Another Look at Australia as a Linguistic Area
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1. Introduction

Australia has been described as an ancient and complex linguistic area, with few recoverable genetic groups (for example Dixon 1997, 2001, 2002). Evidence cited in support of the areal model of Australian linguistic relationships includes: the lack of bunching isoglosses for major typological features such as prefixation, bound pronouns, noun classes, and phonological changes; the difficulty of classifying the Pama-Nyungan daughter languages into discrete subgroups; the apparent time-depth of settlement of the continent (at least 50,000 years); and longstanding, widespread multilingualism.

There are, indeed, many isoglosses that cross-cross the continent, and progress has been slow in establishing subgrouping and completing appropriate reconstructions. However, I argue here that contrary to recent high-profile work on Australian languages, Australia is not a 'special case' where traditional methods are of no use. Moreover, I hope also to put aside the idea that the use of the terms 'genetic relationship' or 'subgroup' necessarily implies the ability to model the languages on a neat family tree. Finally, our lack of ability to model a given group of languages on a family tree does not necessarily imply a great time depth to the group.

In this paper I begin by giving a brief overview of the assumptions made in the punctuated equilibrium model. In Section 3 I summarize the arguments given to account for why more traditional models are assumed not to work in Australia. In Section 4 I summarize some of the major problems in applying punctuated equilibrium in Australia, and finally in Section 5 I present an alternative scenario which account for the facts and the patterns we see.[1]

2. Overview of punctuated equilibrium

In this section I concentrate on the major features of punctuated equilibrium as a model of language change. I will discuss the notions of 'punctuation' and

[1] This is a summary version of a much longer paper in progress, which will be included in a book on linguistic areas edited by Yaron Matras, April McMahon, and Nigel Vincent.

'equilibrium' and the problems with applying these notions to language change, and linking language change to non-linguistic events in a systematic way. I will address Dixon's criticisms of the comparative method and the family tree model, and the 'special situation' of Australian languages.

2.1 General Summary

Dixon adapted the term 'punctuated equilibrium' from evolutionary biology (and specifically Eldredge and Gould 1972) to refer to his model in which language change can occur at vastly different rates over different periods of time.

In the model, language groups are assumed to have coexisted for most of human history without a great deal of disruption. During this time, languages continue to change, largely, Dixon (1997:71) implies, by borrowing from one another. In this way traits can diffuse across an area. Thus during equilibria local as well as larger linguistic areas are created, across boundaries of genetic families. The long periods of equilibrium are interspersed with much shorter periods of punctuation. Punctuation could be caused by several factors, either natural or manmade. The introduction of new technology, invasion, mass migration, fire or flood are some examples of punctuation. Dixon (1997:73) argues that it is only in times of punctuation that recognizable splits, of the kind well modeled by a family tree, occur.

A prediction of punctuated equilibrium is that languages in an equilibrium situation will 'converge to a common prototype'; that is, they will tend to borrow lexical items and grammatical structures until they reach approximately 50% in common (Dixon 2002:26-30). According to Dixon, in Australia any lexical or grammatical component of a language can be borrowed between languages, and there is there is no universal that basic vocabulary is borrowed less than non-basic (although this may be the case in European languages).

Although Dixon challenges the applicability of the family tree model to Australian languages, to my knowledge he does not challenge the principle of the regularity of sound change or the applicability of the comparative method in broad terms, although he claims that the amount of borrowing between Australian languages make its application susceptible to false results. The comparative method is based on the assumption that internal language change is regular, and that regular, systematic correspondences between languages are meaningful and highly unlikely to be due to chance. What one does with this inference, however, is not strictly part of the comparative method.[2] The comparative method only relies on our assumption that languages employ regular sound change and exceptions to this require special pleading. If Australian languages could be shown to change sporadically, then this would mean that the comparative method could not be applied in Australia, although nowhere in Rise and Fall of Languages

[2] Nor, incidentally, is the comparative method necessarily related to the family tree; this point will be taken up further below. The comparative method can still be used, for example, within linguistic areas and in the analysis of loan words (for an Australian example see Koch 1997 on loan words between Arandic and Warlpiri), which is what makes it such a powerful tool.

(or elsewhere, to my knowledge) does Dixon challenge the idea of regularity of sound change in principle.

Dixon (1997:149-152) does attack the comparative method as a 'discovery procedure', lambasting those (such as Hoenigswald 1960 and Anttila 1989) that equate the comparative method to an automatic synchronic phonological analysis. He argues that there are many places where a strict application of the comparative method will yield the wrong result. He is of course right to attack such reconstructions, although one would be hard put to find a thorough, detailed and plausible reconstruction of any family using the comparative method that applied it blindly, without taking into consideration the likelihood of the changes to be reconstructed and the possibility of language contact. In reality, neither synchronic nor diachronic phonology is practiced as a mechanical discovery procedure. Problems like opacity are encountered, recognized, and dealt with in both synchronic and diachronic linguistics.

Dixon further criticizes the comparative method because it will over-generate in some cases. For example, if a change occurs in all the languages of a group independently, it may well be reconstructed to the ancestor of the group, giving a false common innovation. We cannot reconstruct states for which we have no evidence. This is, of course, a weakness of the comparative method; however, it is a problem with all reconstructions of the past — if evidence has been obliterated, we won't see it, no matter what the method used (carbon-dating, stratigraphy, thermo-luminescence, punctuated equilibrium, glottochronology). This isn't a valid criticism of the comparative method *per se*, it's a fact of prehistory.

Dixon's third criticism of the comparative method is that proto-languages look neat and uniform, whereas 'natural languages are rather like an old garment that has been patched and mended' Dixon (1997:45). Dixon implies here and elsewhere that the comparative method can only reconstruct regularity and that we can never reconstruct all aspects of a proto-language. I do not dispute that we will never be able to reconstruct all aspects of a proto language, but I do not agree that we can only reconstruct regularity. To take some established examples from Indo-European of areas we where can reconstruct irregularity: we can reconstruct certain irregularities in paradigms, such as the presence of full grades in locatives of ablauting root nouns despite the expected zero grade for oblique cases. We can reconstruct a vowel *a distinct from that of the sequence *h_2e (that is, the *a*-coloring laryngeal + *e*) despite their merger in all daughter languages: compare the behavior of the Greek root δαμάζω 'I tame', and its ø-grade passive participle δμᾱτός (< *damH-/dmH-tó-), which shows a 'real' *a* which deletes in the zero grade, with the reflex of the root *steh$_2$- 'stand' which does not lose the vowel but gives στατός, from *sth$_2$-tó-, with vocalization of the laryngeal. Another example of reconstructing irregularity involves the remodeling of forms of the nominative case in some paradigms, due to Szemerenyi's Law (involving compensatory lengthening over a resonant as the result of the loss of final *-*s*). There are numerous other minor 'rules' to deal *precisely* with the irregularities which we can reconstruct.

Another 'reconstruction of irregularity' which Dixon does not mention is loan-word analysis. It is precisely because these forms are irregular that we can identify them as being worthy of separate explanation. It is also possible to reconstruct loans into proto-languages, using with case the same methods which we use to identify loans in modern languages.[3] For example, it may be the case that the loan from B to A shows a different reconstructible accent pattern, an odd consonant cluster, a morpheme otherwise unidentifiable, or an odd root shape. Such criteria allow us to infer, for example, that Proto-Germanic *rīk(i)ja- (Gothic *reikeis*, Old High German *rihhi*, English *rich*) is a loan from Celtic. Comparison with Latin *rex* and Sanskrit *rāj-* imply a reconstruction **rēg-*, and Celtic exhibits a regular sound change **ē* > *i*, but this is not a regular change in Germanic. The irregular correspondence identifies the word as a probable loan. Thus instances where the comparative method fails can be as instructive as instances where it succeeds.

3. Punctuated equilibrium in Australia

Dixon (1997:91) regards Australia as a 'prototypical example of a long-time diffusion area'. Indeed, he states that his punctuated equilibrium model is the *only* way to account for the current distribution of Australian languages. In this section I outline the main arguments that Dixon presents, specifically applied to Australian languages.[4]

Dixon's views of several aspects of Australian languages are at variance from those of most other Australianists (as I understand them) and go some way to explaining his insistence on the lack of applicability of the family tree model in Australia. The main difference is the view that non-Pama-Nyungan languages are archaic in relation to Proto-Australian, and the non-Pama-Nyungan languages are innovative. This follows in part from the fact that Dixon's 2002 reconstructions follow general typological principles — that is, that languages tend to go from isolating to agglutinative to inflectional and back again; this is the basis for his view of the non-Pama-Nyungan languages as innovative and the Proto Pama-Nyungan 'area' as an archaic diffusion area. The Pama-Nyungan type is assumed to be the original type, and the non-Pama-Nyungan languages are assumed to have innovated from that type by cliticizing pronouns or catalysts to verbs to form inflectional bundles, as has also happened sporadically in Pama-Nyungan languages such as the Southern dialects of Baagandji (Hercus 1986).

Dixon also assumes that Australian languages have been evolving fairly much *in situ* from early on since the initial expansion into Australia (probably around 60,000 years ago). His reasons are partly to do with origin myths (cf Dixon 1996) and partly because, as he states, if Proto-Australian had been spoken only a few

[3] See, for example, Sammallahti 1998 on loans into Saamic languages from Indo-European.

[4] One cannot do justice to the full picture in few pages; the reader should consult Dixon (2002) for a much more detail account.

thousand years ago, the split and expansion would have happened rather recently, and a family tree model should be applicable (Dixon 1997:92).

Finally, Dixon presents two arguments that Pama-Nyungan languages do not form a genetic family. The first is that if Pama-Nyungan were a genetic group, we should be able to reconstruct innovations between Pama-Nyungan and higher level groups (e.g. Dixon 2001:93). The paucity of such reconstructions should cause us to question the genetic status of Pama-Nyungan itself. He further argues in the appendix of Dixon 2001 that many of the forms taken to be common retentions from earlier stages of Pama-Nyungan (for example the widespread first person dual pronoun *ngali*) should better be treated as diffusions across a large area. The wide-scale diffusion and convergence that Dixon argues for in punctuated equilibrium is also his explanation for the lack of progress in reconstructing subgroups of Pama-Nyungan and for the non-bunching isoglosses which criss-cross the continent.

4. Problems with punctuated equilibrium as applied to Australia

Dixon is correct to point out the lack of reliably-established groups and subgroups in Australia, however there are several problems with the arguments he deduces to claim that traditional methods do not work in Australia.

Much previous subgrouping has been based on lexicostatistics (especially that of O'Grady, Wurm and Hale 1966). The O'Grady, Wurm and Hale classification was based on little data (a 100-item list) and was never meant to stand as a final classification (Wurm 1972:109). It dates from a time when our basic knowledge of Australian languages was still very patchy – indeed, part of its purpose was a 'stock-take' of the languages still spoken. The O'Grady, Wurm and Hale classification, in much later Australian comparative work, has been either assumed or rejected out of hand, without proper evaluation by traditional methods. It is sometimes forgotten that it was a provisional schema, with each grouping requiring confirmation or correction by traditional historical-comparative methods. However, until recently detailed data have not been readily available to allow much accurate reconstruction to take place.

Dixon argues against the validity of a reconstructed language Proto-Pama-Nyungan partly on the grounds that one can reconstruct no innovations from a higher proto-language that set it apart from other Australian groups. One must realize, however, that demonstrating genetic relatedness and demonstrating membership of a subgroup are different things; for example, one does not need to believe in and reconstruct Proto-Nostratic in order to define Proto-Indo-European! It is true, of course, that if one views Proto-Pama-Nyungan as a subgroup of 'Proto-Australian', to justify this one must provide cognates from Proto-Australian and show innovations to Proto-Pama-Nyungan, but it is not necessary to do this to show the cohesiveness of a Pama-Nyungan family of itself.

Finally, let us consider the arguments that Dixon uses to claim that Pama-Nyungan is an ancient linguistic area. Part of the argument rests on the use of origin myths and oral history (e.g. Dixon 1996) which, if true, would tie certain

Pama-Nyungan groups to particular areas for at least the last 10,000 years. It is worth pointing out in general terms some of the problems with using origin myths and other types of myths to date population expansions and other aspects of prehistory. Take as an example the following story about a volcanic eruption, told in Dyirbal to Dixon in the 1960s and quoted from Dixon 1972:29:

> It appears that beneath the veneer of fantasy some myths may provide accurate histories of events in the distant past of the people. There is, for instance, a Ngadyan myth that explains the origin of the three volcanic crater lakes Yidyam (Lake Eacham), Barany (Lake Barrine) and Ngimun (Lake Euramoo). It is said that two newly-initiated men broke a taboo and so angered the rainbow serpent, the major spirit of the area (...). As a result, 'the camping-place began to change, the earth under the camp roaring like thunder. The wind started to blow down, as if a cyclone were coming. The camping-place began to twist and crack. While this was happening there was in the sky a red cloud, of a hue never seen before. The people tried to run from side to side but were swallowed by a crack which opened in the ground ...' This is a plausible description of a volcanic eruption. After telling the myth, in 1964, the storyteller remarked that when this happened the country round the lakes was 'not jungle - just open scrub'. In 1968, a dated pollen diagram from the organic sediments of Lake Euramoo by Peter Kershaw showed, rather surprisingly, that the rain forest in that area is only about 7,600 years old. The formation of the three volcanic lakes took place at least 10,000 years ago. All this points to the story of the volcanic eruptions, and of the spread of the rain forest, having been handed down from generation to generation for something like ten millennia. This is perfectly possible: recent archaeological word suggests that aborigines have been in Australia for at least 25,000 years,[5] and the Dyirbal could well have been in more or less their present territory for 10,000 years or more.

There are several problems with the assumption that Dixon draws. First, just because people tell a story now and claim it as their own, that should not imply that they are the direct descendants of the people that the story happened to. Dixon gives no dates for population settlements in the areas he talks about, but from Mulvaney and Kamminga (1999:334) the area was settled around 5000 years ago, and extensive exploitation began from around 2000 years ago, considerably later than the eruption.

Furthermore, the events in such stories are often very general and do not need to have been true (for example, seeing an island off the coast, it is very easy to make up a story that it was once possible to walk to it). The events, even if true, might not refer to the same area. Australia is, after all, a very big place.

More importantly for methodology, for every element in a story that might be true, there is usually a chunk of the story that is discarded (for example, the

[5] The figure is now 55,000 years +.

rainbow serpent in the Dyirbal myth above). This very selective use of data leads us to ignore almost *everything* which does not fit our ideas of the prehistory of the area and only pick up on the aspects of the myth that fit.

To return briefly to archaeology, we do have evidence for settlement beginning well over 40,000 years ago in different parts of the country (O'Connor 1999, Mulvaney and Kamminga 1999). However, we also have ample evidence that the current distribution of languages is highly unlikely to represent anything like patterns of original settlement. The archeological literature is full of descriptions of site abandonment and recolonization over time periods of over 10,000 years (see, for example, Smith *et al* 1991). We also have considerable evidence for Holocene expansion, intensification and reoccupation of previously abandoned areas (McConvell 1996, Veth 1993, for example). Thus we have no evidence that the distribution of Pama-Nyungan languages reflects the dispersal of the original settlers to Australia, but we do have quite a bit of evidence for a *lack* of continuity in many parts of the country.

5. An alternative model

I have gone into a discussion of punctuated equilibrium in some detail, in order to describe the model and to summarize the relevant issues in accounting for the current distribution of Australian languages. In this section, I outline my alternative scenario and the accompanying model of linguistic differentiation as it applies to the Australian situation.

The ideas on which this model is based have been around for quite a while, and various authors, including McConvell and Evans (1997), assume something like what I am proposing here. The model of that describes linguistic differentiation is due almost entirely to Ross (1988, 1996, 1997). However, it is worth spelling out the scenario in detail, and making a number of assumptions explicit, precisely for the reason that this model does seem to be assumed or subsumed under a family tree model. Evans (in press), McConvell and others who have written on the question still write in terms of a splitting family tree which is as yet to be fully articulated.

Another important point to make is that this model is a model of language change and classification/genetic relatedness; it is not a model encompassing socio-historical factors (other than the point that speakers of different linguistic allegiances must remain in contact); thus while I subscribe to the view (following Thomason and Kaufman 1988) that the history of a group of languages is a function of the history of its speakers, and does not exist in isolation, I do believe that socio-historical factors should be modeled independently of their linguistic consequences; as Kuteva (1999) and Campbell (in press a, in press b) have convincingly shown, there is no one-to-one correspondence between non-linguistic events and types of language change; thus while linguistic history is a

function of non-linguistic events, the two can, and should, in my opinion, be modeled separately.[6]

Finally, what I am suggesting here is one possible way in which the languages of Australia, particularly of the Pama-Nyungan group, could be related, and a scenario to account for their current distribution. I am not claiming by any means that it is the only way, but it does fit our current knowledge not only of the Australian prehistoric situation, but our knowledge of mechanisms of language change elsewhere in the world — in my model, Australia is an unusual case, but it does not require special pleading.[7]

The scenario that I propose to account for the lack of clear binary splits in Pama-Nyungan has its basis in dialect geography. The principles of dialect geography are well-known from works such as Chambers (1998) and will only be briefly repeated here. The three most important for my purposes are:

- Speech communities are differentiated by isoglosses; isoglosses tend to bunch along natural barriers to communication but do not necessarily do so;
- speech communities in the same geographical area often form chains of mutual intelligibility; that is, adjacent languages may differ minimally but either ends of the continuum may be mutually unintelligible;
- speech varieties at the epicenter of an area tend to be innovative, while isolated conservative pockets exist on the fringes.[8]

Now, consider the relationship between dialects and the family tree model; it has long been a paradox of genetic linguistics that the relationships between languages may be modeled on a family tree, but the relationships between languages do not fit a tree so easily. See, for example, Hock (1991:432ff) especially p 450:

> The linguistic relationship between neighboring dialects of the same language very commonly cannot be stated in terms of tree diagrams. This is a consequence of the fact that these speech varieties remain mutually intelligible, stay in close contact, and therefore continue to interact with each other on a day-to-day basis, with shifting realignments as political and social circumstances change. It is therefore unrealistic to expect clear, 'tree-diagram' splits in such dialect continua.

[6] Of course, this is not to say that a unified treatment is impossible, just that it is impossible on the current data; this raises an important point about the design of linguistic models and what they do; they do not have to provide reasons for the effects exhibited in the data. There is a crucial difference between descriptive models and explanatory models, and many authors appear to confuse the two, or provide one for the other. This is (almost) purely a descriptive model.

[7] Note that although, especially in the following sections, I am directly contrasting my model with Dixon's; I do not mean this to be an adversarial paper, far from it! Rather, I wish simply to point out that there are strikingly different ways of viewing the same data.

[8] Note that the 'epicenter' of a linguistic area need not be the geographical center of the area.

Now, consider what we would expect to happen if dialectal speech communities continued to diverge, *in situ* and remaining in linguistic and social contact with one another. I argue that in such a situation we would expect to find a series of dialectal epicenters, with the speech communities around those epicenters speaking varieties more similar to one another's than to varieties of other epicenters; we would expect to find some isoglossic scarps; that is, divisions where isoglosses have accumulated and bunch; we would also expect to find conflicting subgroupings. That is, we would find languages that exhibit traits that would in family tree terms lead them to be classified in multiple groups.

In this scenario, the possibilities for subgrouping would be quite limited, and often conflicting. However, crucially, the languages are still *genetically related*, in the same way that dialects without bunching isoglosses are. They will still probably exhibit regular correspondences and they are still descended from a single parent.

There seem to be three major advantages to this model over punctuated equilibrium to account for the distribution of languages in Australia. Firstly, there is a place for both divergence and convergence as processes of language change; punctuated equilibrium stresses convergence as the main mechanism of language change. Secondly, it makes Pama-Nyungan look much more similar to other areas of the world. We no longer have to assume that Australia is a special case. Thirdly, and related to this, we do not have to assume in this model that there has been intensive diffusion of many linguistic elements that in other parts of the world are resistant to borrowing (such as shared irregularities).

My model does assume that the dispersal of speakers of Pama-Nyungan is relatively recent; not, that is, of the order of 50,000 years or more ago, perhaps an order of magnitude more recent. This is in accordance with the relevant archaeological evidence, although, of course, linking the spread of Pama-Nyungan with the series of (re)colonizations of abandoned or previously uninhabited areas is an unsupported assumption.

These claims should not be considered original, and others have claimed what amounts almost to the same thing. The work of Malcolm Ross, in particular, discusses cases of conflicting subgrouping in Oceanic and in particular 'innovation-linked' subgroups; such groups share a common set of innovations, which are not shared outside the subgroup, yet the languages themselves provide us with no evidence for a single common proto-languages from which all the languages in the subgroup are descended.

6. Conclusions and further directions

I have shown in this paper that the punctuated equilibrium model and the classical family tree models are not the only way to view the history of Pama-Nyungan expansion in Australia; that we can assume a date of expansion more recent than initial colonization and still account for the lack of an articulated family tree, and that we can reconcile diffusion areas with recent migration and still talk in terms of genetic relationship.

Of course, this model requires intensive testing, both at the level of Pama-Nyungan but also at the level of subgroups of Pama-Nyungan. Work on this is progressing, including the results published in Bowern and Koch (2002) and Dench (2001); other work is in progress. Without detailed, step-by-step reconstruction and plotting of isoglosses, hypotheses of innovations, and the like, however, these ideas remain untestable.

I show that the current distribution of Australian languages need not be explained by the heavy diffusion scenario that Dixon argues for, but can be explained within mainstream models of language change. Indeed, I argue that the Pama-Nyungan family can be most easily explained as a relic of a dialect area. This implies that the similarities between Pama-Nyungan languages are due primarily to common genetic inheritance, and the overlapping isoglosses, to a large extent, from either parallel innovation or from old dialect areas.

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