

# LEXICOSTATISTICS AND AUSTRALIAN LANGUAGES: PROBLEMS AND PROSPECTS

PAUL BLACK

## 1. INTRODUCTION

Geoff O'Grady has been one of the modern pioneers of the comparative study of Australian languages with his work towards the reconstruction of Proto Ngayarda (O'Grady 1966) and Proto Pama Nyungan (O'Grady 1979). One aspect of this work about which he must surely have developed mixed feelings was his role in a large-scale attempt to classify all Australian languages on the basis of the wretched data available some thirty years ago (O'Grady, Voegelin & Voegelin 1966). (Unknown to him, I played a tiny role in the same project, ignorantly sorting through lists of language and tribal names while working part-time for the Voegelins during one of my undergraduate years at Indiana University.) The classification was a major step forward, but at the same time it was full and explicit enough that it could soon be challenged by others able to gather better data or even just able to subject the early records of one particular area or another to more careful scrutiny.

In his comparative work O'Grady had taken an early interest in the common lexicostatistical approach to language classification, although he did not find the standard list of meanings used to be very suitable to Aboriginal languages (O'Grady 1960). The O'Grady, Voegelin and Voegelin (1966) classification was in part based on lexicostatistical evidence, but here other problems arose: not only was much of the data available both limited and unreliable, but because of the large number of languages the lexicostatistical percentages were generally calculated only for adjacent pairs of varieties (see O'Grady & Klokeid 1969). Unfortunately these are precisely the ones most likely to have borrowed vocabulary from each other and thus further affect percentages which already tended to be based on poor data. At the same time, the lack of a full matrix of percentages made it impossible to apply any but the crudest sort of subgrouping criteria, which regrettably were nonetheless accepted as a norm for further work on Aboriginal languages (see Wurm 1972:110).

We should be able to do much better today. Not only has there been nearly thirty more years of research on Australian languages, but we can now take advantage of improved computer technology and the computer archiving of much lexical data on Australian languages at the Australian Institute of Aboriginal and Torres Strait Islander Studies (AIATSIS) in Canberra. Whereas work towards improving our understanding of the relationships should draw on all evidence available, at the moment much of the best evidence seems likely to come from lexicostatistical studies.

Why this is so is the subject of the present article. Regrettably the history of lexicostatistics has been plagued by prejudice and misunderstanding, part of what Sankoff (1973:96) described as "a tradition of hostility towards probabilistic modelling in historical

linguistics". A major example of misunderstanding was Chrétien's (1962) highly influential paper on the mathematics of the approach, and the prejudice can be seen in the failure of the same journal to publish a rebuttal by Sankoff and three mathematicians that ultimately appeared as Dobson, Kruskal, Sankoff and Savage (1972).

Many linguists seemed to dismiss lexicostatistics on the basis of the smallest evidence that it might not always 'work', as if it could not be useful if it were not infallible. I suspect this was largely because they were not prepared to cope with the mathematical nature of the approach: if it wasn't infallible, then how could they evaluate the results they obtained to decide what parts to trust and what parts to question? In any case the same linguists seldom questioned the traditional approaches to subgrouping even though they tend to be no more reliable. This seems rather odd in that statistical approaches do not pretend to produce fully accurate results, whereas one might hope for more from the 'qualitative' traditional approaches.

The present paper accordingly begins by pointing out the limitations of more traditional approaches and why we can expect lexicostatistics to perform better in Australia. Since it has sometimes been argued that the peculiarities of Australian languages could limit the applicability of lexicostatistics, I then draw on previously unpublished work (Black 1974a, 1979a, 1979b, 1979c, 1980b) to show (a) why name taboo cannot be expected to affect lexical change in Australia to nearly as great extent as has sometimes been imagined, (b) how proposed cases of rapid lexical change are not supported by the available evidence, and (c) how lexicostatistical approaches can be used to detect and cope with cases of heavy borrowing.

## 2. AUSTRALIAN LANGUAGES

Australian languages seem to conspire to frustrate genetic classification. In earlier days what was obvious was that many words, such as *mara* 'hand' and *kuna* 'faeces' recurred across the continent with little phonological change. This made the relationship of the languages obvious, but the uniformity provided no basis for subgrouping. More recently, thanks largely to Hale (e.g. 1976a, 1976b), it was realised that various languages have indeed undergone extensive phonological change, if often of very similar sorts. Most strikingly, various groups of languages, especially in Cape York Peninsula and central Australia, have lost initial consonants and sometimes the following vowel.

One might hope that this would provide good evidence for subgrouping, but it usually has not. For one thing, similar changes may be observed in languages so widely separated geographically that their subgrouping is highly unlikely. For example, it would be possible to posit essentially the same set of rules to account for both initial dropping and the loss of distinctions among final vowels in Walangama of south-western Cape York Peninsula (see Black's 1980a study of Norman Pama) and Nganyaywana of coastal New South Wales (Crowley 1976), but I doubt that anyone would seriously consider subgrouping them together.

On the other hand, even within a reasonably clear subgroup similar changes sometimes seem to have occurred independently. For example, all of Hale's (1964, 1976a, 1976b) Northern Paman languages lost at least some initial consonants, and yet this cannot be taken to have happened just once in the common protolanguage. The biggest problem is that initial nasals could not have been lost before they caused various changes in the next consonant in

some but not all Northern Paman varieties. In Ngkoth (NGK), Luthig, Linngithig, and Yinwum (YIN), following intervocalic nasal-stop clusters were reduced to stops or prenasalised stops to varying extents, in Yinwum alone following intervocalic stops also become prenasalised, while in such other varieties as Aritinngithig (ARI) no such changes occurred: e.g. \**nYintu* > ARI *ntyu*, NGK *ti*, YIN *<sup>n</sup>ti* 'you (SG)'; \**nYipul* > ARI *pul*, NGK *pyul*, YIN *<sup>n</sup>pyul* 'you (DU)'. In addition, initial laminals were not lost in Ngkoth before they caused the following vowel to be fronted, as in \**tYuma* > \**tYima* > \**ima* > NGK *mya* 'fire', but most other varieties do not share this change: e.g. \**tYuma* > \**uma* > Awngthim *mwa*. Furthermore, whereas most varieties lost all initial consonants, Uradhi retains some in lenited form, as in *mata* < \**maRa* 'hand'.

Thus initial dropping cannot be treated as a single shared innovation providing evidence for Northern Paman as a subgroup. Even where it can be attributed to the common ancestor of a group it is such a commonplace development in Australia that it hardly constitutes strong evidence in support of the grouping. Similarly, although various languages along the south-western coast of Cape York Peninsula have lost final vowels, this 'areal feature' also seems to provide no evidence for subgrouping.

About the only evidence for a genetically significant shared innovation I've seen for Australian languages actually suggests grouping Hale's Northern Paman (NP) and Middle Paman (MP) varieties. On Cape York Peninsula, at least, apparently only these languages show a merger of intervocalic \**t* and (retroflex) \**R*, perhaps originally to \**t* but ultimately to glottal stop in many of the languages: e.g. \**maRa* > NP Uradhi *mata*, Mbiywon *ta*, Awngthim *ʔa*, MP Umpila *maʔa*, Wik-Mungkan *maʔ* 'hand'; \**kuta(ka)* > NP Uradhi *utaya*, Mbiywom *two*, Awngthim *ʔwa*, MP Umpila *kuʔaka*, Wik-Mungkan *kuʔ* 'dog'. The same languages also show a change of final alveolar \**r*, perhaps originally to a fricative \**ɣ*, but later to *y* or zero in some languages: e.g. \**akur* > NP Uradhi *akuy*, Awngthim *kawɣ*, MP Kuuku Yaʔu *akuy*, Wik-Muminh *aku* 'skin'. (This particular example also shows that Middle Paman is sometimes like Northern Paman in lacking initial consonants, but it is not clear what earlier consonant, if any, might have been lost.) However, velar or velarised reflexes of final \**r* have also been found for languages further south in the Peninsula: e.g. \**ɣamur* > Koko-Bera *ɣəmér* but Kok-Narr *ɣəmék* and Kurtjar *ma:ry* 'armpit'.

Thus even in Cape York Peninsula, where there is substantial evidence of phonological change, there is still very little evidence for shared innovations that might support a detailed classification of the languages. This should not be surprising, since it has long been clear that the traditional approach suffers from three major problems:

- (a) Some types of changes are so common that they are not particularly good evidence for shared innovations. This is generally true of such changes as the palatalisation of consonants before front vowels, and in Australia it also seems to be true of such changes as the loss of initial consonants. Hockett (1958:519) suggests that:

So many of the common innovations on which we have to rely for historical grouping are of this sort that it is rarely safe to put one's faith on a *single* common innovation: we look, rather, for several which set off the same subgroup of languages against the rest of those in the family.

- (b) Hockett (1958:521–522) also notes that where we do find an abundance of common innovations, sometimes they support conflicting subgroupings. For example, a number of innovations support subgrouping the major branches of Indo-European in different

ways, perhaps because they spread over the different sets of branches when these were still mutually intelligible dialects of Proto Indo-European.

- (c) Finally, there is no reason that languages must change in a way to produce shared innovations, so evidence may simply be lacking. This seems to be the case not only for many Australian groups, but also for such ones as the Cushitic group studied by Black (1974b).

### 3. LEXICOSTATISTICS

Lexicostatistics seems to provide considerably more evidence for subgrouping Australian languages than we have found from shared innovations. For example, even lexicostatistical percentages based on just 100-item word lists, collected by Hale for thirty varieties spoken on Cape York Peninsula, provide reasonably clear evidence for Northern Paman and other putative subgroups (Black 1974a). Interestingly, these percentages also provide somewhat weaker evidence for a higher level grouping of Northern and Middle Paman, as suggested by the evidence of shared innovations. It is not really surprising that lexicostatistics provides evidence for subgrouping where shared innovations do not: lexicostatistical percentages can always be calculated, and unless they are uniformly low or very unusual they will support some subgrouping hypothesis or another. The question one must ask is how reliable this evidence is.

Much of the evidence for the reliability of lexicostatistics is based on the simplest and most common approach, which goes back to Swadesh (1950, 1952); see also Hymes (1960) for a survey and Dyen (1975) for a variety of relevant papers. The approach starts with determining, for each language variety being considered, the form(s) that most commonly represent each meaning on a set list of perhaps one or two hundred relatively 'basic' meanings. Then, for each pair of language varieties, a judgement is made as to which meanings are represented by cognate words in the two languages and which are not. A lexicostatistical percentage is then calculated as  $100(c/c+n)$ , where  $c$  is the number of meanings judged to have cognate forms and  $n$  is the number judged not to have cognate forms (meanings for which no decision can be made are ignored). The percentages are then interpreted as indices of relative similarity and as such taken as evidence for subgrouping. Although applications in the literature have used a variety of procedures, both appropriate (e.g. Dyen 1962) and inappropriate, to determine a subgrouping from the percentages, perhaps it is better to view the process as one of deciding which subgrouping hypothesis is best supported by the percentages; see §4.3 and §4.5.

Considerably more advanced approaches to lexicostatistics have been investigated, for example, by Dyen, James and Cole (1967) and by Kruskal, Dyen and Black (1971, 1973). However, as Embleton (1986:66) puts it:

An important and perhaps surprising result is that these more complex models behave remarkably similarly to the simpler ones, partially justifying some of the earlier (and often implicit) assumptions, which should therefore no longer be challenged as being totally unrealistic.

Four types of evidence have been found to support the validity and general usefulness of the common approach to lexicostatistics:

- (a) Evidence that vocabulary tends to change at a reasonably constant rate was first published by Lees (1953), who used historical records on thirteen languages (eleven

Indo-European plus Coptic and Chinese) to estimate that about 19% of 215 relatively 'basic' meanings underwent lexical replacement each one thousand years. This rate tended to be confirmed in studies of Japanese, Carib and Arabic varieties (see Hymes 1960:12–13), but it was questioned by Bergsland (1958) and Bergsland and Vogt (1962), who found strikingly lower rates of replacement for Icelandic and—using eighth and tenth century manuscripts to represent the fifth century language—Georgian. Blust (1981) is said to have provided evidence of some sort (presumably not based on historical records) suggesting highly variable retention rates among Austronesian languages, but I have not yet been able to examine this paper.

It has long been known that the use of different languages within the same community often leads to heavy borrowing (Bloomfield 1933:461–475), and Pawley and Ross (1993:448–452) have recently proposed that it can also account for why some Austronesian languages seem far less conservative than others. One may accordingly wonder if the conservative nature of Icelandic relates to its relative isolation, rather than (or in addition to) the prevalence of literacy, as has sometimes been supposed. Although this may suggest that lexical replacement rates can be affected considerably by contact, presumably the effects are relatively short lived, as the languages that may have had an initially great impact on each other achieve some sort of equilibrium. There also seems to be no reason to suppose that such intimate contact was significantly greater in parts of Oceania than in Europe, whose historical complications are well known. It may thus be quite normal for languages to undergo such contact effects for just a few centuries now and then, and yet over the course of several millennia the effects on replacement rate need not be great.

- (b) The theoretical basis for expecting vocabulary change to be regular was clarified by Sankoff (1969; see also 1973), who showed that it could follow from stochastic variation in the frequency with which particular words represented particular meanings. Even though all our word choices may be well motivated as we speak, statistical regularity can be seen in the overall pattern of choices, just as it can be seen in such things as patterns of transportation or telephone use even though these also ultimately involve individually well-motivated decisions.
- (c) Carefully produced lexicostatistical classifications tend to agree with ones based on other evidence, although the correspondence is not always perfect. In particular, Dyen, Kruskal and Black (1992) found that a lexicostatistical classification of eighty-four contemporary varieties of Indo-European agreed in most respects with the most commonly accepted classification. Icelandic certainly proved unproblematic, despite its unusually low rate of lexical replacement (see (a) above). The most striking disagreement was that the lexicostatistical classification failed to find evidence for grouping the Indic and Iranian languages into an Indo-Iranian branch. However, this grouping is well established on the evidence of such ancient varieties as Sanskrit and Old Persian; it is not clear that it could be established on the basis of the contemporary varieties used in the lexicostatistical study, or that applying lexicostatistics to the ancient varieties would fail to provide evidence for the grouping.
- (d) Computer simulation of language divergence and its reconstruction (Sankoff 1969; Guy 1980:28–31; Embleton 1986:79–96) suggests that a lexicostatistical subgrouping based on two hundred meanings in ten languages or so will be fully accurate less than half the time; that is, more likely than not it will contain at least one mistake. Even so, Embleton's work makes it clear that these mistakes tend to be quite limited. Using

another measure she found correlation of nearly 0.97 (out of 1.00) between the actual tree and the lexicostatistical subgrouping for a model involving standard assumptions (a 19% replacement rate and no borrowing), or ranging between 0.85 and 0.97 for a model involving other retention rates and allowing for a borrowing rate of up to 30%. The same studies show that accuracy decreases significantly as the number of meanings is cut to one hundred or less. Some of Guy's (1980:9–10) criticisms of the common approach to lexicostatistics are based on a study involving as few as forty meanings, which not surprisingly produced results ranging from "poor...to incredibly bad".

Clearly lexicostatistics is imperfect. Perhaps it is too imprecise to be a worthwhile basis for estimating actual divergence times—an application known as *glottochronology*—especially since it is not clear that the rates of change observed for languages with long written traditions need hold for unwritten languages. For subgrouping purposes, however, its imperfections seem small enough to live with, especially considering that no other approach has yet provided much evidence at all for classifying Australian languages.

It is not surprising that we have no single reliable means of making inferences about prehistory. In such a case it seems best to consider all the evidence available. Lexicostatistics and the traditional use of the evidence of shared innovations complement each other in a valuable way because they are based on quite different sets of assumptions. When we are lucky enough to find that both approaches support the same results, we can be very confident that we are on the right track.

#### 4. DO AUSTRALIAN LANGUAGES HAVE SPECIAL PROBLEMS?

To the extent that some of the best evidence in support of lexicostatistics is based on Indo-European languages, one may wonder if it is equally applicable to Australian languages in spite of cultural and linguistic differences (e.g. word taboo practices, widespread multilingualism, an abundance of synonyms) and relatively small population sizes. At one point Dixon (1970, 1972:331–337) went so far as to propose an alternative lexicostatistical model based on the assumption that Australian languages normally borrowed so heavily from their neighbours that adjacent languages tended toward an equilibrium level of 50% shared vocabulary. Let's review some problems that have been proposed in connection with name taboo and the rates of lexical change and borrowing to see how compelling they are.

##### 4.1 NAME TABOO

The debate about the possible effects of name taboo on lexicostatistical investigation is fairly old; see, for example, Bergsland (1958:655–656), Hymes (1960:8–9) and Bergsland and Vogt (1962:126–127), and see also Simons (1982) and Alpher and Nash (1981) with respect to Austronesian and Australian languages respectively. For Australian languages the notion that name taboo could affect lexical change is even older. Fraser (in Threlkeld 1892:xvi–xviii) stated that:

When a man or a woman dies, his family and the other members of the tribe, as far as possible, never mention his name again, and discontinue the use of those ordinary words which formed part of his name; other words are substituted for those common ones, and become permanently established in the daily language of the clan or sub-tribe to which the deceased belonged.

In a footnote to the above Fraser added that, "It is possible that the discarded word resumes its place in the language after a while; this point I have not ascertained; in all events the adopted word remains".

As described by Fraser, name taboo would seem likely to affect lexical change, but it is not clear how great the effect might be. If the 'discarded' words never returned it could promote massive lexical change, as has been proposed for the Tiwi language by both Pilling (1970:268) and Osborne (1974:5); we will return to this case later. If the main source of substitutes was through borrowing from nearby varieties, this borrowing would surely also be heavy, as Dixon (e.g. 1972:331) suggested. On the other hand, if 'discarded' forms generally did return, the main effect might be to increase the numbers of synonyms in the language (see also Heath 1979:409-410). For Western Desert communilects, in fact, Hansen (1984:8) has suggested that name taboo is one of the factors, along with intergroup movement and intermarriage, that has promoted an abundance of synonyms for even basic vocabulary. One might expect the availability of synonyms to make it unnecessary to borrow replacements for tabooed vocabulary, but in any case it may make the impact of further borrowing less significant, since a newly borrowed form generally just becomes one of several alternatives. To the extent that older synonyms may eventually be dropped from the language, of course, the end result may be no different than if the older form had immediately been replaced by an adopted form, but this need not imply especially rapid change or especially heavy borrowing.

What we now know about name taboo practices rules out at least the most extreme effects. Much of the following evidence was originally presented by Black (1979a):

- (a) Name taboo is or was clearly temporary in many Australian Aboriginal cultures, lasting from a few months (e.g. in Torres Strait) to many years (among the Tiwi). For Australian groups between Alice Springs and the Gulf of Carpentaria Spencer and Gillen (1904:526) noted that the taboo did not even extend to all members of the tribe. There is no clear evidence that name taboo was permanent for any Australian group, although some brief, early accounts may suggest this was the case.
- (b) For some groups, it is very clear that the names of deceased people are later reused. The Wik-Mungkan (McKnight 1975:86-87) and Yirr'-Yoront (Alpher and Nash in preparation) actually had ceremonies at which the name was returned to use a few years after the death. While studying the languages of the Kurtjar I found that people were often named after certain grandparents, and for Koko-Bera I even found a case of a nickname (*Thakathéw* 'for spit') being inherited along with the proper name. Roth (1903:20) and Haviland (1974:229) also report names that were still in use long after the first person they were known to refer to had passed away.
- (c) Words cited as examples of tabooed vocabulary have also been attested as still being in use in the language years later. For Tiwi, Hart's (1930:282) example of *mulikina* 'full' and Pilling's (1970:268-269) of *tartuwali* 'shark' are both listed in Osborne's (1974) study and in a more recent Tiwi dictionary. In discussing the 'Maroura', Holden (in Taplin 1879:21-23) noted that *Therto* 'head', *Konito* 'belly', and *Muna* 'hand' had been tabooed, and that the word for 'water', which he lists elsewhere as *Nucko*, had changed five times in recent years. These forms were subsequently attested in Curr (1886, II:239-240) as *thirtoo*, *korntoo* ('stomach'), *murra*, and *ngookoo* 'water' respectively; I suspect that Holden's *Muna* was simply a typo for *Murra*. Some fifty years later Tindale

(1939) similarly attested *ku:ntoi* 'belly' and *ʔok:o* 'water'. For other examples of proscribed vocabulary returning to use see Capell (1963:A-11) and Dixon (1980:28).

- (d) There are a variety of sources for replacing tabooed vocabulary, including the use of resources already in the language as well as borrowing from another variety well known to the community (see Dixon 1980:99). The former include synonyms, use of auxiliary vocabulary (see Capell 1963:A-11), compounding or paraphrase (see Douglas 1971), semantic shift (see Dixon 1980:122), and special vocabulary, sometimes glossed as 'no-name', whose only function is to serve as a temporary substitute (see Nash & Simpson 1981). The special substitutes can be found across the continent:

Ngarluma *Joocurree* (Hall 1971:30) or *Djugari* (Gray 1976:148);

Manjilyjarra *kunmarnu* (Patrick McConvell pers. comm.);

Gurindji *yini murlung* (Nash & Simpson 1981);

Mudbura *kulu murlung* or *mijingu* (Nash & Simpson 1981);

Warlpiri, Warlmanpa, Warumungu *kumunjayi* (Nash & Simpson 1981);

Pintupi/Lurija *kunmanarranya*, *kunmanytjayi*, *kunmanu* (Hansen & Hansen 1977);

Mangarayi *gaji* (Francesca Merlan pers. comm.);

Kurtjar *rdookirbharr* (general), *rdookirchontik* (place), *tamark* (namesake) (Black & Gilbert 1986);

Koko-Bera *ngəlpəngənpələy* and *ngəlpərrəny* (author's data);

Olgol *arambat* (Sommer 1976:231);

Yirr'-Yoront *wolonn*, *wall-warrch*, *mer-warrch* (depending on the speaker's relationship to the deceased; Alpher & Nash 1981);

Wik-Mungkan *kootamāta* (for a man's name), *nhamparriya* (for a woman's name) and *Weenana* (for a place) as reported by Sutton (1978); *yukaino* (for a namesake) and *wätjänänn* (other) as reported by Thomson (1946);

'on the Bloomfield [River]' *Tanyu* (namesake of the deceased; Roth 1903:20);

'Chepara' *Warkumbul* and *Waimungan* (for male and female family names; Howitt 1904).

Since name taboo was generally temporary, and since many languages used substitutes which were also clearly temporary, this cultural practice alone need not have had much effect on lexical change in Australia. One may, of course, imagine that there may be other reasons for rapid lexical change or for heavy borrowing, and we will consider these two matters further.

#### 4.2 THE RATE OF LEXICAL CHANGE

The 40,000-odd years that people have lived in Australia provides ample time for their languages to have diverged to a point where no relationship can be recognised. Even so, similarities in lexicon and grammar are apparent across the continent. On the one hand this suggests that many Australian languages may be descended from a common protolanguage far more recent than 40,000 years ago, and on the other that there is no particular reason to



expect lexical change to be especially rapid in Australia. If lexical change were very rapid, it would be difficult to explain why many basic meanings (e.g. basic verbs, body parts) are represented by similar forms in languages across the continent. The similarity can be attributed to borrowing only if it is assumed that the more basic forms were borrowed more widely than less basic ones, a situation opposite any known case elsewhere in the world.

The alternative possibility that lexical change is unusually rapid in Australia certainly has not been apparent from the comparison of earlier and later attestations of languages. O'Grady, Voegelin and Voegelin (1966:26) noted that a list of vocabulary obtained for Parnkalla in 1960 agreed almost totally with one published in 1844. Similarly Haviland (1974:231) found that most of the Guugu Yimidhirr words published by Cook in 1770 remained "completely recognisable today"; Alpher and Nash (1981) estimate the retention of vocabulary over the two hundred years as being between 89% (if Cook was fully accurate) to 98% (if plausible explanations of error are accepted).

The best candidate for having exceptionally rapid lexical change is Tiwi, since it seems quite different from other Australian languages (although this could also be due to its isolation on Bathurst and Melville Islands) and because its speakers have had especially long-lasting taboos against using words resembling the names of the deceased. In view of the latter Osborne (1974:5, fn.8) proposed that, "It is quite likely that under these conditions the complete vocabulary of a language would be replaced in the course of several centuries".

Change in Tiwi has not been nearly as rapid as Osborne suggested, however. This can be seen by comparing Osborne's (1974) attestation (or more recent data) with that of Spencer (1914:464-476). Black (1979c) found that out of 227 forms in Spencer having meanings for which Osborne also attested forms, 139 (60.5%) are straightforward matches, 58 (25.5%) involve minor differences in pronunciation or glossing (e.g. Spencer's *tua* 'enough' to Osborne's *tua* 'finished') 16 (7%) seem to involve mistakes in glossing (e.g. Spencer's *wailàni* 'breath, air' is similar to Osborne's form for 'yawn', which suggests how Spencer might have elicited his form), and only 16 (7%) cannot be matched up.

The last sixteen may well involve other errors or perhaps dialectal differences; they include (minus certain diacritics) *wianabàni* 'after' (cf. Osborne's *waija* 'now'), *punkìnyi* 'before', *iri ùmkeir um bùlunga* 'cheek', *pùndua* 'middle', *djidìngurin* 'man', *wunmali* 'mud' (second of two forms), *àmìna pùra* 'moon', *gnabàiya jera* 'often', *auinterabìli* 'pleased, glad', *ubùрма gamu* 'singing', *wiinigàni* 'then', *ninnèi* 'there', *anàba* 'wait', *kaùpo* 'water (fresh)' (second of two forms), *upaurilìridi* 'weak' and *wòrkai* 'What for? Why? What is the matter? What do you want?' Readers can decide for themselves to what extent such forms from an early attestation should be considered evidence for rapid lexical change.

There could seem to be somewhat better evidence for rapid lexical change in a paper by Lithgow (1973) on the Muyuw language of the Milne Bay District in Papua New Guinea. By comparing the speech of older speakers—said to speak *Ag-wuliwel* 'the language of the old people'—with that of younger ones he found reason to believe that 27 forms on the Swadesh 100-item lexicostatistical test list had become or were becoming replaced by new forms, 13 of which were not cognate with the older forms.

It turns out, however, that many of the forms used by the younger speakers were already reported for a Muyuw dialect (see Lithgow 1976) attested under the name 'Muroa' in the British New Guinea Annual Report of 1889-90 (pp. 148-152). Of the 27 forms that Lithgow

(1973) believed had been replaced or were undergoing replacement, Black (1980b) found that the 1889-90 data was clearly closest to the 'Current Speech' in 15 instances and possibly closest in two more, whereas it was closer to the 'Old Speech' in only four or five instances. Of the other five forms, the older source lacked information on three forms and had forms similar to both the 'Old' and 'Current Speech' forms for the other two. The data thus suggests that something very close to 'Current Speech' was spoken before many of the speakers of the 'Old Speech' were born.

Lithgow (pers. comm., 17 September 1980) kindly furnished me with further details on the fascinating complexities of the situation. It seems inappropriate for me to repeat them here (I hope he has published them elsewhere), but at best they suggested that the case for rapid lexical change rests on a bit of hair splitting. Even if Lithgow's 'Old Speech' forms should represent an earlier, unattested stage of the dialect he was studying, one might wonder why the use of forms from another dialect should not be viewed as a shift in dialect allegiance rather than heavy borrowing—what would make the difference?

#### 4.3 THE PROBLEM OF BORROWING AND ITS DETECTION

It is not difficult to find examples of noticeably heavy borrowing among Australian languages, whether or not this has anything to do with name taboo. What may not be realised is that such cases are found throughout the world, and even among the Indo-European languages dealt with in the lexicostatistical study by Dyen, Kruskal and Black (1992). For example, Albanian is believed to have borrowed all but a few hundred words of its entire vocabulary, and English borrowed over half of its lexicon from other languages; see the chapter on intimate borrowing in Bloomfield (1933:461-475). For an example of heavy borrowing among Cushitic languages, see Black (1976b).

When we can identify borrowings, as we often can in Indo-European, we can avoid counting them wrongly as cognates for lexicostatistical purposes. When we cannot distinguish borrowings from cognates, on the other hand, heavy borrowing can have a marked effect on lexicostatistical percentages. By the same token, however, the effect itself often enables us to detect the borrowing and make allowances for it, as Dyen (1963) demonstrated.

To see how this works, consider how lexicostatistical percentages are used to establish a subgrouping. Example (1) shows lexicostatistical percentages among three languages of the Torres Strait area, Gudang and Yadhaykeno on Cape York and Meriam Mir of eastern Torres Strait. (The percentages were calculated rather quickly (Black 1979b) on the basis of an 88-item list of meanings. Like other percentages cited in this section their purpose is simply to illustrate certain principles, rather than to present authoritative information on the languages themselves; the latter is best left to specialists working with the best sources of data.)

(1) Yadhaykeno	YAD				
Gudang	49	GUD			
Meriam Mir	4	7	MER		

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graph TD
    Root(( )) --- YAD
    Root --- Node1(( ))
    Node1 --- GUD
    Node1 --- MER
  
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From these percentages it should be clear that Yadhaykeno and Gudang form a group at 49% shared cognates, and that both of them are much more distantly related (if related at all) to Meriam Mir, with which they seem to share 4% and 7% cognates respectively. We can accordingly represent their relationships (if Meriam Mir is indeed related) by the tree diagram

shown on the right. Note that this tree diagram, and the chronological sequences of linguistic splits that it represents, implies that the relationship of Yadhaykeno to Meriam Mir should be exactly the same as that of Gudang to Meriam Mir. By the same token Meriam Mir should share about the same percentage (allowing for statistical variation) with both Yadhaykeno and Gudang. And indeed it does, since the difference between 4% and 7% is not statistically significant.

More generally, for any three linguistic varieties that can be classified appropriately in a tree diagram, the two lowest lexicostatistical percentages (or any other measure of similarity) should be approximately equal. (The third percentage may or may not be higher, depending on whether or not two of the varieties form a subgroup excluding the third.) This pattern often does not hold for dialects of a single language, whose relationships are not well represented by tree diagrams (see Black 1976a, Dobson and Black 1979), but it should hold increasingly well as distinct languages undergo 'independent' development (as this is traditionally viewed). Accordingly, where the pattern does not hold for more divergent varieties, we can tell that something is amiss. Consider example (2), which adds other languages of the Torres Strait area to those of example (1):

(2) Yadhaykeno	<u>YAD</u>			
Gudang	49	<u>GUD</u>		
Mabuiag	9	23	<u>MAB</u>	
Meriam Mir	4	7	14	<u>MER</u>
Kunini	1	4	5	24 <u>KUN</u>

Clearly Yadhaykeno groups with Gudang at 49%, as before, but notice that their percentages with Mabuiag, of western Torres Strait, differ considerably: only 9% for Yadhaykeno but 23% for Gudang. Either the former percentage is depressed somehow, or the latter inflated considerably; the former is not impossible (see Dyen 1963), but the latter can easily be due to undetected borrowing. (Contaminated data is another possibility, since the Gudang data was from an early attestation.) Accordingly these percentages do not provide any good reason to group Mabuiag with the Yadhaykeno-Gudang groups, since its relationship to these is better represented by the 9% (or something not much higher) than by the 23%, and since 9% is not much higher than the 1% to 7% Yadhaykeno and Gudang share with the remaining languages. (There are other types of evidence suggesting that such a grouping could be correct, but the relationship may be too remote to be apparent from our lexicostatistical evidence.)

To continue with example (2), it seems that Meriam Mir groups with Kunini, of the Papua New Guinea mainland, at 24%. Accordingly we would also expect these two languages to share about the same percentages with Mabuiag, but again they differ, and again it seems likely that the 14% between Meriam Mir and Mabuiag may be inflated by borrowing, and that the 5% between Kunini and Mabuiag is closer to representing the actual degree of relationship.

To summarise my approach to subgrouping, I am simply seeking the genetic tree that is best supported by the percentages. In many cases, such as those above, the appropriate trees are fairly obvious, but where the data is less clear one could apply a mathematical measure of goodness of fit in order to evaluate the alternative trees. In any case, any such tree diagram will imply that certain of the percentages should be about equal, and significant deviation

from such equality may suggest that some of the percentages have been inflated by undetected borrowing.<sup>1</sup>

#### 4.4 A SECOND WAY OF DETECTING BORROWING

There is actually another way of getting a general idea of the amount of borrowing between two varieties. It seems clear that vocabulary varies considerably in stability, with words for some meanings (e.g. 'see', 'long', 'hand') tending to be far more stable than words for others (e.g. 'stretch', 'smooth', 'ghost gum'). Comparative linguists often distinguish these as more and less 'basic' vocabulary respectively, and in investigating genetic relationships they tend to trust the more basic vocabulary, viewing the less basic as more prone to borrowing. The distinction seems to correlate with frequency of use, which, as you may recall, was also the basis for Sankoff's (1969) model of lexical change. In any case it also has a practical application. As the frequencies  $f$  of meanings decrease we expect the retention rate  $r$  for cognates to also decrease while the rate of borrowing  $b$  increases (if the situation is conducive to borrowing), as shown in Figure 1; see Arapov and Kherts (1972) for evidence of this in the recent histories of several European languages.

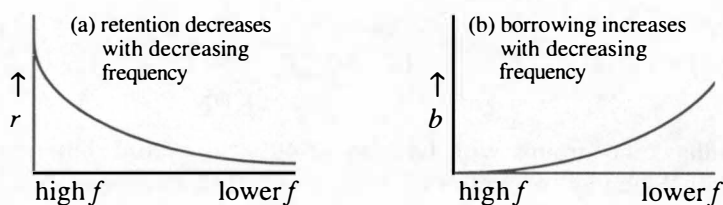


FIGURE 1: RETENTION  $r$  AND BORROWING  $b$  VERSUS MEANING FREQUENCY  $f$

These patterns will not be apparent from a single set of lexicostatistical statistical percentages, but there are other ways to use them. For example, consider the eastern and western Torres Strait languages, Meriam Mir and Mabuiag, whose common percentage (in example (2)) appeared to be inflated by borrowing. We do not have data on word frequency in these languages, but we can fall back on the notion of more and less 'basic' vocabulary. In particular, using Ray's (1907) conveniently parallel word lists for the two languages I made a count of related forms (i.e. without trying to distinguish cognates from borrowings) among 395 meanings divided into the following four groups, which are presumed to range from most basic to least basic:

<sup>1</sup> It may be noted that Guy (1980) has proposed using correlations between the full sets of percentages for each pair of varieties as a basis for subgrouping. His approach seems to work well in computer simulations that involve varying rates of change, but it is not clear how well it would cope with undetected borrowings, since these can be expected to detract from the correlations that should provide a basis for determining the proper subgrouping. Guy's (1980:27) view of borrowing is quite different from commonly held views: he proposes that detected loanwords be treated as missing data, rather than as non-cognates, as is commonly done because they do indeed represent instances in which originally cognate forms have been replaced by non-cognates. Guy's explanation is rather cryptic:

To score them as non-cognates (or as cognates for that matter) is like letting scratches on a record influence one's appreciation of the performance of a musical work. Loanwords should be treated as missing items, for that is precisely what they are, as scratches on a record are bits missing from the original pressing.

Vocabulary Group	Related forms
a) 13 closed-class grammatical forms	0%
b) 82 basic nouns, verbs and adjectives from a 100-item list	11%
c) 73 other 'basic' words	12%
d) 227 terms for species names and cultural terms	27%

The fact that the percentage grows as the vocabulary becomes less basic and more cultural would certainly seem to confirm the heavy borrowing suggested by the percentages in example (2).

A similar pattern can be seen in a comparison between the Ngandi and Ritharngu of eastern Arnhem Land, which Heath (1978a) presented as a case of extremely heavy borrowing. Fortunately Heath's (1978b) Ngandi vocabulary usually notes related Ritharngu forms, and so it is easy to study their distribution. Leaving aside the closed-class forms, which are not listed in his vocabulary, the percentage of related forms rises from 33% for the 76 'most basic' forms to 42% for 77 other 'basic' forms to 57% for 228 species names and cultural terms.

To make it clear that this pattern is due to borrowing, let us consider another case. Example (3) shows lexicostatic percentages among the Oykangand, Koko-Bera and Kurtjar languages of western Cape York Peninsula, based on a 'basic' word list of 158-165 items for each pair of varieties.

(3) Oykangand OYK

Koko-Bera	21	<u>KOK</u>
Kurtjar	15	20 <u>KUR</u>

The differences among the three percentages are not statistically significant. These differences suggest that there could have been borrowing between Koko-Bera and either Oykangand or Kurtjar or both, but it doesn't really matter, because even if we knew that either the 20% or the 21% was inflated by borrowing, the other is not really different enough from the 15% to allow us to confidently propose a subgrouping.

In any case, however, consider how the percentages of related forms vary over the same four types of vocabulary distinguished earlier. The following table gives data for all three pairs of languages, using the abbreviations introduced in example (3):

Vocabulary Group	OYK-KOK	OYK-KUR	KOK-KUR
a) 14-15 closed-class forms	60%	57%	50%
b) 79-82 'most basic'	26%	16%	24%
c) 65-68 'other basic'	8%	6%	9%
d) 80-159 'least basic'	29%	8%	19%

The middle column, for Oykangand and Kurtjar, is what you would expect when borrowing was not heavy: the percentages tend to decrease from more to least basic vocabulary. For the other two columns the same pattern is seen for the first three categories, but the percentage rises in category (d), suggesting that there has been significant borrowing, but not enough to have had much affect on the pattern of more basic vocabulary.

(Actually some effect on the latter is visible in category (b). In the same way that example (3) suggests that there was borrowing, the fact that both the OYK-KOK and KOK-KUR

category (b) percentages are eight to ten percentage points higher than the 16% of OYK–KUR suggests that one or both of the former may be inflated by borrowing. That this is not also apparent from category (c) suggests that my division into ‘more basic’ and ‘other basic’ is less than perfect. If the categorisation were more perfect, one might argue that the borrowings in category (b) should be well under 9%, since that is the highest percentage in category (c.)

#### 4.5 A MORE EXTENSIVE EXAMPLE

Since the above cases were chosen to illustrate certain problems, they may leave you wondering if all lexicostatistical work on Australian languages is highly problematic. To balance the picture, consider the relations among Tryon’s (1974) so-called ‘Daly Family’ languages. Table 1 displays two sets of percentages: in the lower left are those calculated by Tryon (1974:xiv) for all pairs of varieties, whereas in the upper right are my own percentages between certain selected pairs. Tryon’s percentages clearly support the subgrouping shown across the top of the table: a Mulluk Group containing Mulluk and Daly Subgroups, a Wogaity grouping, what I’ve called an ‘apparent Brinkin–Maranunggu Group’ containing the Maranunggu and Brinken Subgroups, and a Tyemeri grouping. Of these, Tryon (1974:xiii) himself did not recognise the Brinken–Maranunggu Group, but instead proposed a Brinkin–Wogaity Group that included the Wogaity grouping as a third member in addition to the Maranunggu and Brinken Subgroups.

To locate the more problematic aspects of the classification, let us consider how we can use Tryon’s percentages to subgroup the languages. To start the subgrouping we consider the highest percentages, which appear along the diagonal due to the way I’ve arranged the varieties. As you use these to group varieties together, you are claiming that their relationships with outside varieties should be about the same—in terms of Table 1, that the percentages in the rectangles off the diagonal should be about the same. To the extent that this does not happen, the mathematical fit becomes poor.

It turns out that there are no problems in using the percentages enclosed in the boxes with the heaviest lines to establish the following groupings: the Mulluk and Daly Subgroups, Wogaity, the Maranunggu Subgroup, the grouping of the middle four varieties within the Brinken Subgroup, and Tyemeri. It is also no problem to establish a Mulluk Group in view of the percentages of 41% to 48% between the Mulluk and Daly varieties (in the rectangle off the diagonal). Adding Maramanandji (MMN) and Maregar (MNG) to the Brinken Group is also well indicated, even though the percentages (in the larger, thin-lined box around the diagonal) vary from 70% down to 54%. This variation could perhaps be the remains of dialect variation that does not lend itself well to being represented in a tree diagram (see Black 1976a).

The grouping of the Maranunggu and Brinken Subgroups into a Brinken–Maranunggu Group is also straightforward, even though it was not recognised by Tryon. Although the percentages between the two subgroups range from 34 to 51, some of the higher percentages are between adjacent varieties (this is indicated by the underlining) and could well be inflated by borrowing. Whereas some of the higher percentages are not between adjacent varieties, for one variety to borrow from a second affects its percentages with all varieties that are very similar to the second.

TABLE 1: LEXICOSTATISTICAL EVIDENCE FOR DALY RIVER LANGUAGE CLASSIFICATION (BASED PARTLY ON TRYON 1974)

		Mulluk Group			Wogaity	apparent Brinken–Maranunggu Group						Tyemeri							
		Mulluk Subgroup		Daly Subgroup			Maranunggu Subgroup			Brinken Subgroup									
		MLK	TYR	KMR	MTN	YNG	PNG	WDY	AMI	MND	MNN	MMN	MDN	MTH	MAM	MTY	MNG	NGA	NGE
Mulukmulluk	= MLK			30		12.5				13	9		10				9	9	
Tyeraity	= TYR	65																	
Matngala	= MTN	45	41			13				13	7.5		9				8	6	
Yunggor	= YNG	48	45	80											<i>Box D</i>				
Kamor	= KMR	42	40	75	72	<i>Box A</i>													
Pungupungu	= PNG	33	36	35	33	35					15	7		9			9	4	
Wadyiginy	= WDY	28	35	29	30	28	79								<i>Box B</i>				
Ami	= AMI	24	30	27	25	25	34	39										<i>Box C</i>	
Manda	= MND	21	28	26	30	26	34	33	86										
Maranunggu	= MNN	27	26	29	29	29	37	32	72	72		51		49			38	17	
Maramanandji	= MMN	14	19	18	20	20	20	17	40	41	46			79			62	24	
Maridan	= MDN	16	15	19	16	20	22	18	43	45	47	64							
Marithiel	= MTH	16	17	21	20	21	22	20	44	45	51	63	89				75	26	
MareAmmu	= MAM	16	16	18	18	18	23	19	42	45	48	58	84	88					
Marityabin	= MTY	15	15	19	17	18	21	18	42	44	44	56	83	84	85				
Marengar	= MNG	15	16	18	17	18	19	16	35	34	39	54	67	68	70	68		19	
Ngangikurrunggurr	= NGA	22	19	20	19	20	20	18	30	29	30	32	35	32	33	31	35		
Ngengomeri	= NGE	16	18	18	19	20	21	18	27	23	30	34	34	33	34	30	35	84	

From Tryon's percentages alone a problem emerges. The lowest percentages in the table, well off the diagonal, tend to be between 15% and 20%. The Wogaity grouping shares much higher percentages with both the Mulluk Group languages (28% to 36%) and the Maranunggu Subgroup (32% to 39%). At least the latter seem to be inflated by borrowing, because the Maranunggu Subgroup is only one part of Brinken–Maranunggu, and Wogaity's percentages with the other part, the Brinken Subgroup, range from 16% to no higher than 23%. Does Wogaity form a higher level grouping with the Mulluk Group, or are those percentages inflated by borrowing too? (As the underlining shows, many pairs of varieties are adjacent.)

There also seems to be some evidence that Tymeri may form a higher level grouping with Brinken–Maranunggu, but again one may wonder if the shared percentages (in the long rectangle at the bottom) are simply inflated by borrowing: at 23% to 35% they are not even as high as the questionable percentages between Wogaity and the Maranunggu Subgroup.

Tryon's percentages are based on a 200-item word list that includes about forty items of less basic vocabulary, including names of plant and animal species, terms for material culture, and even the words for 'policeman' and 'tobacco', which can hardly be traced back to a common protolanguage. Thus it's not really surprising if the percentages are often inflated by borrowing. To allow for this I calculated percentages for just a sample of varieties, essentially one from each group, using only about a hundred of the more basic items; these are the percentages in the upper right hand half of Table 1. From these it seems clear that Wogaity should not be grouped with either the Mulluk or Brinken–Maranunggu Groups: its highest percentages are only 12.5% to 15% with either of these groups (in Boxes A and B respectively), which are not significantly higher than the 13% shared by the Mulluk and Brinken–Maranunggu Groups themselves (in Box D). Since most of these percentages are between adjacent languages, they may well be inflated by borrowing.

There is a somewhat better case for grouping Tymeri with Brinken–Maranunggu. The percentages between the two groups (in Box C) are noticeably higher than their percentages with other groups (in Boxes B and D). Even if the higher percentages are inflated by borrowing, the 17% between non-adjacent varieties (MMN and NGA) is still well above the 4% to 10% that the two groups generally share with the outside languages.

It should also be noted that this evidence does not actually support a grouping of all of the varieties into Tryon's 'Daly Family'. Whereas the lowest of my percentages among these languages ranges from 4% to 10%, I found that the same varieties shared from 1% to 6% with Koko-Bera, halfway across the continent. Tryon's own lowest percentages are much higher, and in an earlier work (Tryon 1968) he found that the lowest percentages among the languages tended to be noticeably higher than their percentages with three other languages of the area, namely Warrai, Kungarakany and Wageman. In both cases, however, the percentages that might seem to establish a 'Daly River Family' could well be inflated by borrowing.

This points out another problem with applying lexicostatistics to Australian languages. Our current lexicostatistical techniques work well for groups with a time depth not much greater than Indo-European. From the fact that lexicostatistical percentages as low as 8% have been found among Pama-Nyungan languages, it seems likely that many relationships will simply prove to be too remote for lexicostatistics to provide useful results, at least without substantial further development in the methodology. Even so, lexicostatistics should



be able to provide useful evidence in many cases, as the Daly River example should demonstrate.

## 5. PROSPECTS

It thus seems that lexicostatistics remains one of our most promising means for realising O'Grady's early goal of classifying all Australian languages genetically. Although some have suggested that Australian languages change more rapidly or borrow more heavily than languages elsewhere in the world, this is not supported by the available evidence. At the same time, it seems clear that lexicostatistics can provide useful evidence for linguistics in such cases as the study of the 'Daly Family' languages, at least if the approach is well applied. Lexicostatistics is not infallible, but neither is any other approach, so that the ideal is to apply different approaches to permit cross-checking.

The main disadvantage of lexicostatistics is that current approaches tend to be limited to a shorter time depth than is probably needed for Australia as a whole. At the same time lexicostatistics has a number of advantages. Up to the limits in time depth it will almost always provide useful evidence for classification, whereas the traditional use of shared innovations does not always provide such evidence. It can generally be applied more easily and quickly than qualitative methods involving reconstructions, though this should not be taken to mean that it should not be applied as carefully as possible. In addition, as I have shown, lexicostatistical techniques can not only cope with cases of heavy borrowing, but they can actually provide evidence for them.

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