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REVIEW ARTICLE

R. M. W. Dixon, *Australian Languages: Their nature and development*. Cambridge: Cambridge University Press, 2002. Pp. xlii, 734. Hb $95.00.

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INTRODUCTION*

Australian languages is a typological approach to the description and historical study of the Aboriginal languages of Australia. Together with its planned companion volume, *Australian languages: A complete catalogue*, a collection of sketches of some 250 languages, it is intended to present non-Australianist scholars with comprehensive information about Aboriginal languages and to offer specialists a summary and an agenda for future research. There are 14 chapters: "The language situation in Australia," "Modelling the language situation," "Overview," "Vocabulary," "Case and other nominal suffixes," "Verbs," "Pronouns," "Bound pronouns," "Prefixing and fusion," "Generic nouns, classifiers, genders and noun classes," "Ergative/accusative morphological and syntactic profiles," "Phonology," "Genetic subgroups and small linguistic areas," and "Summary and conclusion." (Aspects of syntax such as constituent order are given only cursory attention; see pp. 78–79.)

Dixon's credentials to write a comprehensive book on Australian languages are impressive: fieldwork resulting in a number of descriptive and general publications since 1964, and from the early 1970s teaching and supervision of numerous students who have gone on to work in this area. He has put a great deal of knowledge and analytic work into each of his chapters. It is thus in a sense paradoxical that many Australianists, myself included, find much to object to here.

Throughout this book (xx, 698–99) runs an insistence that Australian Aboriginal languages, in many regards but with special regard to the claimed lack of major subgroups and inapplicability of the historical-comparative method to them, are unique in the world. These claims of uniqueness are belied by the successes of the comparative method in Australia (see Koch 2004, Bowern & Koch 2004 and other papers in Bowern & Koch [eds.] 2004, and Evans 2003 and other papers in Evans [ed.] 2003) and by the existence of other, well-studied language families with "flat" phylogenetic structure and numerous loan-versus-inheritance mysteries. The identification of and study of loans is, to be sure, of central importance in historical-comparative linguistics, but Dixon's typological approach takes diffusion as the default hypothesis for any language change and, in so doing, often fails to try to identify just which similarities are in fact due to diffusion and which are not.

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There is far more material in this work than can be described, let alone critiqued, here. I confine this commentary to a small selection of topics, with the aim of showing through a few detailed examples some of the consequences of Dixon's methods both for our understanding of the historical problems and for future research. As examples of his style of argumentation, with regard to its breadth on the one hand and its limitations on the other, I discuss his vocabulary, pronominal, and verb paradigm reconstructions and his typology of verb+coverb combinations, I point out some of the questionable uses of statistics, and I consider his modeling of the sociocultural setting of these languages and of language change processes in Australia. I address, in passing, questions concerning the Pama-Nyungan family, which Dixon (especially 44–54) sees as a typological artifact and which I regard (1991, 2004) as clearly a genetic family, and problems of replication arising from Dixon's method of listing languages (xxx–xlii).

RECONSTRUCTION

In his section on vocabulary (96–130), Dixon makes something resembling lexical reconstructions: "I generally give the basic form of a lexeme. It is of course to be expected that it will lose vowel length in a language that lacks contrastive length, undergo initial dropping in an initial-dropping language, final dropping in a final-dropping language, and also assimilations and/or lenitions, etc. in other languages" (102). He does not attribute these "basic forms" to protolanguages, and indeed insists that no protolanguage applying to a large proportion of the continent can be reconstructed. He is very serious in substituting typology for historical-comparative reconstruction: In his consideration of the regular correspondence of initial *l/r/d/y* (as in *lirra/rirra/dirra/yirra* 'tooth' or 'mouth' in languages across the continent; to a comparativist the attestations in Pama-Nyungan languages are the regular reflexes of *rirra, with some residue to be accounted for, perhaps as loans), his wording of the "borrowing" option is "from a neighbouring language which has A DIFFERENT PROFILE WITH RESPECT TO THIS PARAMETER"(126–7; emphasis mine).

This example notwithstanding, Dixon by and large pays only cursory attention to problems with the regularity of sound correspondences and is all too ready to appeal to shortcuts. For example, he suggests, without endorsing, a derivation of Western Torres Strait *kuma* 'faeces' from **kuna*, a clearly Proto-Pama-Nyungan (PPN) etymon with reflexes in languages right across the continent meaning 'faeces' (108,130). But *kuma* is arguably a reflex of PPN **kuma*, which continues in other languages in the sense 'blood'. Here Dixon entertains, without endorsing, a "special correspondence" *n* to *m*, rather than a semantic shift in a word in the body-part-and-product domain, with possible contamination from a reflex of **kuna*. The result, here and elsewhere, is a picture of apparent chaos where, in many cases, subtle explorations of language history are possible.

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Other areas in which Dixon has been tempted to provide "reconstructions" include that of free pronouns (300–15; here he does not use the term "basic form"). He first provides a set of reconstructions "across languages of groups A-Y, WA-WM" (300; these are the languages otherwise known as Pama-Nyungan, whose existence as a genetic group he has been at great pains to deny [44–54]), and then he generalizes to the continent as a whole (315). These "reconstructions" are made without attention to regularity of sound correspondences (see Alpher 2004:123–5).

When Dixon focuses attention on inflected forms (209–37, for verbs), problems multiply. Walmajarri (Pama-Nyungan, Western Australia) *kampawu* 'will cook it' (past tense form *kamparni*), is indisputably derived (222) by lenition of the **k* in a protoform **kampaku* – cf. Pitjantjatjara *kampaku* 'will burn' – to *w*. However, Dixon considers this lenition to be an instance of grammatically conditioned sound change (with a morpheme boundary before the *w*), since "it is certainly the case that *k* does not lenite to *w* in all intervocalic positions." He no doubt has in mind here instances like Walmajarri *luku*, Warlpiri *luku* (both 'heel') and Walmajarri *kakarra*, Warlpiri *kakarrara* (both 'east'), suggesting Proto-Ngumpin-Yapa (the established terminology; for Dixon this would be "proto-Northern Desert Fringe" or "pWJ") **luku* and **kakarra*, respectively. For Walmajarri, Hudson (1978:12) sets up the future ending as underlying /-ku/, realized as *ku* (the historically original form) after consonants and *wu* after vowels. Note, however, that the shortest stems in Walmajarri have the form CV(-)C- and hence that this /-ku/ can never appear as the second syllable of a word after a vowel. In fact, the conditioning factor (at least historically) appears to have been INTERVOCALIC IN A SYLLABLE AFTER THE SECOND, a consequence of first-syllable stress that is attested elsewhere in Australia (see Alpher 2004:101). Consistent with this hypothesis is the pair Warlpiri *marnakiji* ~ *marnikiji* (with other alternants), Walmajarri *marnuwiji* (both 'conkerberry'), suggesting Proto-Ngumpin-Yapa **marnukiji* (M. Laughren and P. McConvell, pers. comm.).

Interesting and difficult-to-counter claims, requiring a theoretical approach to deal with, have been made that sound change can be conditioned by grammatical factors like morpheme boundaries (Kiparsky 1988:367, 372ff., and references). Cases like Walmajarri *kampawu*, however, are not among the challenging examples. And in Dixon's rush to judgment on this matter something is lost: the possibility of making a real study of diffusion and diffusional stratigraphy and hence of culture history through comparative linguistics, since there are examples of the type Warlpiri *ngalikirri*, Walmajarri *ngalikirri* (both 'top grindstone').

Although eschewing protolanguages, Dixon cannot resist postulating, in chapter after chapter, something rather like an original typological state from which development commenced. With regard to verb inflection: "It seems reasonable to extrapolate back to an earlier stage that was basically agglutinative; that is, all types of morpheme boundaries were readily segmentable" (215). I have argued

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elsewhere (Alpher 1991) on comparative grounds that there is no reason to suppose, with regard to Proto-Pama-Nyungan, that all tense forms in the paradigm of any given verb were equally segmentable. Even supposing that inflectional endings of verbs always originate as something not attached to the verb (and there are serious reservations about this assumption; see Anderson 1988:338), there is no reason to suppose that all the endings in a paradigm originated this way at once. There is every reason to suppose that no matter how far back one goes, a gradation of morphological transparency obtained.

The program for research here is, in general, almost exclusively a typological one. Historical-comparative reconstruction, on any level larger than that of rather small subgroups, is discouraged. That is the thrust of much of Dixon's presentation of putative irregularities.

STATISTICS

Underlying the lexical part of Dixon's diffusionism is his lexicostatistical "50% equilibrium level" (27–28): In an "equilibrium" situation such as is said to have existed in Australia, the lexicons of two adjacent languages, no matter how unlike initially, will through mutual borrowing come to resemble each other over time to the extent that roughly 50% of the lexical items are the same, and two adjacent languages starting from identity will lose vocabulary by replacing it with loans from other languages to the extent that roughly 50% will remain in common. The assumptions underlying this claim include the following: that lexical "sames" are counted whether or not they are identifiable loans – this assumption alone renders the findings incommensurable with those of the "classical" lexicostatistical work performed, and critiqued, in the 1950s and 1960s; that the size and "basicness" of the lists does not make a difference, a claim Dixon has long made but for which the evidence has never been published; and that the fraction of lexical replacement that is accomplished by borrowing is about 80%, an absurdly high figure without basis in data from other language families and at serious variance with available estimates from Australia (cf. Alpher & Nash 1999).

While firmly rejecting the claim that there is some rate of lexical replacement that is constant across time and across languages, at least in the Australian context, Dixon does not acknowledge that this very assumption of constancy lurks in his use of lexicostatistics to draw inferences about recency of language adjacency. He surmises (682–683) that the Maric languages ("Ja," with five divisions, some of them multidialectal) spread like this: "As a guess, I would opt for the proto-language having been spoken on the [Pacific] coastal strip, with speakers of Ja increasing in number and expanding in all directions – first north, then west, and lastly south." This inference is based on the 50% lexical similarity of the northernmost Maric language to its non-Maric neighbor, Dyirbal, versus figures of 29% for unnamed Maric subdivisions with the nearest languages to the west and of 10% for unnamed Maric subdivisions with the nearest languages to

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the south (note that, at this level of specification, replication of these results is not possible). Terms like "guess" and "opt for" are hedges, as is another formulation: "Study of the vocabulary similarities between a given language and each language which it borders often carries a suggestion that the language has been in contact with some neighbours for a relatively short time, and with others for a much longer period" (29–30; see also 47–8). It is clear, however, that no such conclusion is possible absent an assumption that different languages (here three subdivisions of Maric) replace vocabulary at roughly the same rate – an assumption that Dixon (45) vigorously denies.

Among Dixon's arguments for rejecting Pama-Nyungan as a genetic group is a statistical one, which also illustrates an implicit claim that the comparative picture is chaotic. In a division of Australia into four areas, "non-Pama-Nyungan" and three arbitrary divisions of "Pama-Nyungan," he counts the number of "recurrent lexemes" occurring in at least two of these subdivisions and finds (p. 50) that each of the four subdivisions contains roughly the same number. If for no other reason than that the non-Pama-Nyungan forms are non-comparable (see Alpher 2004:119–122), this argument is a misuse of statistics. Note also that, given the level of nonspecificity in the naming of groups, replicating these results is discouragingly difficult, even if no evaluation of cognacy is attempted. Take for example the "basic form" *madha-* 'chew, bite, suck, eat' (121). It is listed as occurring in, among other groups, "J" ("Greater Maric," Pama-Nyungan: 6 listed sources, 13 "languages," many with no listed source), and "NBf" (as *madja-* 'kiss'; Maningrida subgroup, non-Pama-Nyungan: 4 languages, 4 sources). Another example: *bun.ga-* 'fall' (118), listed "with assimilations" also as *bunda-* (in the Pama-Nyungan groups "M" – Central East Coast; and "N" – Central New South Wales, many sources and many languages, some not sourced) and *bungga-* (in a number of Pama-Nyungan groups and also "NBf"). No attempt is made to look for regularities in the "assimilations" with *bun.ga-*; no effort is made to ascertain if the Pama-Nyungan forms are in fact mutual genetic cognates; no claim is made – indeed, none can be rigorously substantiated – that the "NBf" forms are genetic cognates of the others. All that is done is blind listing and counting.

COVERBS

Another example of Dixon's typological approach is his treatment (183–201) of verbal derivation with coverbs, noninflecting elements that together with a "simple" verb that inflects for tense-aspect-mood constitute new lexical verbs ("complex verbs"). An example in Walmajarri is *pina-karri-* 'hear', with *pina* 'ear' and *karri-* 'stand'. Dixon types languages with such constructions along a number of parameters: how many inflecting verbs, how many coverbs, and how many coverb+verb items there are; what proportion of the inflecting verbs are regularly used in this way; whether other material can intervene between the two;

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whether a synchronic morpheme boundary is discernible between the two; whether person-number prefixes (if any) are attached to the main verb or the coverb+verb complex. He finds three main types:

> Type (a). Few simple verbs, each with a generic meaning [examples: 'go', 'get', 'carry']. Some of the simple verbs may occur alone, but all can be combined with a coverb in a complex verb construction, and it is these that predominate in texts.
>
> Type (c). Many simple verbs, most with specific meanings. Just a few simple verbs, with generic meanings, can be combined with a coverb in a complex verb construction, and it is these that predominate in texts.
>
> Type (g): Very many simple verbs, all tending to have rather specific meanings. There are very few compound verbs, both on a dictionary and on a text count. (197)

There are intermediate types, and Dixon postulates lines of development (speculative scenarios) from one to another, sometimes in both directions. An example:

> *Type (c) becomes Type (a).* . . . One possible scenario is that those simple verbs that do not occur in complex verbs could gradually drop out of use, so that one goes from Type (c), with hundreds of simple verbs, to Type (b), with thirty to sixty simple verbs, to Type (a), where the only simple verbs remaining are the few – usually between about five and about thirty – occurring with coverbs. (197–198)

Again, Dixon hazards a guess at an initial state – typological, not a protolanguage, and to take him seriously here as elsewhere one must imagine an "original state" potentially consisting of more than one language, just so long as they are of the same "type":

> An obvious question to ask is: what was the original type of verbal organization like, at the earliest stage of the Australian language area? This is in fact not a fruitful question to pursue. Types of verbal organisation may well have been oscillating, within the scheme shown . . . for tens of millennia. There may have been several circuits around the cycle. It is not possible, on comparative linguistic evidence, to perceive what the point of entry might have been. . . . It is impossible to decide on the original type of verbal organisation from the normal methodology of linguistic comparison and reconstruction. But my a priori hypothesis concerning the original pattern of semantic organisation would suggest a variant of Type (a) as the point of entry. (200)

In fact, normal comparison and reconstruction, though it cannot reach the "original" language (the "Proto-Australian" of Dixon 1980, which in this book is correctly considered out of reach) and does not readily yield typological classi-

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fications of this kind, nonetheless is able to contribute something to this discussion.

A preliminary comparative study of the Gunwinyguan languages (Harvey 2003) identified some 1,300 etyma, of which roughly 200 are coverbs and about 30 are "complex verbs," 24 of them formed on one of the generic simple verbs **ka-* 'take, carry', **ma-* 'get' or 'do, say', and **pu-* 'hit'; some 24 simple inflecting verbs have been reconstructed (Alpher, Evans & Harvey 2003:345). Because the person-number prefix precedes the coverb in these languages, Dixon regards the "complex verb" as "fused," his Type (d), which in other respects can resemble Type (a), (b), or (c). In the case of proto-Gunwinyguan, the type appears to have been (a), though caution is in order because the definitions of these types involve statistics, which will be unreliable when applied to a reconstructed protolanguage.

Proto-Pama-Nyungan was apparently different: In a list of some 160 etyma, 35 are inflecting verbs (Alpher 2004), and I suspect that the protolanguage had a great many monomorphemic verbs. Although it seems probable that there were "complex verbs" of the type Walmajarri *jina-ma-n* 'to track' ('foot' + 'do') with a generic simple verb, and although coverb etyma (**pirri-* 'scatter') exist, convincing examples of full cognate "complex verbs" attested across the geographical spread of Pama-Nyungan have not yet been identified (like this is the situation that Bowern 2004:340–4 finds for Proto-Nyulnyulan). Most interesting, however, is the correspondence (in a couple of instances) of coverbs in one language with inflecting ("simple") verbs in another, distant area of Pama-Nyungan: the Walmajarri coverb *jingkirti* ~ *jingkirri* 'laugh' (closely related Djaru has *jingkiri* 'laughing') occurs in *jingkirti-ma-n* 'to laugh' and *jingkirti-ka-ng* 'to make fun of' and corresponds to an inflecting verb **cangkari-* reconstructed for northeastern Pama-Nyungan. Comparative evidence available at present does not permit a conclusion as to whether the inflecting verb or the noninflecting coverb came first in these cases.

Some of Dixon's types seem to be arbitrary points, and not necessarily stopping points, in speculated transitions. Even an apparently compelling instance of typological interaction (if person-number prefixes invariably precede the coverb + verb unit, then fusion of coverb and verb must have preceded the development of person-number prefixes; 188, 410) involves a notion of "fusion" that is in part defined by the failure of prefixes to intervene and is in part dependent on a speculation – possibly quite wrong with regard to the languages of Arnhem Land – that person-number prefixation is a relatively recent innovation. I submit that ordinary comparative reconstruction, with its concrete examples, is a more promising avenue of research

MODELS OF LANGUAGE CHANGE AND SOCIAL SETTING

As Dixon sees it, the predominant engines of language change in Aboriginal Australia have been diffusion and typological drift, with sound change and the

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analogical changes it stimulates playing a less conspicuous role. It has been shown repeatedly for Australia as for elsewhere that items like affixes and closed-category words can diffuse. Dixon, however, operates under an assumption that diffusion of closed-category items is pervasive, including, for example, not just a few but many instances of diffusion of pronouns. This claim is used, in particular, as an argument against "Pama-Nyungan" as a genetic group. For example, the pronoun *ngali* (277–83, 297), usually 'you and I' (dual), is very widespread but attested only in Pama-Nyungan languages. Dixon holds that it originated somewhere in the north and diffused from one language to the next, backed by paradigmatic pressure toward a particular "type" of pronoun system (280–2), always southward and never to the languages in the north that on other grounds are reckoned to be non-Pama-Nyungan. Other pronouns are held to have done the same. The limitation of their distribution to putatively Pama-Nyungan languages is dismissed as a matter for further study (297; elsewhere in the book, as at 29–30, strong discontinuities are attributed to recent spread of languages).

Claims of this kind would be more plausible if backed by truly concrete historical-comparative linguistic findings and a sophisticated model of the social organization in which language contact and spread take place. Neither is forthcoming in Dixon's study. In his picture of the social situation in which these languages are used and change, the fundamental political unit in traditional Aboriginal life has been the "tribe" (3): "Each had its own territory, system of social organization, traditional oral literature and laws, song styles, and its own 'language' – just like the nations of Europe, but on a smaller scale." Dixon is careful to distinguish a "language" in the linguist's sense, which can consist of a chain of mutually intelligible dialects spoken by a number of "tribes," from the "language" that is a fundamental possession of a "tribe" linked indissolubly with its land.

This picture is based partly on an outmoded social-anthropological model (almost a European folkloric one) and partly on material Dixon gathered in the field in the rainforest area of northeastern Queensland beginning in 1964. However, on the basis of anthropological research all over Australia from the 1920s on, and in particular on the basis of intensive fieldwork by both anthropologists and linguists in aid of Aboriginal land claims from 1976 on, it appears that Dixon's picture, if taken to hold true of the "classical" situation, is false. In well-studied tropical areas like western Cape York Peninsula and northeastern and central Arnhem Land, traditional organization has been first at the level of the exogamous clan, or "patrifilial group," according to some analysts (Sutton 1998:24,39 and 1991:49–55, Keen 1995; with regard to the Northeast Queensland rainforest see Rigsby et al. 2002, especially chaps. 4 and 7), and the ceremonial group, a loose multilanguage conglomerate in which membership is not necessarily mutually exclusive with membership in other such groups. In these areas each clan possesses a language, and its neighbors (and the groups with

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whom intermarriage is most likely to take place) very often have different languages or dialects. This picture is clearly drawn in Sutton 1978.

With regard to marriage across language boundaries, Sutton shows schematically with adjacent hexagons (1978:23) how, if the idealized language unit is the single clan (one hexagon), as is quite often the case in areas rich in resources, all immediate contacts (the six faces of the hexagon) will be with speakers of other languages or dialects, whereas in, say, a seven-clan/hexagon single-language bloc there will be twelve internal (same-language) faces (marriages within the "tribe") and eighteen external ones (out-marriages). Increase the number of language-uniform units, and the ratio of in-marriage to out-marriage will increase.

The "clan" is by no means universal in Australia, particularly in the central and western desert areas, where the list of traditional owners of a site or tract is not an entity such that, for any given person, that person can be said definitively to be "in" or "not in" it. However, patterns of marriage alliances rarely appear to be guided by language (or "tribe") as a criterion of preference. The pattern, as Dixon sees it, that "A spouse would normally be taken from another group of the same tribe but sometimes from a neighbouring tribe" (4) is in a sense a fact of geometry and is not explicable on the basis of a "tribal" ideology.

The unit from and to which linguistic diffusion takes place would appear to be smaller than the "tribe," a notion which is an artifact, the result of a convergence of an older scholarly tradition (in particular Birdsell 1950, 1953, 1957) and an emerging ideology among Aborigines themselves of "tribal" identity, the "new tribe" (Sutton 1998:68; Rumsey 1995:197–199). Birdsell did groundbreaking, important, and justly celebrated modeling of the relation of population-bearing capacity in an area to group size and of the process of population spread into an uninhabited area (referred to by Dixon especially on 34). He conceived of the "tribe" as a land-holding and land-defending unit and as a "genetic isolate" in the sense used by population biologists (Birdsell 1950:279–80, 1953:171–2, 1957:53; see also Tindale 1974:55–88). I would argue that the "tribe" so conceived is not an essential part of the model. Birdsell used average annual rainfall in an area as a rough first approximation to a predictor of carrying capacity and used "tribes," as determined from Tindale's map (1940), as a way to quantize the processes of gene flow and population "budding" in the settlement of uninhabited country. However, population density and the average geographical spread of territories of origin of spouses can just as well be modeled as a continuum, directly from clines of average annual rainfall. Inhibitors of marriage alliances are not necessarily either geographical or linguistic.

The "tribe" is not a useful concept for sociolinguistic study of traditional ("classical") Aboriginal Australia. The "clan" is (see, e.g., Sutton 1978), but many linguistic phenomena appear to operate according to other divisions. With regard to the taboo on words sounding like the name of a recently deceased person (27), for example, it would be useful to have, for at least one case, a thorough

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enumeration of persons who must observe it. This is unlikely to be defined by clan membership; it will be permanent for a spouse but limited to a mourning period for others, and for most it will be situational – not a "group" in the usual sense. In connection with a related topic, Dixon (43) invokes, without definition, the "local group," long a contentious subject in the literature, that will be of little help here. A desideratum for study, if that is still possible, is whether in a multi-estate one-language area what is borrowed at, say, the eastern edge is not necessarily adopted among land-holders at the western edge significantly sooner than in the next language in any direction. Questions like these cannot be asked within an analytic framework limited to the "tribe."

Dixon grounds his theory of language differentiation in the "punctuated equilibrium model" (31–35), a consequence of which – he holds without strong support – is that the expansion of a language or dialect group over a wide area and its subsequent split into subgroups, as occurred with Indo-European, can only be due to a "punctuation"—a cataclysm of some kind like war or conquest, or expansion into a previously unpopulated area. He argues (31) that if branching out like that of IE had been the norm, one would expect an astronomical number of languages to exist after 50,000 years, the presumed minimum age for language as we now know it and for human settlement in Australia. However, he has reckoned only in passing with language extinction, which in a universe of clan-sized languages must have happened a great many times. Such extinction, and the subsequent succession to ownership of the land by a neighboring group with a legitimate traditional claim to it (without population shift), has to result in language shift and expansion, and ultimately in split.

In-depth analysis of social relations might facilitate evaluation of claims of a type that Dixon advances throughout the book, that "types" diffuse independently of the concrete forms that instantiate them (25–6). For example, Warlpiri is said to have innovated new "simple" verbs under diffusional pressure from the neighboring Western Desert and Arandic languages (199). The information cited does not support this specific conclusion. Perhaps data of another type would support the general point. Samples of the speech of fully or partially competent bilingual people, together with histories of the lives, travels, and networks of these persons, would be in order. The territories in question are enormous, and the notion "tribe" and the "tribal boundaries" drawn on maps cannot give much help. It would not be easy research, involving languages of which the speakers are not the owners. But some of the scenarios proposed in this book would seem to require support of this kind.

NOTES

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