

R. M. W. Dixon, *The rise and fall of languages*. Cambridge: Cambridge University Press, 1997. Pp. vi + 169.

Reviewed by Brian D. Joseph, The Ohio State University

Drawing frequently on his rich and extensive experience with the languages of Australia and Oceania, author Dixon has written a book that is filled with claims and assertions that are sure to be as controversial as they are thought-provoking; indeed, practically every page of this book had me making notes and comments, though not always nodding in agreement. Dixon's general topic is language change, especially what we can surmise about language development in the distant past, and he puts forward a central thesis as well as some related subsidiary claims that some linguists will find quite provocative.

The thesis Dixon presents most forcefully in this work is that the punctuated equilibrium model of Eldredge & Gould 1972, originally conceived of as a model of biological evolution, can be applied fruitfully and insightfully to an understanding of the way languages have developed. By way of elaborating and defending this thesis, Dixon, after an introduction and chapter entitled "Preliminaries" in which he discusses his views on the nature of language, offers chapters on "Linguistic areas and diffusion", "The family tree model", "Modes of change", "The punctuated equilibrium model", "More on proto-languages", and "Recent history", before closing with a chapter on "Today's priorities", one entitled "Summary and prospects", and an Appendix ("Where the comparative method discovery procedure fails").

As suggested by the title of the Appendix, a subsidiary claim put forth here, one tied to his central thesis concerning punctuated equilibrium, is that the utility of the

“Stammbaum” (“family tree”) model for language development and language relationships, as well as that of the Comparative Method for reconstruction, is restricted only to certain types of situations, and is not universally applicable to all types of linguistic development. As he puts it: “‘family tree’ is only one of several interrelated models needed to explain linguistic relationships and development over the past 100,000 or so years. It is applicable to situations during periods of punctuation” (140).

One further subsidiary thesis — more a goal, really — that is developed in many places throughout the book is the political/practical aim of encouraging the exploration and description of human linguistic diversity, especially through field work “documenting the diversity before it is — as it will be — lost” (5).

I discuss below the content of several of these chapters, and offer some reflections on the claims contained therein, together with some general observations about the book.

I should start, though, by saying that the author does not define his audience appropriately. On the one hand, much in this book is of great interest to the professional linguist, but such an audience will also find much to disagree with and question. On the other hand, parts of the book suggest it is aimed at the interested lay person. For example, the basic claims concerning the comparative method and the putative value of a punctuated equilibrium model to language change make most sense directed at scholars, whereas statements such as “there is no necessary connection between literature and writing” (81), or “there is nothing that could be called a ‘primitive language’ (with just a few hundred words and only a little grammar)” (65), or “it is a common belief that all languages have three tenses —

past, present and future. This is far from being so.” (118) are surely more meant as correctives to views that a nonlinguist reader may hold. Even the presentation of his subsidiary aim of encouraging field work vacillates between being aimed at professional linguists and being directed to a general readership. On the one hand, Dixon seems to want to rally general support for the idea (note the subtitle “Why bother?” for section 9.1 concerning the value of describing endangered languages, where he successfully counters the simplistic Darwinian view — “survival of the fittest” — that many lay people take towards the survival of languages); however, any changes to be effected in the practice of linguistics must necessarily start with decisions by professionals to take up the author’s challenges (and to ignore his sometimes blunt condemnations — for which he offers no apologies (6) — of their frank lack of interest in language description, despite what he characterizes as a lot of “talk” (144n.3) about language endangerment).

To some extent this (actually quite mild) “schizophrenia” regarding audience is a function of the brevity of the book, with only 152 content pages in a rather small (4"x6" (= 10x15cm) format. The presentation of serious claims thus generally comes through the medium of overly concise statements that mask important complexities. Still, Dixon’s simple, direct, and often boldly stated observations about language hold much for professional linguists to learn from, and to argue with, as becomes clear below as I enumerate some of the interesting and controversial claims, chapter by chapter, commenting on them from the perspective of the languages I know best.

In Chapter 1, “Introduction”, we encounter the first discussion of the family tree model, as Dixon puts it (with similar statements in Chapters 6 and 10): “For

some groups of languages — for instance, Semitic and Polynesian — the family tree model is entirely applicable. For others it may be less so; the similarities that have been taken as evidence for genetic relationship may really be due to areal diffusion” (1). This view, however, repeated later on e.g. in Chapter 4 (29), seems to suggest that the family tree model and the “wave” (i.e. diffusionary) model are mutually exclusive, when instead it is more reasonable to admit that each one responds to a different type of question or set of circumstances — that is, in cases of diffusion, such as the well-studied structural convergences found in the Balkans, the family tree model may not yield insightful results, but it provides the backdrop against which innovations found in one language can be judged as non-inherited. In a related matter, when he castigates (p.2) the common practice in which proto-languages as reconstructed “tend to show tidy and homogeneous patterns” even though “attested languages are seldom like this”, Dixon is surely asking too much of standard methodologies. For one thing, reconstruction of variation is possible and sometimes even essential, e.g. when the offspring languages show great diversity (as perhaps with the thematic genitive singular ending in the Indo-European languages, some of which show *-os (Hittite), some of which show *-osyo (Indo-Iranian, Greek, Armenian, marginally Italic), some of which show *-eso (Germanic), and some of which show *-ī (Celtic, and, to a considerable extent, Italic)), or when they show similar but irreconcilable forms (as perhaps with the well-known *-bh- vs. *-m- in various oblique cases in Indo-European, though see Hock 1991:585-6, 590-1 for discussion).

Similarly in Chapter 1, Dixon introduces his notion of equilibrium and what he sees as its linguistic correlates (all elaborated on in Chapter 6). In a state of

equilibrium in geographical areas with “relatively easy communication ... there would have been a number of political groups, of similar size and organization, with no one group having undue prestige over the others [and] each would have spoken its own language or dialect” (3). Dixon then claims that “during a period of equilibrium, linguistic features tend to diffuse across the languages of a given area so that — over a very long period — they converge on a common prototype” (4). This claim may very well be right, but even so, some qualification is needed. It describes well the situation with classic Sprachbund cases, such as that found in the Balkans, where the “Pax Ottomanica”, the period of relative stability in the Medieval Balkans during the time of the Ottoman Empire, gave rise to the conditions in which heavy structural borrowing could occur. However, equilibrium must surely be at best an indirect cause, in that it would foster bi- or multi-lingualism, imperfect in some instances, on the part of individuals, which, together with the sociolinguistic accommodation necessary in such contact situations and simplification in natural second language learning, would be the proximate cause of the spread of features and the structural convergence of languages.

Dixon’s Chapter 2 spells out several of his basic assumptions, all generally quite reasonable, in my opinion, though some are controversial. Thus while no one (probably) will argue with Dixon’s claims that “every language ... is always in a state of change” (9) and “the rate at which a language changes is not constant and is not predictable” (9), some (though not me) will take exception to the claim that “there is no universal principle that core vocabulary ... is less likely to be borrowed than non-core items” (10, a view that the author rightly points out vitiates glottochronology), and some will consider his claim that “in the normal course of

linguistic evolution, each language has a single parent” (11) simply wrong in the light of Thomason & Kaufman’s (1988) discussion of Ma’a and Copper Island Aleut (both of which cases are countered, though, pp. 11-13, and at any rate Dixon’s use of the qualifier “normal” provides the leeway he needs here). There are two other preliminary points in this chapter, however, that I must take exception with. In discussing the language versus dialect issue (7, more for the benefit of the nonlinguist audience, it would seem), Dixon says some very sensible things about political definitions (and in Chapter 5, he emphasizes the role of such external factors (61-2)), and also about mutual intelligibility, but he then talks about intelligibility in percentage terms (speakers understanding “very little (maybe 10%) ... or almost everything (70% or more)”) which give an air of preciseness to something which most linguists, I would venture to say, would not consider quantifiable (or at least not easily so). Also, on page 13, the reader encounters the oft-drawn distinction between “contact-induced change [and] changes due to the internal dynamics of the language”, although perhaps all that is really at issue there, especially if change is defined by spread through a wider range of speakers (i.e., a speech community) and contexts (and is thus distinct from an “innovation”, the first point of entry for some novel element in a language), is how an innovation first enters an individual speaker’s system.

Dixon closes Chapter 2 with what he terms an “anti-assumption”, namely (14) to “question the assumption that is frequently made ... that all language development, and all types of proof of genetic relationship, must be like what happened in the Indo-European family”. The first part of that “anti-assumption” continues Dixon’s serious challenge to diachronicians, as it runs counter to the

explicit claim of Bloomfield (1925: 130), an eloquent statement that is worth quoting in full:

I hope, also, to help dispose of the notion that the usual processes of linguistic change are suspended on the American continent (Meillet and Cohen, *Les langues du monde*, Paris 1924, p.9). If there exists anywhere a language in which these processes do not occur (sound-change independent of meaning, analogic change, etc.), then they will not explain the history of Indo-European or of any other language. A principle such as the regularity of phonetic change is not part of the specific tradition handed on to each new speaker of a given language, but is either a universal trait of human speech or nothing at all, an error.

Is Dixon suggesting that these principles are suspended, e.g. in Australia, or is his reference to “language development” just vague enough to allow him to have his cake and eat it too?

The second part of Dixon’s “anti-assumption”, however, seems a bit odd, considering that later, in Chapter 4 (37-44) he comes down so hard on proponents of Nostratic and Proto-World, calling their claims of distant genetic relationships “simply implausible” and methodologically flawed in their insistence, very much unlike traditional Indo-Europeanist practices, “that the main thing to be considered when formulating a genetic connection between two languages is lexemes ... [not] correspondences between grammatical forms, preferably grammatical paradigms”

(39n.9).¹ However, Dixon is not being inconsistent, since he states (40) that “the error in all this work is not just in failing to take proper scientific care in comparing languages, but in relying on family trees as the only model of linguistic relationship”. His own “integrated model ... combining the family tree and diffusion models”, Dixon claims, would remove any “temptation to perpetrate anything such as ‘Nostratic’” (40).

In Chapter 5, the concern is with the nature of change, and in part the question of whether change is sudden or gradual. Dixon claims that “many types of change ... are not gradual but rather happen fairly suddenly, often within the space of a generation or two” and, in keeping of course with his interest in punctuated equilibrium, likens change to a “series of steps” rather “than a steady incline” (54). Terminology may be a problem here, though, for in what sense is a change “sudden” if it requires two generations to run to completion and be fully generalized? The distinction referred to above between “innovation” and “change” might be helpful here, for an innovation, almost by definition, will always

¹Dixon is rightly agnostic in Chapter 5 with regard to whether “language developed just once (monogenesis) or separately in two or more places (polygenesis)” (66, so also in Chapter 10, 143), even though the Proto-World hypothesis, which Dixon clearly does not favor, would presumably lead one to posit monogenesis (on the impossibility of monogenesis for *all* languages, see Salmons & Joseph (1998: 3n.7), who note that signed languages such as ASL are full-fledged languages in all relevant respects that have an entirely different origin from any non-signed language).

be sudden but a change, if it depends on spread, will necessarily have some degree of gradualness (though the spread can be quite rapid). He may of course not be talking about spread through a speech community but rather the emergence of patterns in a subsystem of the grammar, but even there gradualness can be found. The replacement of infinitival complementation by finite clauses in Post-Classical Greek, for instance, took over a thousand years to affect all verbs in the language fully, and Dixon's claim that it is unlikely that one person out of a person/number paradigm for pronouns "would first become an obligatory bound clitic, while other pronouns remained as free forms" is counter-exemplified by the creation of a weak subject pronoun in early Modern Greek for third person only (and only for two predicates).

As should be clear, Dixon is as much interested in linguistic prehistory as in linguistic history,² and he makes numerous assertions about the prehistoric linguistic situation in several parts of the world. In Chapter 6, for instance, he elaborates more fully on his punctuated equilibrium model, describing periods of equilibrium and their linguistic ramifications (i.e., Sprachbund-like convergence, see above), the effects of sudden events — mostly nonlinguistic in nature, (e.g. due to natural causes, aggressive conquests, etc., acts continuing into the modern era as Europeans spread all over the globe, as Dixon reminds us in Chapter 8) — that disturb and fragment the stable state of equilibrium, leading to splitting of political groups and thus the development of new languages, all with "the original genetic

²And, we might say as well, as in linguistic future, given his very strong statements about the need for documentation and description of dying languages.

relationships ... progressively blurred, due to the diffusion of linguistic features throughout the [preceding] equilibrium period” (73). Thus for Dixon, the linguistic diversity in the Americas is a relatively recent phenomenon, “quite compatible with a 12,000-20,000-year period” (94), a viewpoint which the author himself characterizes as “diametrically opposed” to that of Nichols 1990 who posits a longer time-span, c.35,000 years, as an essential ingredient of such great linguistic diversity. Such views are interesting and provocative, to be sure, but inherently quite speculative. Dixon sums up his chapter by stating that “language split is almost always accompanied by expansion into new territory” (96), though he immediately is at pains to explain away the counterexample of the Nakh-Daghestanian family of the North-east Caucasus area, suggesting that the long-standing diversity there in a self-contained area “could probably only happen in mountainous country”. Perhaps, but again, we seem to have mostly speculation.

There are many more comments to be made on a variety of other topics, such as Dixon’s views on language contact (Chapter 3) or his comments on the origin of language (Chapter 5, but note also his reference to the emergence of language as “the first punctuation associated with language” (73), but space considerations demand that those areas be left to individual readers’ judgments.

It should be clear that in many ways, this is a highly personal book, one in which Dixon had an opportunity that most academics would die for, namely a chance to expound one’s views relatively unfettered. He was thus able to be at once a proselytizer and a provocateur, and to stake out strongly stated positions, yet be selective as to what he wants to highlight, which languages to discuss, which issues to focus on, etc. The result is a most interesting book; it is not always correct and

certainly not as right as the author himself may believe. I remain unconvinced that the Comparative Method fails in the way Dixon suggests in his Appendix, and the speculative nature of much of what he says about prehistory relegates these views to the domain of the interesting but unproven in my mind. Similarly, despite the attractiveness of applying notions of biological evolution to language and language change — a view that has a long history and is based on the idea that language is (like) an organism — I see the parallels more as metaphorical and not substantive.³

³See Janda & Joseph 2000 for discussion of this very point, where reference is made to the trenchant observation of Bonfante (1946: 295) that “languages are historical creations, not vegetables”.

Undeniably, though, this book makes one think! And perhaps that is the surest measure of a successful book, whether or not one is convinced by the content.

Author's address: *Department of Linguistics*

222 Oxley Hall

The Ohio State University

Columbus, OH

USA 43210-1298

E-mail: joseph.1@osu.edu

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