

On linguistics, linguists, and our times: A linguist's personal narrative reviewed

DANIEL W. HIEBER

R. M. W. Dixon, *I am a linguist*. Leiden: Brill, 2011. xvii + 391 pages, ISBN 978-90-04-19235-5, EUR 125, USD 178.

1. Introduction

R. M. W. (Bob) Dixon is one of the most renowned linguists of the past fifty years. His numerous contributions to the field include grammars of the Dyirbal (Dixon 1972), Yidj (Dixon 1977a), Fijian (Dixon 1988), and Jarawara (Dixon 2004) languages, each of which has been lauded as a model for aspiring grammar-writers (see, for example, Tsunoda 2005: 232, Chelliah & de Reuse 2010: 286), with the lattermost receiving the Leonard Bloomfield Award in 2006. He shaped a prestigious linguistics department at Australian National University during his long tenure as department head (1970–1990) and wrote several seminal articles in typology, most notably “Where have all the adjectives gone?” (Dixon 1977b) and “Ergativity” (Dixon 1979). He has published several influential books, namely, *Searching for Aboriginal languages, memoirs of a field worker* (Dixon 1984), *The rise and fall of languages* (Dixon 1997), *The languages of Australia* (Dixon 1980) – which was later expanded into his *magnum opus*, *Australian languages: Their nature and development* (Dixon 2002)¹ – and most recently his three-volume treatise *Basic linguistic theory* (Dixon 2010a, b, 2012). Lastly, Dixon and his partner Alexandra Y. Aikhenvald founded the Research Centre for Linguistic Typology (RCLT) at the Australian National University in 1996, which they then relocated to La Trobe University in 2000.²

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1. The appellation *magnum opus* is Dixon's own (p. 333). This is worth mentioning because it shows the high value Dixon places on typological description and comparison over other pursuits, as will be corroborated throughout this review.
 2. Today, after the departure of Dixon and Aikhenvald for James Cook University, RCLT is known as the Centre for Research on Language Diversity.

These accomplishments and more are detailed in the present volume, *I am a linguist*, a book which is part autobiography, part ideological monologue, part exhibition for Dixon's achievements, and part critique of academia, the discipline of linguistics, and society. It is a wide-ranging and meandering volume, almost 400 pages in length, bound in beautiful hardcover with full-color glossy photographs of prominent people and places in Dixon's life. It includes a short laudatory foreword by Peter Matthews, a "skeleton" sketch of Dixon's life and achievements, and a complete bibliography of Dixon's publications. Other references are sparing but given at the end of chapters where relevant, Dixon's preferred approach being the frequent use of the phrase "many linguists believe . . ." rather than resorting to "quotationitis", which he criticizes elsewhere (Dixon 2010a: xii).

In many ways this book is an informal synthesis of Dixon's ideas on language, some of which he has been espousing in various forums for almost fifty years now. Yet neither those works nor the present volume can claim to present a comprehensive overview of Dixon's views on language. In order to understand the "Dixonian synthesis", as it were, it is necessary to treat the ideas of this book in the broader context of Dixon's other writings. Because this volume is also semi-autobiographical rather than wholly academic, and presents a somewhat simplified version of Dixon's ideas in order to be accessible to the non-specialist (although see Section 5 for criticisms in this regard), it is even more incumbent upon interested linguists to consider this book in the context of Dixon's other works. Given this, and given that it would be uncharitable to assess one's ideas on the basis of their lay version alone, I have supplemented this review with discussions of Dixon's ideas more broadly and quotations from his other works, so as to ensure that his ideas are accurately represented and – in the cases where I give criticism – to show that I am not attacking straw men. In every case, I have found the lay versions presented here to be an accurate representation of the views expressed in Dixon's academic works elsewhere, often to their demerit. This review article thus serves a double purpose, as both a review of Dixon's latest work, and a more general compendium of the ideas expressed therein and their implications for typology.

2. Summary

This book consists of sixteen chapters, nine or ten of which can be called autobiographical; of these, one chapter (Chapter 4) and half of another (Chapter 10) are devoted to chronicling Dixon's non-linguistic pursuits – discography and fiction writing. The remainder of the autobiographical sections recounts his fieldwork, progression through academia, and other personal narratives. Though many readers will find these sections fascinating, I will not dwell on

them much further in this review, choosing instead to focus on those areas of most relevance to linguists and typologists especially.

The balance of the book, sprinkled between the autobiographical sections, consists of chapter-long harangues on each of the following: the university system (Chapter 7); linguistics as a science (Chapter 9); academic standards (Chapter 11); treatment of indigenous peoples (Chapter 12); religion and missionaries (Chapter 14); technology (Chapter 16). Numerous ideological themes recur throughout these chapters and the work as a whole, some sociological in nature, others theoretical. Many of these tenets are made quite explicit by Dixon, who summarizes them in his concluding chapter, while others can be inferred from Dixon's discussions throughout the book and his work elsewhere. Because these beliefs are the core message which Dixon hopes to convey with this book, and because they are also of the greatest relevance to academic linguists, they will be the primary focus of this review and – except for the brief summary which follows – form the basis for its organization. This, I believe, is the most faithful way to represent the content and intended message of this book.

In Chapter 1, "A day in the field", the reader follows Dixon through a (somewhat atypical) day in the life of a fieldworker, as he works through grammatical conundrums (albeit fairly simply ones, for the sake of the lay reader) and navigates local politics in the Amazonian rainforest. Chapter 2, "What is linguistics? – A journey of discovery" is where Dixon articulates his position that linguistics is a scientific discipline, subject to the same standards as other natural sciences. Chapters 3 to 6 constitute the main autobiographical section of the book, describing Dixon's family history, childhood, and arrival at linguistics, followed by his first foray into fieldwork, and his appointment to department chair at Australian National University in 1970. Chapters 8 and 10 continue the autobiography, relating Dixon's work on Yidj and Fijian into the '80s. Interspersed among these are two rather vitriolic chapters – Chapter 7, on "The role of universities", and Chapter 9, "The science of linguistics, and other approaches to the study of language". Chapter 11, "Academic standards", continues the invective, decrying the sloth, bureaucracy, and jealousy which Dixon sees as prevalent in academia, while Chapter 12, "The delegate from Tasmania", sets down a long list of grievances on behalf of indigenous peoples – down to the misspelling of Aboriginal words – and very briefly presents Dixon's views on language revitalization. Chapter 13 then details Dixon's journey "Into the Amazonian jungle", which segues into his discussion of religion, missionaries, and SIL in Chapter 14, "God and Magog in Brazil". He wraps up with Chapter 15, "A productive partnership", describing his first encounter and ensuing relationship with partner and distinguished linguist Alexandra Y. Aikhenvald. From the generous way that he heaps accolades upon her, even in his own biography, it is clear that Dixon is tremendously proud of his partner,

making this an endearing chapter to read. Dixon concludes with Chapter 16, “Living a life”, where he imparts and re-emphasizes the central messages in the book.

Having briefly summarized this work, I turn now to Dixon’s core precepts.

3. Dixon’s central tenets

The following tenets not only recur numerous times throughout the text, but are singled out by Dixon in his concluding chapter as the book’s major takeaways. For each point, I present Dixon’s views on the matter, and then offer some critique and evaluation.

3.1. *Real linguists write grammars of under-described languages*

One of the most pervasive ideas in this book is that only those who partake in the documentation and description of under-described languages deserve the appellation “linguist”. Going further, Dixon even lays out a precise, regimented career path that every linguist ought to follow in this regard (p. 38):

The prospective linguist should, first of all, learn the foundations of linguistic description by attending carefully-selected courses and by studying the classic textbooks, grammars and research reports. They must learn to recognize, make and transcribe every sound that occurs in the world’s languages; learn how to divide up words into their meaningful components; learn how to analyse grammatical constructions; learn how to formulate statements of meaning and to construct dictionary entries; learn about the ways in which languages change; learn how to reconstruct past stages of languages; learn how to investigate the social conditions of language use and the social correlates of the employment of different styles of language; learn the ways in which languages influence each other; and learn about the kinds of things that get borrowed (and why).

After this preliminary instruction, the real apprenticeship begins. This will involve fieldwork on some previously undescribed (or scarcely described) language – recording, transcribing and analysing texts; observing how people use the language in the daily round; writing a grammar and phonology; compiling a dictionary; and publishing a volume of annotated texts.

Once a linguist has mastered the fundamentals of the discipline in this way, they are equipped to go onto the next stage, comparing languages³ and working out the general principles which underlie the nature of human language.

Dixon also expresses this view in *Basic linguistic theory*:

3. In *Basic linguistic theory*, Dixon has a paraphrase of this same passage, but explicitly calls the next stage “typological comparison” (Dixon 2010a: 2). For Dixon, linguistic typology and linguistics as a science are largely the same endeavor. Section 3.3 discusses this in greater detail.

For more than four decades I have been doing linguistics in the true sense of the word – undertaking immersion fieldwork, writing grammars, compiling lexicons. (Dixon 2010a: xii)

Anyone setting out to properly learn linguistics should choose a language which has not been (or has scarcely been) described, and whose speakers welcome co-operation with a linguist. If the language is still spoken on a daily basis the ideal course is to live (for, say, nine to twelve months) as a part of the community, being exposed to the language, gradually gaining proficiency and observing how it is used. (Dixon 2010a: 2)

Dixon is also rather dismissive and belittling towards those he considers to be false linguists, as evidenced by the following passages:

There is a further group, not mentioned before (and not deserving to be) who are not formalists and do not attempt to write grammars of languages, but essay statements of 'typological universals' on the basis of reading grammars of every imaginable quality or lack of quality. These 'armchair typologists' have not been through the indispensable apprenticeship of analysing a previously undescribed language in terms of basic linguistic theory. They do not have the perceptiveness or insight to discern what is a competent grammar. (pp. 186–187)

There are some linguists, of a different ilk, who avoid the travails of fieldwork and do not themselves produce a grammar, lexicon, and text collection for a previously undocumented language, but attempt straightaway to work on linguistic theory. This is rather like a biologist who has only observed animals in picture books (or perhaps in a zoo) and then proceeds to statements about the nature and habits of a particular animal, or about animals in general. Too often, people who haven't worked on a grammar themselves can't distinguish – on looking through the literature – between an adequate grammar and a poor one; they simply don't have the grass-roots experience to enable them to know what to look for. (Dixon 2010a: 2–3)

The immediate consequence of this position for typology is that large typological sampling methods in the tradition of Greenbergian typology are ruled out as unsuitable methods of investigation. The implications of this view for methodology in typology will be more fully discussed in Section 4.

Another unfortunate consequence of these views is that, by rejecting as valid any research on topics other than endangered language description and typology via fieldwork, Dixon relegates many important and productive areas of linguistics to pseudoscience. For example, the important work done by William Labov on variationist sociolinguistics has not only contributed greatly to our understanding of social dialectology and the process of diachronic linguistic change on the scale of a generation, but has helped combat many pernicious myths about nonstandard dialects, and provided legitimacy to a highly

marginalized linguistic group. Yet by his own standards, I think Dixon would have to say that this work does not count as linguistics proper, because of its focus on unendangered varieties of the world's most dominant language.⁴ In addition, Dixon has great praise for certain linguists who nonetheless have never undertaken a descriptive grammar of a language, such as Peter Matthews and Lord Randolph Quirk (p. 270). He is thus rather inconsistent in the application of his definition of "linguist".

Dixon often idolizes the stereotype of the fieldworker as laid out by Bowerman (2008: 2), that is, "some rugged individual who spends large amounts of time working with speakers of 'exotic' languages spoken in remote areas, [and who] lives a life of deprivation and austerity, comforted and nourished by weird insects". Anybody who does not fit this bill is deemed less of a linguist. The following passage is illustrative, and appears on the first page of the book (p. 1):

The well-known languages are well enough known. Working on one is like adding a bit of shading here and there to the map of some familiar land; this is a comfortable task, which can be combined with a comfortable lifestyle. But, far away from electric power and flush toilets, there are languages of strange and wonderful mien.

One reason for Dixon's views is that he sees the science of linguistics as being in its infancy. "[L]inguistics has languished, while other disciplines raced ahead", he writes, and it is still "tied too tightly to the structures that recur in the familiar languages of Europe. [...] This is an under-developed discipline" (p. 25). For Dixon, the first and most fundamental task of linguistics is to collect as much data as possible from the world's languages. Only once this task is complete can the discipline transcend to the 'why' questions, a realm of inquiry which Dixon believes to be "wide open" and largely untouched: "only a few of the 'why' questions can be provided with an answer, at this early stage in the development of linguistics" (p. 31). One can also see how this focus on data collection comports with Dixon's paradigm of linguistics as a natural science (to be discussed further in Section 3.3.2).

3.2. *A language and its features cannot be adequately described without their complete linguistic and social context*

Dixon emphasizes the necessity of context in two respects. First is the importance of social context in coming to understand the grammar and usage of a language: "To be properly understood, a language must be studied in a community where it is spoken" (p. 23). Dixon then gives examples of linguistic

4. Crowley (2007) offers an insightful critique of an earlier formulation of this view by Dixon as well.

phenomena, such as the coding of geographical features in verbs of motion, which would be difficult to understand without being aware of the social and environmental context in which the language is spoken.

Second is the importance of linguistic context in understanding specific words and constructions in the language: "Just as a language can only properly be understood in terms of its cultural context, so each sentence must be considered in terms of the context in which it is uttered" (p. 24). This position is actually a corollary of Dixon's more general perspective on language, which he takes from Saussure: "a language must be treated as a complete system, every part of which has meaning and significance only in the way it relates to other parts of the system" (p. 176; emphasis removed).⁵

The above principles help to reinforce Dixon's belief that fieldwork is a necessary part of being a linguist, since language description is inadequate without it. In the latter respect, at least, he follows similar work by Franchetto (2006), Berge (2010), Buszard-Welcher (2010), and Harrison (2005), which emphasize thoroughness in documentation. One issue with this approach is that, if one insists on having the "complete social context" in conducting language documentation, this has the potential to disenfranchise highly endangered languages or displaced persons. Boynton (2011) has noted the direct effect of such views in the distribution of Native Title payments in Australia, for example. It should also be quite evident that there is no such thing as a "complete" description of a language. Even extremely comprehensive grammars fall short in this regard.⁶

3.3. *Linguistics is a natural science in keeping with the scientific method, based around a single unifying theory of language*

Dixon spends significant portions of this book (Chapters 2 and 9) arguing for his views on linguistics as a natural science. On p. 24, he defines linguistics as

the scientific study of the nature of human language. [...] Linguistics aims to discover what is common to all languages, and the ways in which languages may differ.

A similar definition is given in *Basic linguistic theory* (Dixon 2010a: 5), and both these definitions are nearly identical to common definitions of language

5. In his earliest work on the topic (indeed his first major publication; Dixon 1963), Dixon also credits J. R. Firth's and Halliday's neo-Firthian approaches for many of his views on the importance of linguistic context, which were influential during his time at Edinburgh.

6. See, for example, Claire Bower's incredibly thorough grammar of Bardi, which, despite its scope, explicitly makes no claim to comprehensiveness (Bower 2012: xxxiv).

typology (e.g., Daniel 2011: 44). For Dixon, the science of linguistics essentially is language typology, a view shared by some other typologists, such as Shopen (2007: xiv).

3.3.1. *The four features of linguistics as a science.* Dixon believes that there are four important aspects of any natural science: description, explanation, prediction, and evaluation. His definition of linguistics as typology may therefore be simply a reflection of his belief that linguistics is still in its nascent stages, and so he emphasizes the description of linguistic diversity – for him, the first phase of the science. In this regard, his views are similar to Croft's (2003: 2), which state that each of the different definitional strands of typology correspond to a different aspect of the scientific process, and that typology thus "represents an EMPIRICAL SCIENTIFIC approach to the study of language".

By "description", Dixon essentially means grammatical description of a language and typological comparisons with other languages. But there are significant problems with Dixon's conception of typological comparisons, primarily his rather incautious utilization of crosslinguistic categories, which has been called into serious question by Croft (2001) and Haspelmath (2007, 2010), among others. Dixon's position is best illustrated with an analogy: "It is as if there were a universal treasure chest of grammatical categories, and each language is permitted to make a limited selection from it" (p. 26).⁷ This is what Croft (2001: 10) calls the "smörgåsbord" approach, which "asserts that the set of categories and relations are available to all speakers, but speakers of some languages do not avail themselves of all the categories and relations available to them". Similar positions are elaborated throughout the first several chapters of Volume 1 of *Basic linguistic theory* (Dixon 2010a: 9, 11, 14, 27, 50).

Dixon is next concerned with explanations for the features we see in and across languages, and notes that such explanations can be historical, cognitive, sociocultural, etc. At the same time, the types of 'why' questions Dixon emphasizes are not formulated quite the way one would expect. He states, for example (p. 187), "It is not sufficient to say 'this is the way it is, the language is of this sort'. Why is this so? Spanish has two genders and Turkish none. Why?" But the fundamentally interesting question here for linguistic science is not why Spanish has two genders per se, to which a historical explanation can be given,

7. Additional context is necessary here, lest I be accused of being ungracious in my interpretation of Dixon. The passage continues as follows: "[I]f you have six genders you may only take two tenses and no evidentiality. Except that it's not like that. Almost nothing in life is arbitrary. There must be a REASON for Swahili having eight genders and Tuyuca five evidentials." Dixon's objection here is NOT against the idea of a universal bag of grammatical categories, but rather against the idea that the distribution of these categories is arbitrary. I believe the "grammatical bag" analogy is an accurate representation of Dixon's views – much to their demerit.

but rather “Why do languages have abstract categories like grammatical gender at all? What are the functional motivations for why such systems develop?”⁸ But Dixon focuses on historical, sociocultural, and Whorfian explanations, often to the exclusion of others.

Next is prediction, which Dixon exemplifies with the predicted existence of Pluto using Newtonian mechanics long before its actual discovery.⁹ Dixon believes linguists should make several kinds of prediction: typological predictions regarding the meanings associated with some part of a grammar (i.e., in languages with a small set of adjectives, those adjectives probably refer to properties like dimension, age, or value); typological predictions of intra-language correlations (e.g., languages will make more distinctions in their positive than their negative tenses); predictions regarding possible and impossible features in language; and future predictions regarding how languages will change over time.

A significant problem with this lattermost type of prediction, however, is that any predictions regarding future changes to language must necessarily be *ceteris paribus* statements to the effect of, “all other things being held constant, x will occur”. But of course in the real world all other things are never held constant, and so such statements are not really predictions proper, as much as they are theoretical claims about the nature and workings of language. These theoretical claims can then only be refuted by other theoretical considerations; the empirical evidence is unhelpful in choosing between competing theories, because it can always be claimed that there were other mitigating factors at work. Therefore, *ceteris paribus* statements like the ones Dixon wishes to make about the future of languages can never be empirically verified or falsified. I

8. Dixon actually provides interesting answers to these questions in *Basic linguistic theory*: “a large number of meaning contrasts are mapped onto a restricted set of grammatical choices” (Dixon 2010a: 33, 96); a single contrast like grammatical gender is useful because it can simultaneously code for a variety of semantic distinctions. But he does not make such connections in this volume.

9. Dixon has his facts somewhat confused in this example. He states that Percival Lowell suggested in 1915 that perturbations in Uranus’s orbit predicted the existence of another planet, which led to the first observation of Pluto in 1930. This actually conflates two separate episodes in the history of astronomy, and gets the dates wrong besides. In 1840, Urbain Le Verrier predicted the position of Neptune (not Pluto) based on perturbations in Uranus’s orbit, and Neptune was then discovered in the late nineteenth century. Late-nineteenth-century astronomers ALSO observed perturbations in NEPTUNE’s orbit, leading them to think that a ninth planet (Pluto) existed as well. Based on these predictions, Lowell started his famous Planet X project in 1906 to hunt for the new planet. In 1915, the project unwittingly snapped a photo of Pluto without realizing what it had. Only in 1930 did the project finally photograph and confirm Pluto’s existence.

Dixon actually could have made his task easier here and simply taken a page from the history of linguistics, citing Ferdinand de Saussure’s reconstruction of laryngeals in Proto-Indo-European and their subsequent discovery in Hittite.

also know of no study in historical linguistics that makes claims to the effect of, “other things being held constant, this language will lose its case marking” or “this language will lose its irregular verb forms”. Historical linguists appear to be in agreement that such diachronic prediction is not possible (Campbell 2004: 326–328; Trask 1996: 127; Hale 2007: 35). Yet these are precisely the types of claims that Dixon makes (pp. 34–35).¹⁰

Finally, there is evaluation. For Dixon, evaluation in linguistics means asking, “Are some grammatical and phonological systems – indeed, are some languages – better for a certain purpose than others?” (p. 173–174). He elaborates on this idea in *Basic linguistic theory* (Dixon 2010a: 4):

It is an accepted procedure to evaluate the worth of different economic or political systems. We have outgrown the mistaken colonialist idea that some languages are significantly more ‘primitive’ than others. All languages are ROUGHLY equal in terms of overall complexity. But surely they are not all of PRECISELY the same value. Might not some languages be better than others, for certain purposes? Is one language easier to learn than another? Does one language provide a superior framework for deep discussion of kinship relationships, or of subtleties of taste, or for assessing the worth of cattle herds, or for sports commentary, or for philosophical introspection?

There are several problems with this approach to evaluation in science. First, it is decidedly NOT within the scope of disciplines like economics to evaluate the worth of different economic or political systems. As the pre-eminent economist Ludwig von Mises states, “[s]cience never tells a man how he should act; it merely shows how a man must act if he wants to attain definite ends” (von Mises 1949 [2010: 10]). Economics is a purely descriptive science, which states that IF you want to achieve end x, then means y is the most causally efficacious way of doing so. It is an entirely *wertfrei* methodology. Linguistics, a similarly descriptive science, is no different in this regard.

Second, Dixon misunderstands the function of evaluation in the natural sciences. In the context of the scientific method, evaluation refers to the assessment of (often competing) theories or hypotheses, not the object of study itself. Theories are evaluated in terms of whether they are simple, account for the data

10. Dixon could reply here that predictions about language futures are not IMPOSSIBLE, but merely IMPRACTICAL, given the number of factors which must be taken into consideration. Modern statistical techniques may be capable of isolating various factors, thus making prediction both possible and practical. But there exists a more powerful objection to this stance, which is that much of how language changes over time depends on the subjective choices and valuations of individuals regarding their language use. Such personal valuations and choices are not open to prediction or enumeration, and therefore any predictions based on them are likewise impossible. For more on innumerability when it comes to human preferences in science, see von Mises (1949 [2010: 55–57]).

they take as their object of study, logically cohere, etc. Dixon's proposal, by contrast, is like asking whether gravity is a better attractive force than electromagnetism. Evaluation has nothing to do with, for example, whether neutrons are better than protons.

Third, to say that a language is better at accomplishing a certain purpose than another is an entirely subjective valuation which has no basis in linguistic science. Dixon gives two examples of evaluative claims: a language which has four ways of saying 'you', about which he says, "Surely this is useful, and thus good?"; and the numerous irregular verbs in English, which he states are, "from the point of view of efficiency of communication, a useless complication" (p. 37). But the question then becomes, good for who? In comparison to what? Don't irregular forms correlate with the most frequent forms in the language, and are thus in that way more efficient? And couldn't a language with four forms of 'you' be considered less efficient, rather than more? Did not each of these "inefficiencies" evolve in response to specific historiocultural conditions? All these valuations depend on the perspective of the individual making them, and the particular circumstances and context in which they are used. Certainly, there are times when DESCRIPTIVE evaluations can be given – such as the statement that wings are better than tails for flying, IF flying is the criterion of evaluation – but Dixon here has conflated the descriptive and normative uses of the terms "better" and "worse". When used as categorically normative statements as Dixon intends them, these terms have no place in science, and should not be employed by a scientist in their description of phenomena.

3.3.2. *Scientific theories and p-theories.* Dixon takes there to be two different approaches to theory-creation (pp. 167–168):

THE SCIENTIFIC APPROACH entails observation of the full gamut of facts of human languages, offering description, explanation, prediction and evaluation, generally proceeding by inductive generalisation. All practitioners work in terms of a common, cumulative linguistic theory (in the singular), similar to the theory underlying each natural science.

THE PHILOSOPHICAL (or social science) APPROACH gives rise to a number of competing 'schools', and a corresponding set of 'theories' (in the plural). This is similar to schools of thought in literature, economics, and the like. Far from being cumulative, each school puts forward its own set of a priori ideas concerning some aspect of language, and looks at facts from individual languages only insofar as they relate to its specific agenda.

What Dixon has in mind by a "scientific theory" is what people mean when they say "chemical theory" or "atomic theory", and it is this sense which Dixon believes should apply to linguistics. P-theoretic approaches to language are not considered linguistics proper (p. 175):

The p-theorists conceive of a grammar as a self-contained entity, a sort of algebraic system, which exists in and for itself. They do not see grammar as a bridge between speaker's intended meaning and hearer's understanding, in a context of social situation, within an ethnically-based society. Their interest is in the nature of grammar, as an intellectual object. There is nothing wrong with this. It is just that it does NOT fall within the scope of a linguistics which is conceived of as the science of language (in the way that biology is conceived of as the science of living organisms).

Dixon's dichotomy comports rather well with a well-known distinction in philosophy of science, put forth by Laudan (1977) and summarized by Croft (2003: 280–281):

Laudan argues that there are two different ways in which the term 'theory' is used: to refer to analyses of specific phenomena (e.g. DuBois' theory of ergativity), or to very general and not so easily testable frameworks (e.g. functionalist theory, Laudan 1977: Ch. 3). Laudan calls them RESEARCH TRADITIONS, which corresponds closely to the term 'approach' as it is used in linguistics [...] Laudan's 'theories' may be less confusingly called ANALYSES [...]

Laudan, however, would not say that a science proper consists of merely a single unifying research tradition, as Dixon does.

Two important aspects of Dixon's conception of linguistic theory are that it is (i) cumulative, and (ii) unified. According to him (p. 167),

[e]ach of the natural sciences has a single theory – chemical theory and the like. A natural science does not have a swathe of competing 'theories', each with a different agenda and a dismissive attitude towards other 'theories'.

It is interesting that Dixon does not see linguistics as a social science, given how extensively he stresses the importance of sociocultural explanations for phenomena. I am not sure he has a very clear definition of the dividing line between "natural" and "social" sciences, except perhaps to distinguish them by their presumptive methodology. And though it is not present in the above quotes, Dixon often uses the phrase "basic linguistic theory" to refer to the cumulative theory of linguistics he has in mind. It is apparent that Dixon sees his three-volume *Basic linguistic theory* as representing, in an atheoretical manner (or rather, a-p-theoretical manner), the cumulative body of knowledge in linguistics (p. 345): "I have taught many fine linguists but (thank goodness) there has never been any hint of a 'Dixonian school'. I try simply to impart the principles of linguistics as a science."

Some criticisms are in order. First is that the cumulative approach to scientific theory has been thoroughly debunked in the scientific literature. And here I quote the eminent historian Murray Rothbard (1995 [2006: ix]):

The continual progress, onward-and-upward approach was demolished for me, and should have been for everyone, by Thomas Kuhn's famed *Structure of Scientific Revolutions* [Kuhn (1962)]. [...] Bringing the word 'paradigm' into intellectual discourse, Kuhn demolished what I like to call the 'Whig theory of the history of science'. The Whig theory [...] is that scientific thought progresses patiently, one year after another developing, sifting, and testing theories, so that science marches onward and upward, each year, decade or generation learning more and possessing ever more correct scientific theories. [...]

Kuhn, however, shocked the philosophic world by demonstrating that this is simply not the way that science has developed. Once a central paradigm is selected, there is no testing or sifting, and tests of basic assumptions only take place after a series of failures and anomalies in the ruling paradigm has plunged the science into a 'crisis situation'. One need not adopt Kuhn's nihilistic philosophic outlook, his implication that no one paradigm is or can be better than any other, to realize that his less than starry-eyed view of science rings true both as history and as sociology.

A second criticism is that it is naïve to expect that other sciences are not subject to the same sort of theoretical fracturing found in linguistics. One need look no further than recent theories in physics (namely string theory), which deny point-particle physics in favor of multi-dimensional particle physics, to see this.

Finally, I must say a few words on Dixon's supposedly p-theoretically neutral approach to linguistics. He notes that many descriptive linguists are accused of lacking any sort of theory, and rightly points out that this is nonsense: "One can't do any description without a theoretical basis" (p. 184). Dixon is more correct than he realizes. Descriptive linguists are certainly engaged in theory-formation, but if they disdain overt theorizing, they will be neglecting to think through the assumptions and claims implied by their descriptions. In doing so, they do not produce a work that is (p-)theory neutral; instead what they produce is a work whose theory is poorly conceived and possibly inconsistent. For example, a linguist who calls something a noun in one language is making definite theoretical claims about the existence of nouns and their properties, but vague as to whether they believe NOUN is a crosslinguistic category or simply a comparative concept (Haspelmath 2010). This has explicit consequences for their description and linguistic theory generally. To say that there is a universal bucket of crosslinguistic categories on the one hand, or that crosslinguistic categories don't exist on the other, are two radically different conceptions of the nature of language and language universals – that is, fundamentally different THEORIES of language. Believing that one's theory is THE correct, cumulative theory of language nevertheless doesn't change the fact that this is just one theoretical position among many, and is thus neither p-theory neutral nor scientific-theory neutral. Bower (2008: 10–12), Drescher (1998),

and Dryer (2006) offer similar critiques of purportedly theory-neutral description. It is thus not surprising that Dixon, even while claiming to acknowledge the importance of theory in description, still falls prey to ambiguities in terminology or theoretical position, as will be seen in Section 4.

A general criticism of all these views on linguistics as a science is that they are largely uninformed by the literature in philosophy of science, which any self-styled scientist should be aware of. While Dixon cites positivist philosopher Ernest Nagel's *Structure of science*, which talks about "the almost complete unanimity found [...] in the natural sciences as to what are matters of established fact" (Nagel 1961: 448), he seems unaware of other crucial works in the philosophy of science, such as Thomas Kuhn's *Structure of scientific revolutions* (which soundly refutes Nagel's position; 1962), Karl Popper's *Conjectures and refutations: The growth of scientific knowledge* (1963) or *Logic of scientific discovery* (1959), David Hume's discussion of the problem of induction in *Enquiry concerning human understanding* (1748), or the large body of work which has thoroughly refuted positivist approaches to science which Dixon himself seems to adopt. While in one of his earliest works (Dixon 1963) Dixon actually criticizes logical positivist approaches to language like those of Rudolph Carnap, he is decidedly in agreement with the positivist approach to science generally, which can be summarized by the statement that "empirical data is the exclusive source of our knowledge". Such a view can be seen in the following passages:

I had no hypothesis. What I did was systematically elicit data, and then cross-elic it. Lo and behold – an important theoretical insight emerged, all on its own. (p. 103)

A THEORY is obtained by generalisation upon pattern correlations. (Dixon 1963: 12)

Anyone who believes, as Dixon does, that theory-creation is a purely inductive endeavor definitionally merits the label "positivist".

3.4. *State your opinions directly; spurn political correctness; eschew bureaucracy*

Dixon frequently states his position regarding political correctness, which is that such hyper-correct discourse tends to shut down substantive conversation. While some of Dixon's complaints might be seen as the typical "o tempora, o mores!", his critiques in this regard are occasionally insightful (p. 351):

'Political correctness' takes many forms. It frequently entails not being able to say what you think. [...] The difference is that before you could freely express [such feelings], now you can't. One day recently, Professor B (a scholar I respect)

offered to present me with a book which he'd been given to review but considered to be too execrable to keep on his own shelf. 'But you gave it a good review!' I exclaimed. 'Well one does', came the reply.

It is unfortunate for our discipline if such stories are common, and if we as scholars feel unable to objectively critique others' work for fear of retribution.

Dixon also makes many observations about the way bureaucracy hinders academic work, and his advice to younger scholars in dealing with this is as follows (p. 339): "I have NEVER tried to become familiar with every bit of red tape, and then work out what it may allow. [...] [F]irst decide what you want to do, and then work out how to achieve this." He notes how extremely difficult it is to fire even the worst professor or researcher, and the ensuing decrease in standards this creates. He is among those who believe that universities would be greatly benefitted by being run more like a business, rather than the medieval guild structure they still cling to.

One of his more controversial critiques is of ethics boards, which he sees as entirely unnecessary for most fieldwork in linguistics. While many linguists surely share his frustration with the ethics approval process, Dixon does not seem to be aware that standards exist for addressing these problems (e.g., Bower 2010). Still, most documentary linguists will probably find themselves in agreement with Dixon's concluding statement in this section (p. 86), even if it perhaps misses the larger point:

Plainly, a linguist cannot work in an indigenous community unless they are wanted there. But this has to be worked out on the basis of friendship and trust. To demand written certification demonstrates not only ignorance of the cultural milieu in which work on small minority language is carried out, but also a contempt for that culture.

3.5. *Religion is a crutch, and belief in religion is indicative of a lack of intellectual rigor*

Dixon's attitudes towards religion are best summarized in the following passage (pp. 293–294):

Belief in a god is comforting. It absolves one from responsibility (other than that of belief and worship). And it can fulfill a social role. In olden days the Lord of the Manor would on occasion hint to the village priest (whose vicarage and stipend he provided) about what to preach. No sedition, thank you, and render a proper tax unto the Lord – of the Manor, not the other one – if you please.

Diversity of opinion makes the world an interesting place. And respect for the opinions of others causes it to be congenial. Yet many Christians (and no doubt adherents of some other faiths) believe that they have a monopoly on goodness.

Chapter 14, “God and Magog in Brazil”, is primarily a critique of the sort given above, on missionary activities in Brazil and religion generally.

The reason I characterize Dixon’s views as believing that “religion is indicative of a lack of intellectual rigor” is because passages like the above imply that people only believe in religion for emotional or psychological reasons. It discounts the possibility of a rationally considered belief in a god. Consider also the following (p. 298):

I devised a simple test for distinguishing types of missionary. Some would refer to the ‘doctors’ or ‘medicine men’ of Indian tribes as ‘shamans’ or as ‘pajés’ (the Portuguese term for shaman). These were basically good people, who would offer Christianity as an alternative or addition to the indigenous religion, which they would not try to suppress. And they would help Indians protect their territories from timber-getters and gold-seekers. They were, on the whole, sound linguists (the type we had welcomed to MA and PhD courses). The other ilk referred to shamans as ‘witchdoctors’. They would attempt to stamp out all non-Christian practices [...] and they were – with scarcely an exception – pretty poor-quality linguists.

Interpreting this generously, all Dixon is saying is that there are two types of missionaries – ones who are culturally narrow-minded and ones who are not. This seems fairly innocuous. After all, the statement is practically tautological, true of any class of people, not just missionaries. Why then does Dixon raise the point here, in his discussion of missionaries? It seems clear that Dixon believes there to be a bias among religious individuals towards cultural narrow-mindedness, and a correlation between belief in religion and irrationality generally. And since, following this logic, religious people tend to lack intellectual rigor and cultural appreciation, they are likely to do a poor job with things like writing grammars, which requires these skills; those with more moderate religious views are probably smart enough to do good-quality work.

One particularly egregious statement is also worth comment: “Christianity started among white people and was then spread by their missionaries among black and brown” (p. 296). Not only is this completely false,¹¹ it illustrates the overly simplistic view of race relations presented throughout the entire book. The view presented in this book is that the world divides neatly into white and

11. Christianity began in the Levant among the peoples of the Middle East, and then spread to the Near East before firmly taking root in Europe as the state religion of Rome in 380 CE – quite the opposite from what Dixon states here. Perhaps what Dixon has in mind is the subsequent spread of Christianity out of Europe, beginning during the Age of Discovery in the sixteenth century, when Spanish scholastics from the School of Salamanca founded missions throughout the world for the purpose of converting the peoples in those regions (and simultaneously ushered in the birth of documentary linguistics). Dixon’s statement would then have to be considered both a drastic oversimplification and misrepresentation of these facts.

other, in which white people – along with their associated ideas and customs, such as (according to Dixon) religion and capitalism – are bad and largely responsible for the world's ills.

3.6. *Academic standards have largely vanished, in part because of technology, which promotes intellectual sloth*

Dixon devotes Chapter 11 to academic standards, or rather the lack thereof. He shares examples of how bureaucracy, politics, and academic in-fighting have crippled serious academic work. Some of his critiques are insightful, and good advice for younger academics to remember, e.g., the following (p. 344):¹²

There are a number of implicit assumptions in academia today, all basically unfounded. One is that everything which has appeared in print must be useful and correct. Another is that all opinions are equally good. A further idea is that any new work on a particular topic is necessarily superior to previous publications on that subject.

However, much of the chapter is excessively catty and gossipy. Seven whole pages are devoted to excoriating a certain professor whose methods Dixon disapproved of, while another five are spent ridiculing Oxford's attempt to recruit a department chair. Some of his accusations are actually quite serious, including political, academic, or financial corruption, but a great deal of his criticisms simply accuse his targets of incompetence. In general, he spends an inordinate amount of time in the book condemning the habits and actions of others. His other critiques include grade inflation, work ethic, jealous "mediocrities" who loathe others' success, lack of dedication to teaching, and the uselessness of conferences. In all cases, Dixon holds himself up as a model to be emulated by comparison.

One reason (among many) that Dixon points to for decreasing academic standards is technology. In his view, "computers encourage laziness, which leads to a poorer product" (p. 346), and e-mail makes us less thoughtful, because it reduces the opportunity cost of sending trivial things. Dixon laments the disappearance of the old-fashioned, well-ruminated handwritten letter, and proudly boasts that he has never composed any serious work except by hand (later typing up his handwritten notes for publication). It is hard not to dismiss these grievances as another example of "o tempora, o mores!". In some cases, Dixon's views on the way digital composition affects writing and intellectual abilities run counter to objective descriptive analysis, such as the idea that digital technologies make us somehow less thoughtful. It is hardly surprising that

12. It would be uncharitable of me not to point out that this passage potentially redeems Dixon's position in relation to the Whig theory of the history of science. But this appears to simply be an inconsistency on Dixon's part.

the PROPORTION of deep intellectual output is significantly lower when technology has allowed the TOTAL OUTPUT to increase so voluminously. Academics can now write trivial things – and lots of them – in ADDITION to the many thoughtful works they produce, at significantly less personal cost in terms of time and money. Such considerations have been documented in Naomi Baron’s excellent books on the way that the cultural practices of reading, writing, and composition have changed over time with the emergence of new technologies (Baron 2003, 2008). Finally, Dixon makes no mention of the fact that technology has been utilized to great effect in areas such as historical linguistics, corpus linguistics, and also typology.

4. Implications for typology

Having summarized Dixon’s views on language, we should now ask what their implications are for typology, and what the typological literature has to say about them. Since these ideas are important to understanding Dixon and raised in *I am a linguist*, but not necessarily given thorough treatment, I will rely heavily on *Basic linguistic theory* for this discussion. I will first discuss Dixon’s theoretical commitments, including Basic Linguistic Theory, focusing on the issue of crosslinguistic typological comparison (the fundamental exercise in typology). Then I will note some methodological issues which Dixon’s positions raise for typology.

4.1. Typological comparison

One of the most striking aspects of Dixon’s approach to typology is his adherence to universally-instantiated crosslinguistic categories in language: “[i]t does appear that every language has open classes of words which can be felicitously named noun, verb, and adjective, although the defining criteria vary between languages, as do their full semantic and functional ranges” (Dixon 2010a: 27). He also states that “Basic linguistic theory is concerned with comparing similar phenomena between languages and to achieve this it is convenient to apply the same label to similar phenomena in different languages” (Dixon 2010a: 11). But while such labels are certainly convenient, the important question is whether they are methodologically and theoretically sound. It is ironic and perhaps revealing that, as Haspelmath (2010: 664) points out, Dixon’s approach to typological comparison is most similar to the generativist models of language he critiques so heavily (e.g., Baker 2003). This position is what Haspelmath (2010: 664) calls “categorical universalism”, and utilizes what Croft (2001: 45) terms “global categories”, which are “instantiated in particular constructions in a particular language”.

Dixon argues that belief in crosslinguistic categories is justified because one can always find properties which distinguish them in different languages: “de-

tailed examination (over the past thirty years) of languages [assumed to lack adjectives] suggests that, once again, when all relevant facts are taken into account an adjective class can be (and should be) recognized for every language, distinct from noun and verb classes" (Dixon 2010b: 62). He states that such a class should be recognized for its "role in theoretical generalization", i.e., that we would lose useful generalizations about the class of property concepts crosslinguistically if we did not recognize a universal adjective class (Dixon 2010b: 66–68).¹³

This is arguably a case of what Croft (2001: 30–32) calls "methodological opportunism" (as well as "lumping"): "[c]ross-linguistic methodological opportunism uses language-specific criteria when the general criteria do not exist in the language, or when the general criteria give the 'wrong' results according to one's theory". In this case, Basic Linguistic Theory calls for the recognition of an adjective class when any distinguishing criteria can be found, and so that category is taken to exist for different languages.

A number of criticisms can be made of this approach, all of which are nicely summarized in various sections of Croft (2001, 2003). As I cannot do justice to the objections here, the reader is encouraged to consult Croft's works in their entirety. In short, Dixon lacks an objective way to establish criteria for distinguishing word classes across languages, so that any choice of criteria "looks suspiciously like serving a priori theoretical assumptions" (Croft 2001: 31). Dixon also gives no reason for why word classes should not be further subdivided, based on the same kinds of distributional evidence he used to distinguish them in the first place. Croft continues, "[w]ithout prior agreement or some principled means for specifying which constructions define a category across languages, analysts can use whatever constructions they wish in order to come to whatever conclusions they wish". The following critique from Croft (2001: xiii) is also relevant:

A large class of research articles – again, both 'formalist' and 'functionalist' – is devoted to claiming that phenomenon X in language Y is really a passive, or really is not a passive. Such research discounted or ignored the opposing evidence, which made me uncomfortable. It also missed the point, which was that phenomenon X was interesting and challenging precisely because it sort of was a passive but sort of wasn't at the same time; both its passivelike and unpassivelike syntactic properties were equally important. Just as traditional grammarians tried – unsuccessfully – to fit modern European languages into the mold of Classical Latin and Greek, modern linguists are trying to fit languages of the world into the mold of 'Standard Average European'.

13. Note Dixon's inconsistency here, claiming on the one hand that adjectives ARE or CAN BE recognizable, and on the other that they SHOULD BE recognizable.

Interestingly, Dixon appeals to the theoretical construct of a prototype when describing his crosslinguistic categories, a notion typically attributed to Croft himself (2003). One would therefore expect the two theoretical approaches to be congruent. However, as mentioned earlier, Dixon's disdain for overt theorizing leads him to forego clarification of precisely how he means to use this term. It is clear, however, that he does not mean the precisely-defined typological markedness sense that Croft puts forward (2003). My impression instead is that when Dixon uses the term "prototypical", what he actually means is "most frequent".

It must be noted that Dixon would deny that his approach constitutes categorical universalism (Dixon 2010b: 65–66):

Unlike many formal theories, basic linguistic theory does not consist of a list of components which every grammar **MUST** include. What it does, instead, is provide a range of theoretical tools and a pool of conceptual categories, each of which **MAY** be utilized in the grammar of a particular language if it fulfils a useful role there in description and explanation. [...] Basic linguistic theory does **NOT REQUIRE** that distinct classes of noun and verb be recognized for each language. Our conclusion in the previous chapter that it is appropriate to identify these two classes in every language is based on empirical investigation, rather than it being a theoretical postulate. It is the same with the adjective class.

Dixon thus sees it as simply an empirical fact – a brute fact of the world, to make the positivist connection – that all languages have adjectives, and this fact is merely documented by Basic Linguistic Theory, rather than assumed by it. But it remains unclear how Dixon can hold to this position and objectively motivate the Adjective category without running afoul of Croft's potent criticisms.

4.2. *Basic Linguistic Theory*

Basic Linguistic Theory is not entirely novel. Though Dixon appears to be unaware of him (or at least, in his adherence to anti-quotationitis, has neglected to cite him), Lehmann (1989) describes an accumulated metalanguage which he calls "general comparative grammar". Dixon's earlier works also show the idea in its incubation as far back as 1966 (p. 101):

Dyirbal is a fairly unusual language, right? Why wrap it up in some obscure, posturing formalism? I decided there and then (in late 1966) to turn over a new leaf. I'd write the grammar of Dyirbal – and everything else I did – in as straightforward a manner as possible, in order to make it maximally accessible to the reader.

The idea develops some maturity in *Ergativity* (Dixon 1994: xvi):

The discussion in this book is in terms of the established theoretical ideas of linguistics, as these have developed over the past two thousand and more years [...]

To have chosen any one of [the current theories in vogue] would have constrained the presentation. I consider that the facts, explanations and generalisations given here are most usefully shown through a general typology theory.

The term “basic linguistic theory” was finally made explicit in *The rise and fall of languages* (Dixon 1997).

Dixon also describes Basic Linguistic Theory in another manner (Dixon 2010a: 182):

Basic linguistic theory provides a flexible, analytic framework in terms of which the grammar of each individual language can be formulated. It furnishes an array of grammatical categories and construction types – together with varieties of interrelations between them – from which appropriate choices are made.

Dryer (2006: 207) notes that, under this conception, Basic Linguistic Theory is a descriptive theory, as opposed to an explanatory theory, defined as follows:

Descriptive theories (or theoretical frameworks) are theories about what languages are like. They are theories about what tools we need in order to provide adequate descriptions of individual languages. Explanatory theories (or theoretical frameworks), in contrast, are theories about why languages are the way they are.

Dixon, however, does not adhere to this distinction in either *I am a linguist* or the *Basic linguistic theory* volumes. He often includes functional-typological explanatory theories in his discussions of what are otherwise descriptive theories, conflating the two (e.g., his discussion of functional explanations for Swahili genders and the analogy of a “universal treasure chest of grammatical categories”; p. 26). Moreover, this conflation is present in the very conception of Basic Linguistic Theory: based on the first quote given above, Basic Linguistic Theory would seem to be a purely descriptive theory; but in other places Dixon has described it as “the theory of linguistics as a natural science” (Dixon 2010a: 4). This latter conception would necessarily require that Basic Linguistic Theory also contain explanatory, predictive, and evaluative elements, according to Dixon’s characterization of what constitutes a science. Dixon is thus inconsistent regarding whether Basic Linguistic Theory is to be conceived of as primarily a descriptive theory, an explanatory theory, or both (this final option again evoking similarities to generativist approaches, which claim to be both descriptive and explanatory; cf. Dryer [2006: 216–217]).

Dryer (2006: 210–211) also suggests the following interpretation:

Basic linguistic theory differs most sharply from other contemporary theoretical frameworks in what might be described as its conservativeness: unlike many theoretical frameworks that assume previous ideas only to a limited extent and freely assume many novel concepts, basic linguistic theory takes as much as possible

from earlier traditions and only as much as necessary from new traditions. It can thus be roughly described as traditional grammar, minus its bad features (such as a tendency to describe all languages in terms of concepts motivated for European languages), plus necessary concepts absent from traditional grammar. It has supplemented traditional grammar with a variety of ideas from structuralism, generative grammar (especially pre-1975 generative grammar and relational grammar), and typology.

Basic linguistic theory differs from traditional grammar most strikingly in its attempt to describe each language in its own terms, rather than trying to force the language into a model based on European languages.

On the other hand, the fact that Basic Linguistic Theory has its roots in traditional grammar might also be reason for discarding it, if one believes, *contra* Dryer, that it also inherits the flawed theoretical approaches to language description and typology which have predominated ever since the time of Dionysius Thrax (170–90 BCE). Indeed, it is hard to see how Basic Linguistic Theory represents any significant gain over the Eurocentric approaches to language description when it still claims that the major parts of speech first espoused by the Greeks exist in every language. Gil (2001: 104), for example, illustrates a major problem with traditional approaches, and thus a problem for Basic Linguistic Theory:

Many would-be atheoretical [a-p-theoretical, in Dixon's case] fieldworkers – while deriding their theoretically-oriented colleagues for their excessive abstractness and lack of concern with linguistic diversity – fall into the same trap when characterizing the grammars of Southeast Asian languages in terms of categories whose justification stems, once again, from Standard Average European. Some of the many grammatical categories often imported uncritically from traditional grammatical theory are parts of speech such as noun, adjective, and verb; grammatical relations such as subject and direct object; and a host of more specific construction types, including relative clauses, conjunctions, reciprocals, and many more.

Gil goes on to provide potent arguments for why no such traditional categories should be distinguished for Riau Indonesian and Tagalog. Recognizing this perspective, Dryer (2006: 225) notes that one might accept the necessity of a common descriptive theory, but claim that Dixon's version of Basic Linguistic Theory is inadequate to the task.

These areas of disagreement raise an interesting question: How do we agree on what gets included in basic linguistic theory? Dryer (2006: 226) summarizes succinctly: “as with other theoretical approaches, basic linguistic theory is an overall theoretical framework encompassing different points of view, and criticisms of specific practices within basic linguistic theory can often be construed

as theory-internal disagreements as easily as criticisms of basic linguistic theory itself". Dryer himself lists several fundamental ways in which his own theoretical perspective differs from that of Dixon, while still agreeing with basic linguistic theory in principle. For Dryer, a competing descriptive theory must deviate "radically enough" from basic linguistic theory in order for it to be considered outside the cumulative framework. Of course, if we're being precise, we must realize that every individual researcher working within basic linguistic theory will adopt their own variations on the framework. Identifying what counts as basic linguistic theory is therefore analogous to the problem of identifying what counts as a dialect of a language. We generally accept a good deal of variation in what we call a single language, and so likewise we might accept a fair degree of variation within a single theory before we call it a competing theory.

Despite its relative nascence, Basic Linguistic Theory already faces an unfortunate and complicating terminological issue, which could muddle the discussion for many years to come. The problem is this: on the one hand is basic linguistic theory (lower-case), conceived of as an expression of all those theoretical assumptions shared among linguists today. On the other hand is Basic Linguistic Theory (capitals), which refers to the set of theoretical descriptive statements that Dixon believes all linguists should adhere to. Dixon of course sees these two conceptions as congruent. But others have already noted the ambiguity in their use, e.g., Chelliah & de Reuse (2010: 282):

"Basic Linguistic Theory", a term introduced in the short but influential Dixon (1997), is potentially somewhat misleading. Basic linguistic theory (henceforth BLT) seems to mean two separate things. On the one hand, BLT can refer to the approach to grammar based on semantic principles that Dixon has used in his grammar of English (Dixon 1991); see Chapters 2 and 3 of Dixon (2010a) in particular. This is a very commonsensical theory, but not everyone would consider it a complete theory of grammar. On the other hand, BLT can also refer to the common canon of terminology and basic linguistic knowledge which every modern linguist, regardless of theoretical orientation, will consider his or her own. Thus there is a lot of very basic information in this volume, and much of the book can be read by undergraduates. However, what beginning fieldworkers and grammar writers must be aware of is that the two conceptions of BLT are not always sufficiently distinguished.

Suppose that we accept, in defiance of Kuhn, the idea of a cumulative basic linguistic theory as the collection of theoretical statements to which all linguists agree. Then it seems obvious that the number of theoretical statements contained within basic linguistic theory is extremely small, perhaps even zero, representing the great diversity of opinion in linguistic theory. Dixon, by contrast, would claim that the number of statements included in basic linguistic

theory is rather large (enough to take up three volumes, in fact), and that anyone who does not agree with all or most of these statements is not practicing linguistics proper. Therefore it is clear that Basic Linguistic Theory (capitals – Dixon’s take on the correct theory of linguistics) cannot be the same thing as basic linguistic theory (lower-case – the set of statements to which all linguists agree). While Dixon would like to believe that his theoretical approach represents the accumulation of two thousand years’ research which all linguists agree upon, the fact is that many of Dixon’s theoretical positions are just as contentious as those of the generativist theories he derides.

4.3. *Methodology*

Dixon repeatedly asserts that a grammar is an “integrated system” and that “[e]ach part relates to its whole; its role can only be understood and appreciated in terms of the overall system to which it belongs” (Dixon 2010a: 24). As such, examining just a part of that system in isolation will be misleading (Dixon 2010a: 25):

There are some linguists who do just a little work on a language that is not well described, looking at perhaps a single construction type. This is bad science. One cannot appreciate the role of relative clauses in language X without relating and comparing them to other kinds of subordinate clauses, to the ways of marking syntactic functions, and so on.

This sentiment comports with our basic notions of structuralism and contrast in language, set forth by Saussure. This position is also the basis for Dixon’s critique, quoted earlier, of “armchair typology”, i.e., Greenbergian typological sampling methods. If we accept Dixon, Saussure, and Boas’ beliefs that grammar is an integrated, culturally-dependent whole, one could argue, as Dixon does, that this has an important consequence for typology: no linguist can or should attempt to undertake typological surveys unless they are intimately familiar with the languages under study. This is partly why Dixon insists that typologists read grammars from cover to cover and have a great deal of field-work experience before they can reasonably undertake typological surveying.

Cristofaro (2006: 162) points out the immediate practical considerations of this:

[I]deally a typologist would like to be able to find the information they need without having to read the whole grammar. This is in fact a practical necessity. Given the size of typological samples, that often include hundreds of languages, it is not always feasible for a typologist to read the whole grammar for each of the languages they take into account.

As one anonymous reviewer notes, this is partly an empirical question regarding the quality and quantity of academic output from Greenbergian versus

Dixonian approaches. Have the grammar-writers been more successful than the armchair typologists in making contributions to the field? It is not clear this is the case. Comrie, for example, was writing on ergativity around the same time as Dixon, and quite voluminously (Comrie 1973, 1978, 1979a, b). In fact most of the topics Dixon has written on have been treated by the Greenbergian typologists at one point or another. And large typological databases such as *WALS* and *AUTOTYP* continue to abet to our understanding of typology today. It is also worth asking whether typology would have risen to prominence as a legitimate mode of linguistic inquiry without the pioneering work of Greenberg in the late 1950s and 1960s. Finally, Dresher (1998) notes that Dixon's own career didn't follow these maxims: Dixon was writing on theoretical topics in language (Dixon 1963, 1965) and typology (Dixon 1968) some time before publishing a grammar (Dixon 1972). The empirical question is thus still an open one.

On the theoretical side, we should ask, "how is the move from language-particular description to language-general typological statements possible if one is limited to only the languages they have studied intimately?" This approach would seem to encounter significant sampling issues. Moreover, how is it possible for Dixon to make the generalizations he does in *Basic linguistic theory*, if he himself is only familiar with a fraction of the world's languages? Dixon even notes in the introduction that he focuses mainly on the languages he is familiar with. Are his typological generalizations valid? A second theoretical critique is that, if one accepts the existence of universally-available categories, as Dixon seems to, then why should we refrain from large-scale sampling, since we already know what to look for? As long as the grammar-writer, following Dixon's approach, applies the appropriate crosslinguistic label and notes how it deviates from the "prototype", such large-scale sampling seems unproblematic.

4.4. *Typological prediction*

While Dixon's belief in diachronic predictions has already been critiqued (Section 3.3.1), his views do raise an interesting question, namely, whether "typological prediction" counts as prediction proper in science. Dixon distinguishes two kinds of typological predictions (pp. 33, 171–174): implicational universals regarding the correlation of elements within a grammar, and statistical or absolute universals regarding the properties that all or some languages may have. Are these really predictions in the proper sense? Or should we restrict the meaning of "prediction" to refer only to future state of affairs, i.e., prediction through time? It is precisely this type of prediction regarding the future state of languages and grammars, however, that linguists are unable to make.

Typological universals, too, may be better understood as statements about language and the organization of grammar than as predictions. They have predictive power only to the extent that any descriptive statement has predictive power, i.e., it “predicts” that the statement is an accurate description of reality. Dixon adopts this position in *Linguistic science and logic*, denying that statistical sampling is properly predictive (Dixon 1963: 19, Footnote 14):

It will be noticed that when a theory has been constructed out of some sample which is statistically selected from a corpus we do not say that the theory will predict the patternings of the extra-sample part of the corpus. Since we have employed statistical techniques in assembling the sample and in checking whether the sample was large enough and representative enough for the theory to be a description valid for the complete corpus we could only use the term ‘predict’ in a quite trivial sense here.

This seems to contradict Dixon’s position in *I am a linguist* that statistical universals are predictive. It would be interesting to see typologists discuss the nature of prediction in typology further.

5. Evaluation

I will now make a few criticisms of a general nature. First, I found Dixon’s views extremely difficult to assess, because they would often seem, *prima facie*, to conflict with each other. In this review I have tried, whenever possible, to reconcile these views and interpret them consistently. But on the whole, I cannot escape the conclusion that Dixon is frequently inconsistent in his positions: he bemoans graduate students who aren’t dedicated to their work, but tells us how he shirked his own responsibilities in his PhD; he states explicitly that each language must be described in its own terms, but posits that there are universally-instantiated parts of speech; he claims to respect others’ religions while simultaneously denigrating them. It is thus extremely difficult to determine precisely what Dixon’s theoretical and ideological commitments are.

Second, while a certain simplification of complex research topics is to be expected in an autobiography, Dixon’s presentation of them is sometimes overly simplistic or misleading. For example, when talking about factors which lead to language change, he states that one factor is “the inherent laziness of speakers” (p. 27). It is value-laden terms like “laziness” that perpetuate some of the pernicious language myths which linguists are constantly trying to dispel. Nor does he engage with the current research in the areas he writes on, such as Naomi Baron’s research on technology and language, Croft’s well-known critiques of categorization, or literature in the philosophy of science. It does an injustice to John McWhorter, for example, that Dixon does not cite his well-known work when discussing the complexity of small languages and creole languages (e.g., McWhorter 2001, 2011). In fact, Dixon’s general stance against “quotationitis”

greatly reduces the overall integrity of the book, and perhaps his *Basic linguistic theory* volumes as well. Though this book might not be “academic” per se, a certain level of academic rigor and engagement with current ideas is still to be expected, and the book does not always meet that expectation.

Third, this book suffers unfortunate problems of classification and audience. I have hesitated to use the term “autobiography” in this review article for this reason. The book is surprisingly devoid of the personal details that one expects in an autobiography, and overabundant in ideological reflections on different topics. This is unlike Dixon’s earlier work, *Searching for aboriginal languages: Memoirs of a field worker* (Dixon 1984), which relates a detailed narrative of his fieldwork. Only approximately half the present volume consists of personal narrative, and Dixon says almost nothing about his first wife or children (perhaps four sentences in disparate locations). Given its content, a title like “Reflections on linguistics” may have been more appropriate and representative of the book’s content and genre.

As regards audience, Dixon here had an excellent opportunity to inspire potential linguists, and promote the discipline by capturing the attention of lay readers. The first six chapters accomplish this well enough, and Dixon’s style reflects that audience throughout. But from chapter six onwards, the book disintegrates into such a continual tirade of niggling complaints and academic in-fighting that any non-linguist will likely remain apathetic or even antipathetic to the majority of this book, in no small part due to its vitriolic nature. Linguists themselves will tolerate this because the ideas and issues are relevant to them. Indeed, the ideas in this book, if not the writing style, seem much more strongly targeted to existing linguists.

My fourth criticism, relating to the above, is that the book quite simply makes for unpleasant reading, whether you agree with Dixon’s views or not. For starters, the autobiographical sections sometimes read like an annotated bibliography, showcasing Dixon’s achievements rather than relating interesting facets of his personal life. This is perhaps understandable coming from an academic, who thinks of career accomplishments in terms of intellectual achievements, but the result is occasionally dull and often boastful. Far more distasteful, however, is that, within 400 pages, Dixon manages to insult or deride nearly every class of linguist, and other groups besides. Nor does he shy from airing personal grievances. It is unfortunate that a book which should have been a celebratory tribute to one of the field’s most renowned linguists should be marred by such catty invective.

Dixon’s derisive writing style is most prominent when he discusses religion (p. 293):

I’d [...] decided that the idea of there being a deity was both unlikely and unnecessary. [...] Almost all of those I associate with don’t need such solace. We use our minds for creative thinking rather than obeisance to an ineffable spirit.

Such offhand shots are frequent: “Like most missionaries, Alan doesn’t have a great sense of humor” (p. 312). It is difficult to reconcile these passing polemics with Dixon’s statement on p. 295:

It’s a fact of life that many people need religion. I don’t. But I do respect other people’s beliefs.

Dixon is aware of this potential inconsistency, and explains that he is simply writing “more-or-less straightforwardly, describing things as they are [...] which will no doubt irk many people” (p. 256). But Dixon often walks a fine line between being blunt and being simply insulting.

Dixon should be lauded, however, for his audacity in tackling the sociopolitical topics he does. These issues are important for linguistics and field linguistics, especially given the frequently political nature of fieldwork, language shift, or revitalization. So Dixon should be praised for his willingness to tackle these subjects head-on, and it is good that this “biography” has given him a space to do so.

For those wishing to understand the “Dixonian synthesis”, this is the volume to read. This fact also makes the book an excellent companion to *Basic linguistic theory*. These two books in conjunction present an overview of the ideas that have shaped much of Australian linguistics, thanks to Dixon’s enduring influence. I do not, however, recommend this book to laypersons or potential linguists, nor academics in other fields, for I do not believe it to be an accurate representation of the nature of linguistics. Most readers, after finishing this book, would likely write off linguistics entirely (and Dixon might agree that they are justified in doing so, given his assessment of the field). Professional linguists, however, may find Dixon’s views interesting or agreeable. Perhaps the book will spark productive discussions among linguists in reaction to some of the issues raised.

I conclude with one of Dixon’s own sentiments (p. 340): “Why beat about the bush? One ought to take care not to offend people needlessly, but bad work should (perhaps in the nicest way possible) be identified as bad work.” While Dixon should be praised for his many contributions to linguistics, *I am a linguist* fails to meet the high bar which he has established for himself.

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University of California, Santa Barbara
Rosetta Stone

Correspondence address: Rosetta Stone, 135 W. Market St., Harrisonburg, VA 22802, U.S.A.;
email: dhieber@uemail.ucsb.edu.

References

- Ameka, Felix K., Alan Dench & Nicholas Evans (eds.). 2006. *Catching language: The standing challenge of grammar writing*. Berlin: Mouton de Gruyter.
- Baker, Mark C. 2003. *Lexical categories: Nouns, verbs, and adjectives*. Cambridge: Cambridge University Press.
- Baron, Naomi S. 2003. *Alphabet to email: How written English evolved and where it's heading*. London: Routledge.
- Baron, Naomi S. 2008. *Always on: Language in an online and mobile world*. Oxford: Oxford University Press.
- Berge, Anna. 2010. Adequacy in documentation. In Lenore A. Grenoble & N. Louanna Furbee (eds.), *Language documentation*, 51–66. Amsterdam: Benjamins.
- Bowern, Claire. 2008. *Linguistic fieldwork: A practical guide*. New York: Palgrave MacMillan.
- Bowern, Claire. 2010. Fieldwork and the IRB: A snapshot. *Language* 86. 897–905.
- Bowern, Claire. 2012. *A grammar of Bardi*. Berlin: Mouton de Gruyter.
- Boynton, Jessica. 2011. The cost of language mobilisation: Wangkatha language and Native Title. Paper presented at the workshop "Sociolinguistics of Language Endangerment", LSA Summer Institute, University of Colorado at Boulder, 30–31 July.
- Buszard-Welcher, Laura. 2010. Necessary and sufficient data collection: Lessons from Potawatomi legacy documentation. In Lenore A. Grenoble & N. Louanna Furbee (eds.), *Language documentation*, 67–74. Amsterdam: Benjamins.
- Campbell, Lyle. 2004. *Historical linguistics: An introduction*. 2nd edn. Cambridge, MA: MIT Press.
- Chelliah, Shobhana L. & Willem J. de Reuse. 2010. *Handbook of descriptive linguistic fieldwork*. Dordrecht: Springer.
- Comrie, Bernard. 1973. The ergative: Variations on a theme. *Lingua* 32(3). 239–253.
- Comrie, Bernard. 1978. Ergativity. In Winfred P. Lehmann (ed.), *Syntactic typology: Studies in the phenomenology of language*, 329–394. Austin: University of Texas Press.
- Comrie, Bernard. 1979a. Degrees of ergativity: Some Chukchee evidence. In Frans Plank (ed.), *Ergativity: Towards a theory of grammatical relations*, 219–240. London: Academic Press.
- Comrie, Bernard. 1979b. Some remarks on ergativity in South Asian languages. *South Asian Language Analysis* 1. 211–219.
- Cristofaro, Sonia. 2006. The organization of reference grammars: A typologist user's point of view. In Ameka et al. (eds.) 2006, 137–170.
- Croft, William. 2001. *Radical Construction Grammar: Syntactic theory in typological perspective*. Oxford: Oxford University Press.
- Croft, William. 2003. *Typology and universals*. 2nd edn. Cambridge: Cambridge University Press.
- Crowley, Terry. 2007. *Field linguistics: A beginner's guide*. Oxford: Oxford University Press.
- Daniel, Michael. 2011. Linguistic typology and the study of language. In Jae Jung Song (ed.), *The Oxford handbook of linguistic typology*, 43–68. Oxford: Oxford University Press.
- Dixon, R. M. W. 1963. *Linguistic science and logic*. Den Haag: Mouton.
- Dixon, R. M. W. 1965. *What is language? A new approach to linguistic description*. London: Longmans.
- Dixon, R. M. W. 1968. Noun classes. *Lingua* 21. 104–125.
- Dixon, R. M. W. 1972. *The Dyirbal language of north Queensland*. Cambridge: Cambridge University Press.
- Dixon, R. M. W. 1977a. *A grammar of Yidj*. Cambridge: Cambridge University Press.
- Dixon, R. M. W. 1977b. Where have all the adjectives gone? *Studies in Language* 1. 19–80.
- Dixon, R. M. W. 1979. Ergativity. *Language* 55. 59–138.
- Dixon, R. M. W. 1980. *The languages of Australia*. Cambridge: Cambridge University Press.
- Dixon, R. M. W. 1984. *Searching for Aboriginal languages: Memoirs of a field worker*. St. Lucia: University of Queensland Press.
- Dixon, R. M. W. 1988. *A grammar of Boumaa Fijian*. Chicago: Chicago University Press.

- Dixon, R. M. W. 1991. *A new approach to English grammar, on semantic principles*. Oxford: Oxford University Press.
- Dixon, R. M. W. 1994. *Ergativity*. Cambridge: Cambridge University Press.
- Dixon, R. M. W. 1997. *The rise and fall of languages*. Cambridge: Cambridge University Press.
- Dixon, R. M. W. 2002. *Australian languages: Their nature and development*. Cambridge: Cambridge University Press.
- Dixon, R. M. W. 2004. *The Jarawara language of southern Amazonia*. Oxford: Oxford University Press.
- Dixon, R. M. W. 2010a. *Basic linguistic theory*, Vol. 1: *Methodology*. Oxford: Oxford University Press.
- Dixon, R. M. W. 2010b. *Basic linguistic theory*, Vol. 2: *Grammatical topics*. Oxford: Oxford University Press.
- Dixon, R. M. W. 2012. *Basic linguistic theory*, Vol. 3: *Further grammatical topics*. Oxford: Oxford University Press.
- Dresher, B. Elan. 1998. The formalist in the field. *Glott International* 3(9/10). 8. Reprinted in *Toronto Working Papers in Linguistics* 32 (2010).
- Dryer, Matthew S. 2006. Descriptive theories, explanatory theories, and Basic Linguistic Theory. In Ameka et al. (eds.) 2006, 207–234.
- Franchetto, Bruna. 2006. Ethnography in language documentation. In Jost Gippert, Nikolaus P. Himmelmann & Ulrike Mosel (eds.), *Essentials of language documentation*, 183–211. Berlin: Mouton de Gruyter.
- Gil, David. 2001. Escaping Eurocentrism: Fieldwork as a process of unlearning. In Paul Newman & Martha Ratliff (eds.), *Linguistic fieldwork*, 102–132. Cambridge: Cambridge University Press.
- Hale, Mark. 2007. *Historical linguistics: Theory and method*. Malden, MA: Blackwell.
- Harrison, K. David. 2005. Ethnographically informed language documentation. In Peter K. Austin (ed.), *Language documentation and description* 3, 22–41. London: School of Oriental and African Studies.
- Haspelmath, Martin. 2007. Pre-established categories don't exist: Consequences for language description and typology. *Linguistic Typology* 11. 119–132.
- Haspelmath, Martin. 2010. Comparative concepts and descriptive categories in crosslinguistic studies. *Language* 86. 663–687.
- Hume, David. 1748. *An enquiry concerning human understanding*. London: Millar.
- Kuhn, Thomas S. 1962. *The structure of scientific revolutions*. Chicago: University of Chicago Press.
- Laudan, Larry. 1977. *Progress and problems: Towards a scientific theory of growth*. Berkeley, CA: University of California Press.
- Lehmann, Christian. 1989. Language description and general comparative grammar. In Gottfried Graustein & Gerhard Leitner (eds.), *Reference grammars and modern linguistic theory*, 133–162. Tübingen: Niemeyer.
- McWhorter, John H. 2001. The world's simplest grammars are creole grammars. *Linguistic Typology* 5. 125–166.
- McWhorter, John H. 2011. *Linguistic simplicity and complexity: Why do languages undress?* Berlin: De Gruyter Mouton.
- Nagel, Ernest. 1961. *The structure of science: Problems in the logic of scientific explanation*. New York: Harcourt Brace.
- Popper, Karl. 1959. *The logic of scientific discovery*. London: Hutchinson.
- Popper, Karl. 1963. *Conjectures and refutations: The growth of scientific knowledge*. London: Routledge & Kegan Paul.
- Rothbard, Murray N. 1995. *An Austrian perspective on the history of economic thought*. Vol. 1: *Economic thought before Adam Smith*. Aldershot: Elgar. Reprinted, Auburn, AL: Ludwig von Mises Institute, 2006.

- Shopen, Timothy. 2007. Acknowledgements. In Timothy Shopen (ed.), *Language typology and syntactic description* (2nd edn.), Vol. 1: *Clause structure*, xiv–xv. Cambridge: Cambridge University Press.
- Trask, Robert Lawrence. 1996. *Historical linguistics*. London: Arnold.
- Tsunoda, Tasaku. 2005. *Language endangerment and language revitalization*. Berlin: Mouton de Gruyter.
- von Mises, Ludwig. 1949. *Human action: A treatise on economics*. New Haven, CT: Yale University Press. Scholar's edn., Auburn, AL: Ludwig von Mises Institute, 2010.