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Reassessing Karnic: A Reply to Breen (2007)[1]

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The Karnic subgroup of Pama-Nyungan has had a complex history and despite 6 classifications there is little agreement about its internal composition beyond the lowest-level groupings. Here I respond to Breen's (2007) criticisms of earlier classifications and discussion of subgrouping of 'border' languages such as Arabana-Wangkangurru and Garlali. I further make some methodological points regarding reconstruction and subgrouping.

Keywords: Historical linguistics; Australian languages; Classification; Lexicostatistics; Pama-Nyungan

1. Introduction

Breen's (2007) discussion of the status of the Karnic subgroup of Pama-Nyungan, and his criticism of Bowern (1998) and Bowern (2001a) remind us that subgrouping from a single area of grammar is likely to be unreliable. However, Breen's own article raises many questions about method and data, in particular the recurrent use of lexicostatistics in Australian subgrouping work. There is no reason to suppose that a blunt tool like lexicostatistics will be particularly reliable for Karnic classification. On the contrary: it is precisely complex contact areas such as the Lake Eyre Basin which fail in lexicostatistical classifications.[2] Karnic has been the subject of multiple classification hypotheses since Schmidt (1919), and there is still no general agreement about which languages belong to the subgroup: this would imply that a classification involving tree-like structures misses subtleties about the split up of these languages.

[1] This is based on work done as part of NSF CAREER BCS-844550 "Pama-Nyungan Reconstruction and the Prehistory of Australia". Thanks to my co-supervisors at ANU, Harold Koch and Luise Hercus, for their guidance in the initial work on this topic, and to Luise Hercus and Gavan Breen for data on Karnic languages which has made research in this area feasible in the first place.

[2] This is well-known, as I discuss below, however the continued publication of historical articles using results from these methods strongly implies that the facts could bear with repetition.

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In this paper I address some of the method and data questions which Breen (2007) raises, not only for Karnic, but for reconstruction more generally. Are there methods of reconstruction and subgrouping preferable to the comparative method? What support do the results from lexicostatistics give to a classification in an area like this? Should we expect tree-like groupings in areas like Karnic? I explore some of these ideas in this reply.[3]

2. Issues raised by Breen (2007)

Breen (2007: 178–182) gives a summary of previous classifications of Karnic languages, concentrating on Austin (1990a) and Bowern (1998, 2001a). He finds previous classifications inadequate, and instead proposed some clarifications using a set of lexicostatistical comparisons within word classes, particularly verbs. However, Breen's own results are inconclusive (as he himself acknowledges); his method does not solve the problems already identified in Bowern (1998), in particular with respect to the status of Northwestern Karnic. While Breen claims to show that two languages formerly considered Karnic do not belong to the subgroup, his discussion for Badjiri repeats the tentative conclusions of Bowern (2001b); I discuss the status of Garlali in §2.8 below.

2.1. Reconstruction and subgrouping

Reconstructing prior stages of a language and producing a subgrouping hypothesis do not involve precisely the same methods. In the former case, we attempt to discover what is archaic and what is innovative within a language, we hypothesize a set of changes, and we 'undo' those changes in order to discover as many properties as we can of the language ancestral to those under study. When we are subgrouping sets of languages, we work out which languages are most closely related to one another, and which languages are more distantly related to one another. Subgrouping relies on shared innovations; that is, subgrouping cannot be done without reconstruction. Conversely, reconstruction usually entails a more or less implicit hypothesis about subgrouping. Reconstruction to a proto-language involves identifying shared archaic material, whereas identification of subgroups requires identifying shared innovations. Bowern (1998) was primarily an exercise in reconstruction of nominal morphology and pronouns, and only secondarily a study of subgrouping.

[3] An anonymous reviewer suggested that there was nothing new in this paper. On the contrary; Australianists have consistently worked within the paradigm that 'regular' change causes neat splits in trees and all messiness is caused by diffusion; a view implied also by Thomason and Kaufman (1988) and Labov (2007). I suggest this is a considerable underestimation of the processes involved in linguistic differentiation. The theoretical considerations of 'fringe' languages have also not received attention in Australianist literature.

2.2. Type of data used for reconstruction and subgrouping

One of the main reasons that Bowern (1998) remains unpublished is the inadvisability of basing reconstructions and classification on a single area of grammar. The reasons that the study was confined to pronouns in nominal morphology were prosaic: pronominal and nominal morphology reconstructions could be done in the time available for a fourth year honours thesis, where the extent of diversity of data within the subgroup were unknown and where much information had to be gleaned from original fieldnotes or 19th Century sources. It was also useful to base reconstructions on data of that type because we already had higher-level reconstructions for some Pama-Nyungan case morphology and pronouns, and therefore some evaluation could be made about the extent of innovation within Karnic itself. Austin (1990a) had included lexical reconstructions and sound change, and the systems of verbal morphology are sufficiently different amongst the languages that they are unlikely to be diagnostic for subgrouping. A full lexical reconstruction was not feasible with the time and data available (although one is now in progress).

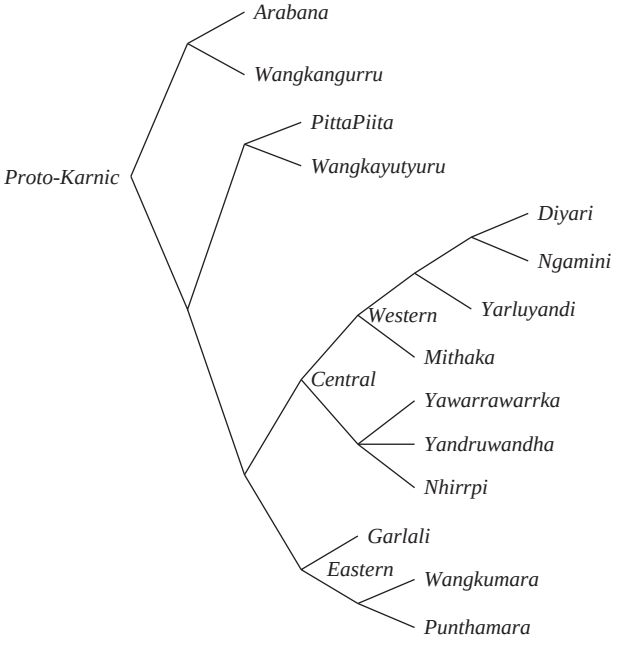
Bowern (1998) had a secondary aim of subgrouping; in particular, I wanted to see to what extent evidence from nominal morphology and pronouns was congruent with the classifications obtained by lexicostatistics (Breen 1971, O'Grady *et al.* 1966) and some change and lexical data (Austin 1990a). In fact, the evidence for subgrouping was ambiguous, even within this area of grammar. There was some evidence for shared innovations which link Arabana-Wangkangurru and Pitta-Pitta (and associated dialects), as Hercus (1994) had already pointed out. However, there is also evidence for shared innovations which group Pitta-Pitta with the rest of Karnic. I suggested that this ambiguity might tell us something about the breakup of the languages. I still hold this view; a brief presentation is given in Bowern (2008a). Breen (2007) points out that I failed to come up with a consistent grouping: I agree, which was part of the reason that in Bowern (1998: Ch. 7) I discussed the areal implications of the subgrouping conflicts.

In the 90-odd years since the publication of Schmidt (1919), we have had at least six distinct subgrouping hypotheses for Karnic, making it one of the most controversial areas within Australia. For example, Austin (1990b) excludes Arabana-Wangkangurru from Karnic, while Hercus (1994) found evidence for grouping Arabana-Wangkangurru with Pitta-Pitta and Wangka-yutyuru. Bowern (1998) also found innovations which linked Pitta-Pitta and Wangka-yutyuru to the rest of Karnic. A summary tree is given in (1). The areas of ambiguity involve the linking of Pitta-Pitta and Wangka-yutyuru, the placement of Mithaka, and the status of Garlali.

The fact that we have yet to reach consensus on anything other than the lowest level groupings may well indicate that a tree is not the most appropriate representation for this group. The evidence for ambiguous subgrouping, as I outline in Bowern (2008a) and Bowern (2008b), includes not only vocabulary but also shared

innovation in morphology, and the reconstructions support conflicting subgrouping throughout the stratigraphic history of the subgrouping.

(1)



Finally, let us consider the reasons that we should base subgrouping on multiple areas of grammar. One reason is that if the heavy borrowing is involved, one area may provide a false result. A more important reason is that correspondences and cognacy cannot be properly evaluated in morphology without reference to sound change in the language as a whole. After all, a correspondence of p : p in an affix requires special explanation if the regular correspondence between two languages in that position is p : f. Morphology is subject to certain special changes which can obscure true cognacy (Koch 1996), and thus establishing cognacy can be difficult if analogy or other change has been involved unless there is also a sound basis for phonological correspondences.

The other standard reason for a statement of preference for using morphology over lexical information is that words are more subject to borrowing, while morphology is. Therefore if the morphology leads us to one set of subgroups, but lexicon to another, morphology should be privileged because it is less likely to be borrowed. However, such arguments are also suspect in cases of intensive lexical borrowing, as morphology may be borrowed along with lexical items. For example, both Latin and English have a suffix *-ar-* ~ *-al* (as in *familiar*, *velar*, *perpendicular*, etc). In this case, however, it is fairly clear that borrowing is involved, since the suffix in English occurs overwhelmingly on items of Latin origin. The borrowings are sufficiently entrenched that we also find morphological doublets, such as *familiar* and *familial*,

which entered the language later. But if we looked at morphology alone, we would be tempted to reconstruct this morpheme.

2.3. Shared innovation and archaism

In §2.1 I mentioned the use of shared innovation and common archaism in reconstruction and subgrouping. From early on in linguistic subgrouping, it has been recognized that shared innovations alone, not shared retentions, are the *only* evidence admitted in subgrouping (see amongst many others Harris (2006) and the references there). This is because shared retentions from earlier proto-languages do not define groups. For example, Germanic, Slavic and Tocharian share cognates of English 'deep' (Tocharian A *tpär*, B *tapre* 'high' (English 'deep') OCS *dЪbrЬ* 'abyss').

However, this does not define a common subgroup. Of course, not all innovations are shared innovations and diagnostic of subgrouping, since some changes are common, but shared changes are the only diagnostic of common subgrouping.

Throughout Breen (2007) there appears to be no clear distinction between shared innovation and shared features. This is, perhaps, a result of a paradox that shared features establish a family but only shared innovations are diagnostic of a subgroup. For example, Breen (2007: 178) uses the fact that Karnic languages show *ngali* in exclusive rather than inclusive meaning as diagnostic value for the subgroup. But this is almost certainly a shared archaism, not an innovation. The inclusive pronouns in Karnic languages are quite diverse, and this would imply that the creation of an inclusive form postdates the breakup of the family.[4] In that case, *ngali* in exclusive meaning is a combination of accident and shared archaism. The collapse of inclusive and exclusive would appear to be a proto-Karnic feature, although it is not diagnostic exclusively of this subgroup since it is also shared by other languages in the region.[5]

Of course, languages which are very closely related to one another share a great deal of material, and such material may be retained from earlier stages. It would be idle to deny the significance for genetic relationship of two languages sharing significant levels of material. No one denies that Karnic languages have many features in common. But there is more than one way that shared items may come about, and that is where the problems lie in sorting out the relationships among Karnic languages. However, in many computational and data-aggregating methods of inferring relationship—including lexicostatistics—there is no principled separation of shared innovation from shared features. This is old news but it bears reiterating in the face of continued publication of work which does not make this distinction.

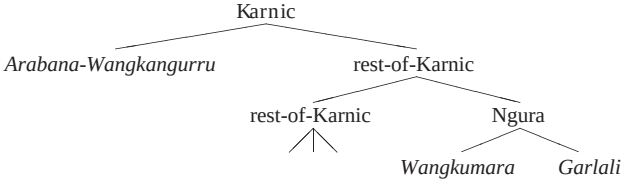
[4] The forms are Arabana *arimpa*, Wangkangurru *aruna*, Mithaka *ngali* (no inclusive/exclusive distinction), Yarluyandi and Ngamini *ngali(kuli)*, Diyari, Yandruwandha, Yawarrawarrka *ngaldra*, Wangkumara and Garlali (Holmer 1988) *ngala*.

[5] Moreover, as I showed in Bowern (1998: 134ff.), the innovation of inclusive forms within Central Karnic is problematic, as borrowing between multiple languages appears to be involved.

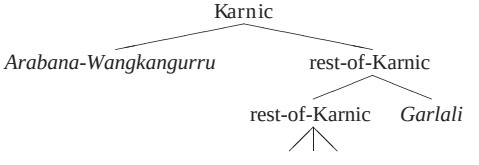
2.4. Tree models and a reality

Breen (2007) works with a family tree model. For example, he uses tree representations (e.g. p. 178). However, there is also an implicit areal model in Breen's (2007) work, too. For example, on page 192 Breen appears to assume that if Kalali (I use the spelling Garlali) is Karnic, it must be in the Ngura subgroup with Wangkumara, the closest Karnic language geographically. This is understandable, given that a lot of confusion between the languages has been between languages called Garlali and Wangkumara[6], but there is no a priori reason to assume for the purposes of subgrouping that if Garlali is Karnic, it **must** be in the same subgroup as the languages that are closest geographically to it. We have no reason to assume that any of the trees depicted in (2) are right, without testing by the comparative method. (2a) shows Breen's implicit hypothesis; (2b) and (2c) show alternatives.

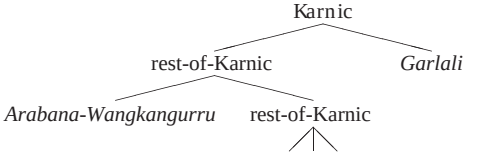
(2) a.



b.



c.



However, there are few areal models properly developed for Australian languages. The comments made by Britain (2001) about the need for non-static areal models are very important for Australia too. While we know a great deal about diffusion patterns in urban and sedentary agricultural societies (see, for example, Chambers 1998), there is little work on similar topics in Australia. Heath (1978) and Miller (1972) are notable exceptions, but Arnhem Land is rather different from the Lake Eyre Basin, and Miller (1972) was extensively criticized by Wilfred Douglas in a reply in the same volume of *Anthropological Forum*. This is an urgent area for further research.

[6] See, for example, the title of McDonald and Wurm (1979), where the title reflects the fact that the authors thought the language they had described was Garlali until just before going to press.

2.5. 'Fringe' languages and sources

A recurring theme in Breen (2007) is the omission of various languages from discussion. In Bowern (1998) and Bowern (2001a) there were a number of languages that were not included, not because I claim they are not Karnic, but simply because I did not have enough data to be able to mount a cogent argument for their inclusion or exclusion on morphological grounds. These included a number of languages around the fringe of the better-attested Karnic languages, including Pirlatapa, Kungkari, Pirriya, Karruwali, Yanda, Guwa and Yiningayi. In the case of Pirlatapa, Austin (1990c) has shown that the language has many similarities to its northerly neighbours Diyari and Ngamini, and I agree with his tentative subgrouping of language on that basis. However, there are only a few sentences known of the language. For the fringe languages Pirriya and Kungkari, there are lexical data but little or no pronominal or morphological forms recorded. They have been included in subsequent Karnic work of mine which includes lexical work.[7] A case for the coherence of Karnic won't be strengthened when the data from such languages is so sketchy. After all, the case for Proto-Indo-European wasn't made from Luwian, Crimean Gothic and Old Prussian, but from copiously attested languages where the data are clear. In a controversial subgroup, the status of badly attested languages must be left until we have a better idea of the structure and content of the proto-language. This can then be revised where the more sparsely attested language provide crucial evidence.

2.6. Another type of 'fringe' language

In the previous section, I described 'fringe' languages as ones for which there was too little data to accurately assess their relationship (or to use them in preliminary reconstructions). However, there is another interpretation of the term. In areal linguistics, 'fringe' or 'peripheral' languages are those which do not participate in the all the changes which the central (core) languages of the area undergo. If the modern distribution of Karnic languages reflects an earlier dialect area (as I've argued elsewhere; see Bowern 2008b), we would expect languages around the periphery of the Lake Eyre Basin to exhibit some properties of core Karnic languages, but not all of them. This is a theoretical claim about the nature of language change and the specific sociological circumstances which led to the breakup of Karnic. Under this definition, Arabana-Wangkangurru is a fringe language, but Arabana is one of the best-described languages in the Lake Eyre Basin (and so definitely not a 'fringe' language under the definition in §2.5.). Another situation in which we might expect fringe languages to develop is in cases of rapid spread of speaker populations followed by quasi-isolation, where the groups participate in few of the changes which characterize one or more particular

[7] In Bowern (1998) I also omitted various dialects of the Pitta-Pitta–Wangkayutyuru dialect chain, such as Kunkalanya and Ringaringa, for the same reason.

groups. Each of these scenarios is testable with the comparative method. Within Karnic, Arabana Wangkangurru and Pitta-Pitta/Wangka-yutyuru comprise a northwestern fringe, Garlali an Eastern fringe, and Kungkari-Pirriya, etc, a north-eastern fringe. In the case of the north-eastern languages, statements about relationship are compromised by lack of data, but this is not true for the other areas. We need to assess the theoretical and practical status of a 'fringe' language (in the second sense) if a discrete family tree model is still to be maintained for these languages. If we are to maintain the status of a fringe language within a family tree model, there must have been intense diffusion in the area. That is also something we can test using the comparative method. In Bowern (1998) I argued that invoking borrowing/diffusion as the sole explanation for conflicting subgrouping was problematic.

2.7. Lexicostatistics

The problems of lexicostatistics have been well-rehearsed over the years and here is no place to repeat them (see amongst many others Teeter 1963). For Karnic, two criticisms are especially important. Lexicostatistics provides no detailed way to distinguish shared innovation from shared retention or sharing (our problem described above); and it assumes a constant rate of replacement (that is, that the languages that share the most common features are necessarily most closely related). Both assumptions are problematic, as described above.

Lexicostatistics is one of several methods available for modelling relationships (its equivalent in evolutionary biology is UPGMA; see e.g. Atkinson *et al.* 2005), not only in evolutionary biology but also in other fields—like historical linguistics—where the data for determining relationships has systematic correspondences. Just like in linguistics, its biological equivalent has been largely abandoned, used only when computationally more intensive methods are infeasible; other methods return more reliable, less ambiguous results.

Breen's method here is a modified version of lexicostatistics, where he compares relative shared percentages of verbs. Breen (2007: 182) states it as a 'fact' that word classes are borrowed at different rates, quoting Breen (1990: 157), who in turn quotes Blake (1979: 129) and Austin (1981: 5). Breen draws conclusions about relationships based on a measurement of shared percentage of verbs as a function of total shared vocabulary. However, the original explication of this idea is rather poorly founded. The explanation of the method in Breen (1990: 154–155) includes case studies from Warluwarric and Arandic, but part of the argument for different rates of cognacy between body parts, fauna, material culture, and verbs relies on a small number of unknown languages and comparison of vastly different (but also unknown) numbers of items. For example, in Table 7.1 (Breen 1990: 155), the number of verbs compared ranges from 5 to 33. We are given averages but no further information behind the summary figures. I am not denying that different semantic fields show different rates of change and different rates of borrowing. But we cannot evaluate classifications based on comparing different rates without a lot more information than was given. We do not

know what languages went into the original sample, or how consistent the figures were within groups.

2.8. Garlali

Breen (2007: 192) disputes the data for Garlali quoted in Bowern (1998). My data were from Holmer (1988), some of the tapes which Holmer recorded (information which was not in the published sketch) and which are held at the Australian Institute of Aboriginal and Torres Strait Islander Studies (AIATSIS), and Breen's fieldnotes for Garlali (MS1206). Within Bowern (1998) I noted differences between Breen's recordings and Holmer's. For this article I recompiled all my original 'Garlali' sources. What seems to have happened is that Holmer's (1988) and Breen's field note morphological and pronominal data were collapsed in some instances with the source fieldnotes for McDonald and Wurm (1979). AIATSIS PMS 4336 'Garlali fieldnotes' (a transcription by Peter Austin of AIATSIS archive tape A1777, recorded by Stephen Wurm) is listed as Garlali, and a transcription of the same material as appears in McDonald and Wurm (1979), which is actually Punthamara. My conclusions about the status of Garlali as uncontroversially Eastern Karnic were therefore wrong.

That is not quite the end of the story, however. Breen's and Holmer's Garlali data are a little closer to Karnic than one might imagine from Breen (2007: 190ff); there are areas of innovation in Eastern Karnic that the Breen/Holmer Garlali data share. Breen (2007: 191) mentions 36 Proto-Karnic forms and lists 20 of them, 12 of which have Garlali reflexes.[8] (3) – (4) lists items which could be considered innovations in an 'Eastern Karnic' with Garlali as a member. I do not assert that this is necessarily what happened, only that it is a possibility. (3) contains items discussed by Breen (2007); (5) contains additional items from my Karnic reconstructions.

(3)

Gloss	proto-Karnic (Austin 1990a)	Kalali (Breen 2007)	CB note
beard	*ngarnka	ngarnkurru	also in Wangkumarra (could be innovation)
black	*murru(murru)	murru	also in Wangkumarra (could be innovation)
breast	*ngama	ngama	not in Breen's list (but retention)
eaglehawk	*karrawara	karrawara	not in Breen's list (but widespread)

[8] One might point out that a 60% lexicostatistical figure was used earlier in the paper (Table 4, p. 184) to argue for subgrouping Pitta-Pitta and Wangka-yutyuru with Arabana-Wangkangurru, although the number of items was larger.

(4)

Gloss	proto-Karnic (Austin 1990a)	Kalali (Breen 2007)	CB note
brolga	*purrarlka	kuntharra	Eastern Karnic *kuntharra
emu	*warrukatyi	kulparri	Eastern Karnic *kulparri
fat	*marni	marni	
father	*ngapiRi	ngarni	also in other Eastern Karnic languages
Hold	*parta-	bartalingu	(not in Eastern Karnic otherwise; only in Karnic in this meaning)
lips	*mimi	mimi	(areal and sporadic; also in Adnyamthanha and Yardliyawarra)
long ago	*matya	matya	
navel	*minta	mintha	
no	*walya	walya	
old person	*karrukarru	karrugarru	
want	*ngantya	nganti	(Holmer 1988)

(5)

Gloss	proto-Karnic (Austin 1990a)	Kalali (Breen 2007)	CB note
spit	*ngaltya	ngaltya	(not reconstructed by Austin 1990); also in meaning 'sweat'
stars	*dityi	dityi~rdityi	(not reconstructed by Austin 1990)

I assume that Garlali *ngurra* 'man' is a reflex of Proto-Karnic *ngurra 'camp' with semantic shift. Reuther (1883) records the word *ngurangura* (Ngamini and Yawarrawarrka) in the meaning 'acquaintances, citizens'.

Ergative marking in *-ngu* is minor in Wangkumara (just on duals); but it was present in Proto-Karnic, and strange things have happened with ergative marking in Garlali (e.g. the use of **ngathu* 1sg.erg and **yuntu* 2sg.erg in nominative environments). This might be last speaker fluency effects or a real change.

3. Conclusions

In summary, Karnic is complex. It's not an area where a single method will give uncontroversial results. Beyond the lowest level groupings (about which there has been considerable agreement) there is considerable disagreement about the internal structure of Karnic, and which languages belong in the subgroup. This in itself is interesting, as 'fringe' languages imply a particular sequence of diversification.

Moreover, the disagreement about the internal structure of 'core' Karnic implies that something other than a family tree may be more useful to model the history of these languages. The complexity of the subgroup forces us to be explicit about the methods we use and the assumptions on which they are based. Moreover, we need a more explicit model of areal linguistics for subgroups like Karnic, beyond claims of 'intense diffusion'.

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