; the conditioning factor between these two forms (which may have been some sort of tense/aspect/mood category, for example) has yet to be worked out. For the transitive forms we await a systematic study, and the abovementioned phenomenon of "pragmatic disguise" (Heath 1991), which results in the rapid turnover and consequent lack of cognation of prefixes registering combinations of two speech act participants, means that we may only end up being able to reconstruct these parts of the transitive paradigm in which at least one participant is third person. Nonetheless, the apparent reconstruability of much of the complex pronominal prefix paradigm across a large number of non-Pama-Nyungan languages suggests (a) that the widespread distribution of pronominal prefixes in this area—contrary Dixon—reflects shared inheritance rather than diffusionally driven convergent grammaticalization from free pronouns, and (b) that, consequently, it will be fruitful to attempt reconstruction of a higher-level unit that includes many, if not all, of the non-Pama-Nyungan languages. See Evans (2003b) for a further discussion regarding formal similarities in verbal derivational suffixes, and in-case-sensitive variants of noun-class prefixes.

A fundamental question in Australian linguistics concerns the relationship between the relationship of Proto-Pama-Nyungan to the proto-language one reconstructs from non-Pama-Nyungan languages. Dixon's model, as we have seen, rejects both these entities, but the widespread and detailed resemblances of form considered in the last two sections point clearly to genetic relationships, at least within each of these units.

One, first proposed by Heath (1978) and continued by him in a series of publications concerning the reconstruction of non-Pama-Nyungan noun-class prefixes (1987), verbal prefixes (1990), and verbal suffixes (1990), adopts a first binary branching of a presumed "Proto-Australian" into a "Proto-Pama-Nyungan" group (as in figure 1).
In its favor, the model in figure 1 posits an entity to which the shared forms mentioned above (and many others) can be attributed, and it accounts for the striking typological difference between Pama-Nyungan and non-Pama-Nyungan languages. However, it fails to account for a number of aspects of the relationship between Pama-Nyungan and non-Pama-Nyungan. First, virtually all investigators have seen the Pama-Nyungan languages as less diverse than the non-Pama-Nyungan ones, a fact most clearly reflected in the large number of non-Pama-Nyungan families set up in the O’Grady et al. classification. Second, there is at least one phonological innovation attributable to Proto-Pama-Nyungan, which has merged the initial labials and apicals that are distinct in most non-Pama-Nyungan languages (Evans 1988). Third, as we saw in section 5, the growing evidence for the lack of various Pama-Nyungan features in all non-Pama-Nyungan languages has shifted many grammatical morphemes from the category of retentions from "Proto Australian" to innovations in Pama-Nyungan. For example, whereas the "Proto Australian" dative -da has widespread non-PN attestation, the "Proto Australian" ergative instrumental -da - ngula and locative -da - ngula are looking more and more likely to be a PN innovation (see section 5 above). Further, some of these Pama-Nyungan innovations can now be given plausible sources based on their development from non-Pama-Nyungan precursors: an example is the development of the Pama-Nyungan system of "prohibition markers" by the analogical remodeling of a more irregular paradigm retained in the Gunwinyguan languages (see Alpher, Evans, and Harvey 2004).

Taken together, these considerations suggest the alternative model, shown in figure 2. This model treats Pama-Nyungan as an offspring of a shared ancestor that also gives rise to some non-Pama-Nyungan languages. The advantages of the Pama-Nyungan offspring model are that it accounts for the higher level of mutual resemblance among the complete set of Pama-Nyungan languages than among the set of non-Pama-Nyungan ones, and for the sharing of certain features (such as elements of the verbal suffix system) between Pama-Nyungan and particular non-Pama-Nyungan branches. Its key disadvantage is that it needs to account for—and motivate—the disappearance of pro-nominal prefixes, though there are possible mechanisms for this, such as the resignification of verb/whilst prefixes into second-position auxiliary.

The challenge facing the Pama-Nyungan offspring model is to show how the suite of distinctively Pama-Nyungan morphemes can be given etymologies from non-Pama-Nyungan cognates with different functions, that serve as grammaticalization sources, and of how the distinctive Pama-Nyungan pronoun paradigm emerged from a previous free-form paradigm with different forms and a different logic. This task, which requires a more etymological approach than has characterized Australian linguistics to date, has barely been touched upon: until very recently, for example, reconstructions of proclausal systems have tended to mix free and bound pronouns as if they belong to a single system (see Harvey 2003). However, I believe that the balance of evidence has now reached the point where the "diffusion model" of similarities between non-Pama-Nyungan prefix systems can no longer be sustained, so that research needs to focus on evaluating the other two competitors.

7. PROSPECT. By way of drawing together the strands that have run through this review, let us return to three main themes: the applicability of the comparative method, the 50 percent equilibrium level, and the relation of AL to previous scholarship in the field.

A. Applicability of the comparative method. AL claims that Australian languages present a special case where the comparative method cannot apply, owing to such widespread diffusion over an incredibly long period of "equilibrium" that it is impossible to distinguish the effects of inheritance from diffusion. Against this, I have argued that the comparative method, applied carefully and patiently, is in fact beginning to yield divi-

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49. Dixon (2002:128-9) rightly points out a number of exceptions to this change, which means that it does not correlate perfectly with the Pama-Nyungan / non-Pama-Nyungan boundary. The correspondences are indeed more complicated than I proposed in my 1990 article; it has since been shown by Harvey (2003a) that these Gunwinyguan languages (Birriny Guw wurk, Djabowon, and Berchamber) are, for example, have undergone a change in the opposite direction, detemporalizing initial bimoraic syllables to apical details. The whole tone is too complex to go into further here, although Dixon (ibid.) nonetheless admits the idea is "basically sound."
dends—brought together most clearly in the recent book by Bowen and Koch (2004). Methodologically, using the comparative method has the great advantage that cases of borrowing stand out all the more clearly—McCowan’s classic studies of the diffusion of subcultural terms (McCowan 1985a, 1997) are a beautiful example of how the specific quirks and exceptions introduced by borrowing can be more clearly disentangled against the basic assumption of regular sound changes, and his more recent work on “linguistic engravings” using evidence from borrowed lexical items offers, for the first time in Australian linguistics, the chance of linking typological changes to discrete changes in the archaeological record, such as the appearance of grindstone technology (McCowan and Smith 2003). It also forces us to produce more finely articulated models of the historical relationships between languages, of the web of phonological correspondences between cognate sets, and of the etymological relationships between morphemes than we can get by simply assuming a vast muddy swirl of endless diffusion.

Of course, borrowing between neighboring languages has taken place, and this means that few of the sound changes that have been claimed apply in 100 percent of cases, but if Indo-Europeanists in the time of Grimm had simply deduced the comparative enterprise hopeless because of a number of exceptions to Grimm’s law, they never would have discovered Verner’s law, nor been able to isolate the provable cases of borrowing that now stand out all the more clearly against a general pattern of regularity. Assuming the comparative method thus invites falsifiability—or refinement of general laws by the identification of borrowing—in a way that the “punctuated equilibrium” model generally has not. But we must be clear about what we mean by “falsification.” As discussed in section 5, many of the claims AL makes against the applicability of comparative reconstruction to Pama-Nyungan, in particular, rest on misapplications of arguments from absence of attestation—assertions that forms are not reconstructable because a certain number of relevant languages lack reflexes, rather than proceeding to reconstruct them by the “three-witness principle.” The principles employed in AL obscure the significant alignment of isoglosses that one finds in the case of Pama-Nyungan once one is assumed to be a normal process (cf. Alpher 2004:124).

B. The “50 percent equilibrium” model. One specific prediction of the punctuated equilibrium model that is falsifiable is its prediction that languages that have been in contact for a long period will come to share 50 percent of their vocabulary. This claim, as I have argued in section 4, is manifestly false, a position already demonstrated in detail by Alpher and Nash (1999). There is not a single well-documented case in Australia where two languages that are not closely related genetically share 50 percent of their vocabulary. The closest case is Ngandi/Ritharrguy, where a recent of shared vocabulary gives a figure of 41 percent, but this is an exceptional case, and it is rare for the percentage to exceed 50 percent, and common for it to be below 10 percent.

Again, part of the reason for my view diverging so greatly from that in AL concerns initial assumptions. If one assumes that the genetic model is basically applicable, then one requires that the case for the 50 percent equilibrium model be based on examples where the languages are known not to be related, or at least not distantly so. If, on the other hand, one assumes the irretrievability of the comparative method at any reasonable depth, as is done in AL, then the claim is difficult to falsify: if languages share over 60 percent vocabulary, they are related; if they share less than 40 percent, they have not been in contact for long enough to have reached the equilibrium level; if they share between 40 percent and 60 percent, they support the model. It is because of the un falsifiability of this latter approach that it is so important to begin with known cases of nonrelativeness, and once one does this, the supporting examples evaporate.

This is in no way to designate the interest or importance of studying areal convergence, and Heath’s careful studies in the late 1970s (Heath 1978, 1979) remain unsurpassed for rigor and detail, at least as far as studies of genetically divergent languages. (Dench [2001] presents a persuasively skeptical case against the family-tree model in the Pilbara, but in relative terms the languages are all rather closely related.) What we particularly need, at this point in time where the indigenous languages of Australia are disappearing with frightening rapidity, are detailed field studies of multilingual speech communities and the way in which processes of convergence (and also divergence!) are played out in the grammars of multilingual individuals: a move to the micro-level where ongoing change is examined.

Once one abandons the reconstructive pessimism that comes with assuming the 50 percent equilibrium model, it becomes possible to reinstate some quantitative measures of language distance, provided that due caution is exercised. The wild discrepancies one finds in the AL classification in the degree of internal diversity of groups considered to be of equal order can only be rectified in a systematic way once some quantitative measures are brought back. This is one of the many challenges that the publication of AL will hopefully provoke Australians into meeting.

C. Fresh starts vs. the cumulation of scholarship. In many parts of its classification, AL takes a blank-slate approach to the problem, consciously holding off from adopting the conclusions—or accepting the premises—of other scholars, or from accepting the composition and naming of widely accepted subgroups. The reason for this, as outlined on p. xxi of AL, is to get out of the “cal de sac of its own making” in which Dixon perceives Australianist work to have become “unmanageable,” and “to make a side-step in order then to move forward.”

There certainly are particular difficulties in doing historical linguistics on Australian languages, and it will help us recapitulate if we spell out the three most important of them here. The first difficulty has been the lack of good descriptive materials. Dixon has done more than anyone to remedy this through his own work and that of the dozens of students he has trained or inspired. Nonetheless, the number of large dictionaries is still pitifully small, and it is therefore no wonder that we still lack a single etymological dictionary of any Australian subgroup.

The second difficulty is the phonological similarity of most Australian languages, something which blunts the edge of phonological comparison. This is certainly an area

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50. McCowan’s study was possible because it drew on four types of evidence: (a) comparison of the structure of different systems (sections and subsections), (b) geographical distance, (c) etymology, as a key to source languages, in particular the etymology of the gender prefixes, and (d) a relative chronology of events and sound changes, allowing us to place the borrowed subsection terms in a stratified sequence of historical changes for each language. Significantly, the otherwise sympathetic discussion of this work on pp. 16–18 of AL mentions the first three, but not the fourth.
that a conceptual "cul de sac" exists. Nor do the technical "sidesteps" it proposes lead forward, because they amount to an alternative null hypothesis rather than a new method, and they do not lead to the sort of articulated model of language history that enables an even more detailed picture to be built up by the cumulative efforts of the scholarly community.

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In this article I have voiced disagreements with just about every aspect of AL: with the claims it makes about the special status of Australian languages, about the implications of standard methods to their historical study, about the operation of diffusionist forces beyond what we encounter elsewhere, and about the classification of Australian languages. On all these issues, I have argued, AL represents a distinctly personal, and in my view debatable, view of the field.

Nonetheless, I believe this volume will have a constructive effect on the historical linguistics of Australian languages. No other book on Australian languages has brought together so much historically and typologically relevant material in one place, so that whatever differences of interpretation one may have, it gives a comprehensive compilation of data from across the continent. Most importantly, its deliberately unorthodox stand on virtually every topic is a bracing challenge to complacency. Whether other scholars react by deploying the comparative method with renewed vigor, by developing or renovating quantitative methods to deal with some of the scaling problems discussed in sections 3 and 4, or by setting out a synthetic picture of language relationships that organizes the many low-level groups into a more coherent larger picture than AL provides, this publication will energize scholarly debate among Australianists.

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Book Reviews


Languages of the Eastern Bird’s Head (LEBH) is an interesting, though challenging, book to review. It does not fit the mold of many edited volumes these days. It is a book that is too rarely seen: no “last words” (except for the last chapter—one more on this later), no exhaustive studies, but solid material to make a typologist delight, data to make a formalist ponder, and comparison both synchronic and diachronic to make anyone interested in the languages of New Guinea sit up.

The volume consists of five parts: the introduction, by R, detailing some broad comparative issues concerning the Eastern Bird’s Head languages, both sociolinguistic and typological; the central chapters, in which we find grammatical sketches of Mpar (by Cecilia Odé), Meyh (by Gilles Gravéille), and Sough (by R); a salvage sketch of Boni by R; and a lexical overview of the area. The context, then, focuses on the languages of the Eastern Bird’s Head (not, as the back cover states or implies) all of the peninsula: “This book is the first detailed introduction to languages of the Bird’s Head peninsula of Indonesia, [sic] Irian Jaya.” I shall (approximately) appropriate the division of labor on the back cover to represent the division of labor in this review: coverage of the sketches, but more attention paid to the documentary aspects of the presentations, and of the introduction.

To review a book, the bulk of which is a collection of sketch grammars, I must examine the point of a sketch description. This is not trivial: who is a sketch grammar intended for? There has, thankfully, been a flood of sketch grammars on the market in the last decade, so we might have some idea of what a sketch grammar is, but not such a good idea about why it is. This question is addressed in the next section. I then devote considerably more space to a summary and critique of the argumentation and conclusions in R’s introduction. Here follow highlights of some constructions of particular theoretical interest that are described in the book, and in the conclusion I summarize the historical argumentation.

The point of a sketch grammar: We might illustrate the two opposing positions in a hypothetico-deductive theory of linguistics, and sketch grammars. On the one hand, we have the work such as Kambera’s (1990) description of Kambera. This is a comprehensive description of the grammar of the language, covering all the major aspects of the syntax, but it does go into considerable detail on selected topics, to an extent that is rare in most comprehensive grammars. This is an attention to detail that makes it all the more valuable for (non-descriptive) phonologists and syntacticians, in a way that many more comprehensive grammars are not. Many of the questions that formal theory-inclined linguists, but not necessarily ran-of-the-mill typologists, would want to know have been

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