J. Linguistics **35** (1999), 579–595. Printed in the United Kingdom

REVIEW ARTICLE

Are there principles of grammatical change?

MARTIN HASPELMATH

Max-Planck-Institut für evolutionäre Anthropologie, Leipzig

(Received 18 January 1999; revised 25 June 1999)

David Lightfoot, *The development of language: acquisition, change, and evolution.* (Maryland Lectures in Language and Cognition **1.**) Malden, MA & Oxford: Blackwell, 1999. Pp. xii + 287.

1. Introduction

The central thesis of *The development of language* is that there are no principles of grammatical change, so that 'historicist' or deterministic approaches to diachronic change are misguided. Instead, Lightfoot argues that language change can only be understood by taking the perspective of the 'growth' (i.e. acquisition) of an individual's biological grammar, which may end up with a different parameter setting from the parent's generation when the trigger experience changes. Such events of grammatical change are abrupt and unpredictable, and Lightfoot suggests that they can be understood better from the point of view of catastrophe theory and chaos theory than under a deterministic theory of history as was common in the nineteenth century.

Most of this is familiar to diachronic theorists from Lightfoot's earlier work (especially Lightfoot 1979, 1991). This book is intended as a general introduction to Lightfoot's research program and its theoretical underpinnings, accessible not only to theoretical linguists, but also to 'beginning graduate students and to historians, biologists, and physicists who have thought a little about language' (xi). In view of the fairly technical material in several chapters, it may be overly optimistic to expect lay readers to profit much from the book. However, it will certainly appeal to a wide audience of linguists, not only graduate students, but also sociolinguists, psycholinguists and historical linguists who want to get an overview of Lightfoot's influential views on language change.

Since the negative part of Lightfoot's agenda, his denial of principles of grammatical change, is so prominent in the book (chapters 2 and 8 are entirely devoted to 'deterministic' approaches to language change), it is bound to provoke reactions by those linguists who have proposed such principles. I will argue in a later section (section 6) that on the normal reading

of the word *principle*, there can be no doubt that principles of grammatical change exist, though of course not as theoretical primitives that cause the changes. But first I will discuss Lightfoot's various theoretical points in some detail, focusing on areas where I see major problems. The book consists of eight chapters, besides the introduction (chapter 1) and the concluding chapter (chapter 10). Chapters 2 and 8 argue against the view that there are predictive laws of language change. Chapter 3 introduces the Chomskyan view of a grammar as a biological entity and discusses learnability issues of language acquisition from this perspective. Chapter 4 presents the core of Lightfoot's theory, which is elaborated and exemplified in the more descriptive chapters 5–7. Chapter 5 discusses changes resulting from the loss of case, chapter 6 focuses on word order change, and chapter 7 describes a number of smaller-scale changes in (groups of) particular lexical items. Almost all the examples are from the history of English, and many of the issues are familiar from the earlier literature. Finally, chapter 9 briefly discusses the biological evolution of language. In what follows I will focus on Lightfoot's theoretical proposals, i.e. chapters 2, 4, and 8–10.

2. Parameter resetting due to changing triggering experiences

One of Lightfoot's paradigm cases for his theory is the loss of V-to-I-movement in English in the eighteenth century (chapter 6.3). In the seventeenth century, we still find plenty of cases of subject-verb inversion (of the type (1a) below), postverbal negation (of the type (1b)), and postverbal adverbs (of the type (1c)). Such structures are commonly analyzed as involving V-to-I movement in Chomskyan syntactic theory ((1a) in addition shows movement to C).

(1) (a) Came she to London?
(b) She came not to London.
(c) The man visited frequently London.

After around 1700, textual attestations of these structures drop sharply and disappear rapidly. Lightfoot's explanation is that around that time children no longer acquired grammars with the V-to-I parameter setting because their triggering experience had changed, i.e. the primary linguistic data to which they were exposed was different from that of earlier generations. He gives three reasons for this change in triggering experience: (i) the rise of periphrastic *do* in questions and negative sentences (whereby (1a) and (1b) occurred less and less often, being replaced by *Did she come to London?* and *She did not come to London*); (ii) the recategorization of modal verbs as auxiliaries base-generated in I; and (iii) the loss of verb-second grammars with obligatory V-to-I-to-C movement. (Presumably the latter two changes were less important, because they were already completed by the sixteenth

century (cf. chapters 7.2 and 6.2), whereas the spread of periphrastic *do* extended from the fifteenth through the seventeenth centuries.)

Although children still heard sentences such as (1c), these were not sufficient to trigger V-to-I movement in Lightfoot's view. He adopts Elan Dresher's 'cue-based' theory of acquisition, according to which a child uses the primary linguistic data not to select a grammar which matches her target input as closely as possible, but to scan the partial representations for certain cues that set the parameters. If the strength of a cue (e.g. its frequency) falls below a certain threshold, it will be disregarded in the emerging grammar. In his discussion of the loss of verb-second grammars (chapter 6.2), Lightfoot makes this concrete: he suggests that about 30 percent of all sentences must show the crucial cue for the verb-second parameter, a nonsubject in preverbal position. In the thirteenth century, one English text shows only 17 percent nonsubjects in preverbal position, which apparently was not sufficient to trigger a verb-second grammar, so that this property was lost.

The strength of this theory is that it can explain why certain changes seem to occur very rapidly, or 'catastrophically': if a cue needs a certain threshold strength, then a gradual, purely statistical change in the triggering experience may result in a rapid, qualitative change in grammars once the threshold is crossed. Furthermore, it may allow us to explain not just general tendencies of change (cf. section 6 below), but particular changes occurring at a particular point in time. We would have to know much more about the precise conditions of a change than we usually know from the historical records, but at least in principle such particular explanations should be possible.

However, Lightfoot's theoretical framework also has many weaknesses, to which I now turn.

3. Biological and social grammars

At the end of chapter 3, which introduces the Chomskyan view of language acquisition as 'growth' of biological entities called 'grammars', Lightfoot concludes that language is an epiphenomenon, and that 'the notion of a language is not likely to have much importance if our biological perspective is taken' (74). Of course, in later chapters he extensively refers to 'languages' such as Middle English, Swedish and Dutch. One might dismiss this apparent inconsistency as simply due to terminological convenience, with no theoretical consequences. But Lightfoot has two real problems here.

The first may be more a problem of rhetoric than a substantive theoretical problem. The book is supposedly about language change (at least among other things), and in the last chapter Lightfoot even suggests that historians in other fields (such as human history or biological evolution) could take his approach to linguistic change as a model for their work (267). But given the purely biological perspective, there is no change at all: grammars grow in

individuals, and mature grammars do not change (or if they do, this is irrelevant for Lightfoot's theory (80)). Only languages, i.e. collective social entities, change. By contrast, Lightfoot's perspective only sees a succession of biological grammars which may differ somewhat from each other. The expression 'change in grammars' can be taken in the literal sense only if the social dimension is not excluded from consideration.

While this first problem may be confusing for some inexperienced readers, the second problem seems to betray a confusion in Lightfoot's own thinking. In chapter 4, he introduces the notion of a 'social grammar', allegedly used by other linguists that do not share his biological perspective. Social grammars are 'algorithms' or 'devices which generate a corpus of sentences belonging to some socially or politically defined entity' (79). But to my knowledge, nobody assumes such a corpus-based notion of grammar these days. Instead, what most linguists work with is a notion of a social grammar defined as a set of regularities that are shared by the individual grammars of a speech community, as perceived by speakers-hearers. Many linguists implicitly work with a notion of social grammar precisely in this sense. The crucial point is that the members of a speech community are able to make generalizations concerning the social value of the linguistic features used in their speech community. They are not just passively 'exposed' to a 'trigger experience', but they notice that there is a speech community which is uniform in important respects. Thus, a social grammar is more than a linguist's abstraction, it is also a speaker's abstraction. Moreover, this sense of SOCIAL GRAMMAR is equivalent to the most common reading of LANGUAGE.[1] When historical linguists talk about 'language change', they mean 'change in social grammars'. A social grammar is a more abstract notion than a biological grammar, but there is nothing conceptually problematic about it. It represents a body of social conventions of which speakers have tacit knowledge.[2]

Lightfoot seems unable or unwilling to conceive of social grammars in this way, but his most serious error is that he sees the biological and the social grammar as two mutually incompatible notions (cf. 104). In fact, they are both useful, indeed necessary, as most linguists have recognized since the 1870's (cf. section 7 below). Saussure conflated the two terminologically in his LANGUE, but he was well aware that it has both a social and a mental

[1] Linguists who call 'language' an epiphenomenon usually define the term extensionally, and so does Lightfoot ('the output of certain people's grammars' (74)). However, it seems to me that the word *language* is rarely used in this way, either by linguists or by lay people. Usually *language* means 'social grammar' in the above sense.

[2] Lightfoot also compares 'the (social) grammar of French' with a notion such as 'the French liver'. The crucial difference is that French people know nothing about each other's livers and do not (consciously or unconsciously) adjust their livers when they observe a consistent change in other people's livers. However, a notion such as 'the French haircut' would be interestingly similar to the notion 'the grammar of French'.

component.[3] I find it fairly evident that a social grammar cannot exist without a community of speakers with biological grammars, but also conversely, a biological grammar would not grow without a social context. Primary linguistic data become triggering experiences only when they have social significance: children do not acquire language by watching TV, and when they grow up in a bilingual environment, they do not grow mixed grammars, but assign the input data to different internalized grammars according to the social context with which the data are associated. Thus, grammars exist in two ways simultaneously: more concretely, as internalized grammars in individuals' brains, and more abstractly, as a set of social conventions of a speech community.

4. Nongrammatical changes

Throughout the book, Lightfoot emphasizes that parameter resetting occurs only as a result of prior changes in the speech the child is exposed to: 'If there were no changes in trigger experiences, there would be no change in grammars' (218). But he is very vague about what kinds of changes these are: they are 'piecemeal, gradual, chaotic changes which constantly affect the linguistic environment' (105), 'the ebb and flow of nongrammatical changes' (106), 'due to chance, contingent factors' (259), 'stylistic innovations' (80). Apparently they are always adult innovations (80). Only one subtype of these 'nongrammatical changes', 'changes in trigger experiences resulting from population movements [i.e. language contact]' (218) is reasonably well-understood.

So Lightfoot's theory is intended to account for only part of observed language changes – apparently, a small part. If Lightfoot did not make such sweeping claims elsewhere, that would be no problem – a good theory of SOME linguistic changes would be a nice thing to have. The really disturbing aspect is that Lightfoot claims that his theory is the ONLY sensible theory of change, and that the 'nongrammatical changes' cannot be explained: 'There is no theory of why trigger experiences should change' (207, similarly 265). But he does not show that this is so – he simply ignores the theories of language that provide explanations for adult innovations and changes based on them (e.g. Vennemann 1983, 1993, Lüdtke 1986, Heine et al. 1991, Bybee et al. 1994, Keller 1994, Wurzel 1994).

More seriously, it is not clear that the notion of nongrammatical change, distinct from grammatical change due to parameter resetting, makes sense. At the very least it requires a revision of the notion of grammars as characterizing people's linguistic knowledge. Lightfoot insists that 'the fact

[3] Cf. Saussure 1983[1915]: 32: '... les associations ratifiées par le consentement collectif, et dont l'ensemble constitue la langue, sont des réalités qui ont leur siège dans le cerveau.' ['The associations ratified by collective agreement, which together constitute the *langue*, are real entities which have their seat in the brain.']

that a person's speech changes does not mean that his or her grammar changes' (84). But what is meant here by 'a person's speech changes'? Of course, if I lost my job as a linguist and went on to work as a taxi driver, my speech would change in the sense that I would talk about different topics and use different lexical material. But such changes cannot alter the grammatical trigger experience for the next generation. Only structurally relevant changes in speech can affect the trigger experience for new grammars, e.g. the fact that Old French *chies* 'house' 'became less commonly used in subject and object position' (203), which contributed to its later recategorization as a preposition (Modern French *chez*, chapter 7.3). But how could an expression decrease or increase in frequency without any changes in people's knowledge of language? Sometimes a frequency increase occurs over many generations, as in the case of English periphrastic *do*, which 'began to occur with significant frequency at the beginning of the fifteenth century and steadily increased in frequency until it stabilized in its modern usage by the mid-seventeenth century' (162). Apparently the frequency of a construction can be acquired by successive generations without being registered by the speakers' grammars. This amounts to the implicit postulation of a mentally represented 'usage grammar', somehow distinct from the 'biological grammar'. Such a language-particular 'usage grammar' is the only way in which one can conceive of 'a change in the way in which grammars are used, but not in the grammar itself' (84). An instantiation of this idea is perhaps Andersen's (1973) concept of an 'adaptive rule', which Lightfoot does not mention but which Newmeyer (1998: 70) invokes in a similar context. But all examples of 'adaptive rules' are cases where speakers adopt features of a prestige variety into their speech. So even if it could be shown that the 'usage grammar' is different in nature from the biological grammar, this would still leave non-contact-induced change unaccounted for.

In the final chapter, Lightfoot acknowledges that he has the potential problem of how to distinguish grammatical changes from non-grammatical changes (265–266). His solution is that a synchronic theory of grammar can help us out, but he does not even recognize the problem of distinguishing synchronically between the 'usage grammar' that he implicitly postulates and the 'biological grammar'. Clearly, the more parsimonious view is that there is only one grammar, which is responsible for all aspects of linguistic knowledge. On this view, consistent changes in frequency always result from some subtle changes in meaning (especially semantic generalization) or grammatical behavior.

5. Gradualness and abruptness of change

A whole chapter (chapter 4) of Lightfoot's book is devoted to the issue of 'gradualism and catastrophes'. He notes that new features (or new grammars) often seem to spread through a linguistic community in 'S-

curves', i.e. the change begins slowly, then picks up momentum and proceeds more rapidly, tailing off slowly before reaching completion (chapter 4.6). Lightfoot proposes to account for such changes by assuming that if a single learner has set some parameter differently from older people, she will produce utterances with innovative structural characteristics, which in turn affect the linguistic environment. Her younger siblings, for instance, will hear these utterances and may be led to produce the same novel structures, so that a chain reaction is set in motion. This scenario seems to account well for the observed gradualness of many changes in the speech community, which nevertheless progress quite rapidly. It is also compatible with the abruptness of change at the individual level.[4]

However, it is not clear to me why Lightfoot thinks that S-curve spreads should be difficult for 'proponents of social grammars, who must postulate the S-curve as an unexplained primitive' (104). On the usual definition of a social grammar (or language) (cf. section 3 above), and on the assumption that most changes are introduced and propagated by adults (cf. section 4 above), one would expect precisely such a progression of spreads. Once a change introduced by an individual catches on, it will be used by more and more speakers and will become an increasingly salient part of the social grammar, so that speakers are increasingly likely to adopt the change. In fact, S-curve spreads require a social-grammar perspective: on the view that grammatical change occurs only when a new grammar develops in a child, a single child with an innovative parameter setting could not have any impact, unless the environmental cues are already near the threshold value for a certain cue. But this presupposes what Lightfoot calls 'non-grammatical changes', and these cannot be accounted for in his model (cf. section 4 above). By contrast, a single adult may well have an impact on the linguistic environment (and on the social grammar), particularly if s/he is a group leader and a role model for others.

Another reason why change may appear to be gradual at the level of the speech community is that there may be two different grammars simultaneously present in the community, even in an individual speaker (chapter 4.5). Lightfoot adopts the term 'diglossia' from Anthony Kroch's work for this situation, though 'bi(dia)lectalism' would have been a less confusing term. Of course, many speech communities are multi(dia)lectal (or at least

[4] I did not understand why Lightfoot needs to argue specifically for abruptness of change at the individual level (e.g. 88). To the extent that one wants to talk about 'change' in biological grammars, it is abrupt by definition, because there is nothing intermediate between two speakers' grammars. Structural abruptness is of course a different matter: a change from [p] to [f], or from a noun to a preposition (as in the case of French *chez*) may well be gradual if one's theory of phonology or syntax allows intermediate points between them (cf. Haspelmath 1998 for further discussion). Unfortunately, Lightfoot does not say precisely what he means by 'gradual' and 'abrupt' (Bennett 1981 distinguishes five different ways in which a change may be gradual or abrupt).

'multisociolectal'), and many or most individual speakers have a command of multiple dialects or sociolects, so this idea is not a priori implausible. But Lightfoot's 'diglossia' is not in any way socially relevant, and in fact its primary motivation lies in learnability considerations: since learners have no access to negative evidence, he claims that obligatory rules are unlearnable without an innate ban on optionality, and apparent optionality must be due to multiple grammars. Thus, optionality of *that* in *I hope (that) I'm right* implies that all English speakers are diglossic, 'have access to two grammars' (94) with obligatory rules. The consequence is apparently that speakers have as many grammars as there are competing constructions. Lightfoot's notion of diglossia is completely unconstrained, and he makes no real effort to close the Pandora's box that he has opened. The fundamental problem in his treatment of gradualness and abruptness again seems to be that he is trying to account for population-based phenomena while at the same time adopting an exclusively individualistic perspective on grammar.

6. Some principles of grammatical change

The main point of Lightfoot's book is that '[t]here are no principles of history' (265), and more specifically, no principles of grammatical change. A grammar B may differ from an earlier grammar A only if B results from a different trigger experience, but since the latter differences are unpredictable, there can be no theory of change.

However, there is no reason to be so pessimistic. Innovations in adult speech may well follow highly general patterns, because all speakers share certain basic assumptions, goals and constraints. For instance, all speakers want to get their messages across with a minimum expenditure of energy, and all speakers want to be at least understood by their interlocutors. Rudi Keller has shown how the cumulative effects of collective actions can give rise to results that nobody intended, in language as well as in other areas of human activity (Keller 1994). Of course, this presupposes a perspective on language change that includes a serious consideration of the social aspects of language. And adult innovations can be cumulative if the introduction of an innovation into a speaker's biological grammar depends primarily on the frequency of its use, i.e. if entrenchment due to frequent exposure (cf. Langacker 1987: 59) is the mechanism that modifies speakers' biological grammars. On this general view, it is not necessary to see language acquisition as the primary locus of systematic grammatical change (although acquisition may well play a significant role).

In (2)–(6) below I list some general principles of grammatical change that have been proposed. This is just a small, illustrative sample of the many diachronic universals that one finds in the literature. I assume that all of them have a good chance of being correct. Most of them involve a claim of

irreversibility, i.e. directionality of change. Some of them are more specific, some are more general (e.g. (2) is probably a special case of (5)).

(2) *Final Devoicing*
In phonological change, voiced obstruents may become voiceless in final position, but the reverse never happens.
Example: Old High German *rad* > Middle High German *rat* 'wheel'.

(3) *Lexical > Functional Category Change* (cf. Haspelmath 1998)
Lexical categories may turn into functional categories. The reverse does not occur, and lexical categories do not turn into other lexical categories (*V > N, *A > V, etc.).
Example: N > P in Old French *chies* 'house' > Modern French *chez* 'with, at'.

(4) *No Degrammaticalization* (Givón 1975, Lehmann 1995[1982])
Grammaticalization is irreversible; degrammaticalization does not occur.[5]
Example: Latin *cantare habeo* 'I have to sing' > Italian future *cantarò* 'I will sing'.

(5) *Survival of the Unmarked* (Wurzel 1994)
When a privative opposition is given up, it is the unmarked member that survives.
Example: Old English opposition *i/y* > Middle English *i* (the unmarked member).

(6) *Behavioral before Coding* (Cole et al. 1980)
When a non-subject argument turns into a subject, it first acquires behavioral subject properties before acquiring coding properties of subjects.
Example: Older English experiencers controlled reflexivization before losing dative case-marking.

In the face of such evidence for diachronic principles, how could anyone maintain that there are no principles or laws of grammatical change? Lightfoot apparently has a specific interpretation of the notions 'principle' or 'law': he seems to mean diachrony-specific CAUSAL PRINCIPLES, i.e. principles that are directly involved in the causation of language change. The above principles are not causal principles in this sense, of course, and few linguists have misunderstood them as such. They are generalizations in need of further explanation, in terms of the individual processes occurring in individual speech acts and individual speakers (as in Keller's (1994) 'invisible-hand

[5] Some counterexamples to this generalization have turned up in the literature, most recently highlighted in Newmeyer (1998: ch. 5). But these are few and do not invalidate the principle as an overwhelmingly confirmed generalization. (See Haspelmath (to appear a, to appear b) for some further discussion of these issues.)

explanations', where the explanation of macroeconomic behavior from microeconomic events is explicitly adopted as a model for linguistics).[6]

So there are no language-specific causal principles of diachronic change – in this I completely agree with Lightfoot. But elsewhere Lightfoot makes much stronger claims. He argues against any kind of 'generalizations which hold of history' (210), although the principles in (2)–(6) represent good cases of such generalizations. In general, he does not distinguish properly between causal principles and generalizations in need of further explanation, and he argues specifically against a number of proposed causal explanations of observed generalizations.

One common type of causal explanation involves the notion of optimization, either speaker optimization (i.e. signal simplification) or hearer optimization (i.e. signal preservation/highlighting). This has a long tradition in linguistics, as Lightfoot notes correctly. But he does not accept optimization explanations for change, apparently because he has not understood how they work.[7] He objects on two grounds. First, the notion that languages become simpler/more natural/easier to pronounce is circular (38): 'simpler' is defined as 'what languages change to'. Second, if there is a bias favoring and disfavoring certain structures, why should there ever be nonoptimal systems? (48, 219) Both objections can be countered easily. There are of course manifold ways of determining simplicity/ease of pronunciation/ease of processing independently of language change, e.g. psycholinguistic experiments measuring processing time, error rate, etc. The old idea that phonological change is often in the direction of ease of pronunciation has been overwhelmingly confirmed (e.g. Pagliuca & Mowrey 1987), and similar arguments can be made for morphosyntax. The second objection Lightfoot answers himself (39, 219): different optimality preferences can be in conflict with each other, so that a local optimization in one place (e.g. vowel loss, leading to shorter words) can have undesirable consequences elsewhere (e.g. more difficult syllable structures, or irregular morphology). This kind of thinking has recently become popular within generative grammar under the general heading of Optimality Theory, but Lightfoot has apparently not been affected by this movement.

But of course, optimization is only the motivation of a type of change, not its full causal explanation. For ease of pronunciation, such an explanation

[6] Of course, many historical linguists are not really interested in explanations at this higher level and are satisfied with principles at the level of observed universals of change. This is analogous to many generative grammarians who do not look for deeper explanations of the blocking principle or of subjacency, although they are aware that such explanations may exist.

[7] Interestingly, Lightfoot is consistent enough to reject optimization accounts also when couched in a Chomskyan framework. Thus, he explicitly rejects his earlier Transparency Principle (Lightfoot 1979), and he argues against Roberts' (1993) explanation of grammaticalization changes in terms of a Least Effort Strategy of the Language Acquisition Device.

would run as follows.[8] Speakers have a certain freedom of deviating from the social grammar in their speech. Every now and then a speaker makes his/her life easier by pronouncing a word more sloppily than is socially accepted (this occurs unconsciously, of course). When an influential speaker does that, s/he may find followers who do the same, both because they want to signal social closeness to the innovator and because the sloppy pronunciation helps them save energy. As more and more people adopt the innovation, it may 'take off' and spread through the whole population in the form of a typical S-curve, as was discussed earlier. That the change may not go in the opposite direction (toward more difficult pronunciation, for instance) follows automatically from this sketchy explanation.

No doubt there is still much work to be done before we can claim that we have causal explanations for all of the observed principles of change, not all of which involve optimization (pace Vennemann 1993). But there is a lot of work that we can build on: Vennemann's 1983, 1988 work on phonology, Lüdtke's 1986 work on change in phonology and morphosyntax, Hawkins'. 1990 work on word order change and performance constraints, Lehmann's 1985 work on grammaticalization (see now also Haspelmath to appear a), Geeraerts' (1997) book on diachronic semantics, Hall's (1992) book on morphological change, Croft's (1996, to appear) work on the role of selection in language change, among many others.

Lightfoot has no use for these explanations because they all involve adult innovations and diffusion to other adult grammars through language use. But in Lightfoot's world-view, adult grammars are not affected by the social conventions, or by language use – grammars 'grow' in early childhood and then apparently remain immutable. Thus, it is difficult for him to conceive of laws or generalizations of change that do not drive the change, but are just the observed cumulative outcome of numerous micro-events. There is undoubtedly work to be done in establishing and explaining observations like (2)–(6), whether we call them 'laws', 'principles', or 'universals', a fact which Lightfoot does not appear to recognize.

7. Who are the 'historicist' linguists?

In the larger picture that Lightfoot draws (especially in chapters 2, 8 and 10), the misguided search for principles of grammatical change is part of a bigger error, the search for principles of history, which was allegedly characteristic of nineteenth-century thinking in general, as exemplified most saliently by

[8] Note that all we can explain in a strong sense is the preference for a change to take place, i.e. the diachronic universal of directionality (of the kind seen in (2)–(6)). We cannot explain why a certain change should have taken place in a particular language at a given point in history, except in the weak sense in which a particular instance of a general tendency is explained by the general tendency (cf. Vennemann 1983 for discussion).

Darwin and Marx. 'At that time, thinkers in various domains had a highly deterministic view of history and sought principles that would yield long-term predictions' (46). But who are the 'historicist' linguists? In his overview of nineteenth-century work in linguistics in chapter 2, Lightfoot tries to convey the impression that such a view was widespread in linguistics of the time, and has been influential throughout the twentieth century (only to be challenged by his non-deterministic, contingent view of change).

Lightfoot does not claim to be a historian of linguistics, but his account is so twisted and error-ridden that many readers will be completely confused. Of course, Hegelian deterministic views of history (of the kind popularized by Marx) had their influence in linguistics as well, for instance in the early writings of August Schleicher. But it is simply wrong to characterize nineteenth-century linguistics in general in these terms. The authors of the first half of the century (such as Rask, Grimm, Bopp) were not much interested in universal laws of language change at all – they mainly focused on tracing the histories of particular languages, without caring about predictions. And in the 1870's, a decisive shift away from the earlier 'historicist' views occurred. The neogrammarians revolutionized linguistics by establishing the uniformitarianist principle (changes in the past are subject to the same constraints as observed today), by focusing on languages as spoken by real speakers (rather than as embodied in literary texts), by insisting that an analysis of social phenomena must begin in the individual, and by taking a strongly psychological view of language. Their uniformitarianism and methodological individualism was no doubt influenced by the work of Charles Lyell in geology and Charles Darwin in historical biology, whose contribution consisted precisely in abandoning the earlier 'historicist' thinking.

Lightfoot acknowledges some of the neogrammarian insights, but due to his failure to recognize the major conceptual shift of the 1870's, his account is confused. It is simply not true that 'linguists generally did not appeal to psychology to explain historical changes' (36). In their 'neogrammarian manifesto', Osthoff & Brugmann 1878 (cited after Arens 1969: 340) adopt an explicitly cognitive point of view:

> Mit der lautphysiologie allein ist es noch lange nicht getan, wenn man über die sprechtätigkeit des menschen und die formalen neuerungen, die der mensch beim sprechen vornimmt, ins klare kommen will... Es muss notwendigerweise noch hinzukommen eine wissenschaft, welche über die wirkungsweise der psychischen faktoren... umfassende beobachtungen anstellt.
> [Phonetics alone is not sufficient if one wants to understand the human speech activity and the formal innovations which people introduce in speaking... In addition, we need a science which makes wide-ranging observations on the effect of the psychological factors.]

Lightfoot goes on to claim that instead of appealing to psychology, linguists sought 'independent laws of history' (36), the sound laws, which an overall conception of 'directionality was intended to explain'. In the neogrammarians' view, sound laws were not independent laws of history, but simply generalizations over observed phonological changes which they claimed to be exceptionless (in modern parlance, one could rephrase this as the claim that phonology is autonomous from other levels of grammar). Sound laws were in need of explanation, not by Lightfoot's 'directionality', but by appealing to phonetics and psychology. Thus, the neogrammarians established a research agenda that has lost little of its relevance today.

Another erroneous statement, repeated from Lightfoot 1979, is that '[b]ecause the neo-grammarians were working with the products of language, rather than with the internal, underlying processes and abstract systems, there were principled reasons why they did not extend their ideas of phonetic change to syntax' (37). In reality, it was the neogrammarians who shifted attention away from texts to speakers,[9] and they did work on syntax in much the same way as they did on phonology (though less extensively). Finally, Lightfoot notes Hermann Paul's methodological individualism, but characterizes it as 'a minority position', which 'played no important role for a long period' (78). In fact, Paul expressed one of the main neogrammarian principles, which soon became the mainstream view in linguistics.[10] Lightfoot concludes that the nineteenth-century program 'was not viable' (39). In fact, the neogrammarians were so successful that modern textbooks of diachronic linguistics still repeat much from such textbooks of a hundred years ago. Moreover, they were so modern (or, conversely, we are so conservative) that Lightfoot has to distort their views in order to present his own approach as innovative.

In chapter 8 Lightfoot claims that the older deterministic approach to language change is by no means dead, and he criticizes some contemporary generative work for proposing explanations of change in terms of 'endogenous optimization'. However, there is no discussion of modern functionalist work on language change – no reference to the influential work of T. Givón, J. Bybee, J. Hawkins, C. Lehmann, B. Heine, R. Keller. They

[9] Cf. Osthoff & Brugmann 1878 (in Arens 1969: 339): 'Die ältere sprachforschung trat ... an ihr untersuchungsobjekt ... heran, ohne sich zuvor eine klare vorstellung davon gemacht zu haben, wie überhaupt menschliche sprache lebt und sich weiterbildet ... Man erforschte zwar eifrigst die sprachen, aber viel zu wenig den sprechenden menschen.'
[Older linguistics approached its research object without having developed a clear idea of how human language works and reproduces itself. Linguists diligently investigated languages, but paid much too little attention to people using language.]

[10] Cf. Osthoff & Brugmann 1878 (in Arens 1969: 344): '... dass die sprache kein ding ist, das außer und über dem menschen steht und ein leben für sich führt, sondern nur im individuum ihre wahre existenz hat.'
[... that language is not a thing which stands outside or above people and leads a life of its own, but has its true existence only in the individual.]

and many others have proposed principles of grammatical change, to be further explained by psychological and social factors.

8. Language change and biological change

In several places, Lightfoot invokes the analogy between language change and biological change that was drawn by August Schleicher in the 1860's and indeed by Darwin himself. I find this a very positive aspect of the book, because my impression is that linguists have not yet learned as much as they could from the striking parallels between the two domains. Lightfoot notes that '[t]he distinction between an individual's grammar and the group notion of a language is analogous to the biologist's distinction between individual organisms and species' (79) (cf. Croft 1996, to appear for a detailed discussion of this point). He also observes that there is a branch of biology, molecular biology, in which it is unimportant whether two organisms are members of the same species (74), and this would be analogous to the exclusive focus on the individual grammar in generative linguistics, where the notion of language plays no role. However, it is also necessary to ask whether biological evolution can be understood from a narrow molecular point of view which ignores species. It seems to me that the language-biology analogy works here as well: just as we need both the individual speaker's and the social point of view for understanding language change, we need both the level of the individual organism and the level of the species for understanding how the interplay of variation and selection produces biological change.

Lightfoot again invokes the analogy where he discusses gradual vs. abrupt change: 'Some biologists believe that species evolve gradually, others that they evolve abruptly' (82). However, he then argues that language change is gradual at the social level and abrupt at the individual level, and I have noted above (section 5) that this is so almost by definition. Similarly in biology: individual organisms cannot differ gradually from each other because there is nothing intermediate between them. So the real debate in biology is whether the pace of change is relatively constant, or whether periods of rapid change ('punctuations') and periods of relative stasis ('equilibrium') alternate. Lightfoot wants to make the point that the second model is valid for linguistics, too: change happens in fits and starts rather than gradually (though of course this is true only of his parameter-resetting changes, not necessarily of 'nongrammatical changes'). Unfortunately, he does not distinguish properly between gradualness/abruptness and constant pace/punctuated equilibrium, so this part of the language/biology analogy does not get put across well.

A further striking similarity between language change and biological change is that it is directed in a sense, and yet does not lead to languages or species that are 'fitter' or 'simpler' than others in an absolute sense. Lightfoot makes the obvious point that '[p]resent-day English is not fitter

than Old English in any general way' (227), but he draws from this the wrong conclusion that 'quests for a general direction of change have not been successful' (227). Since there are many conflicting optimality preferences, directed change cannot lead to an overall improved grammar design, just to local improvements (which are inevitably problematic from some other points of view). Biological change is very similar. As Darwin noted, it makes no sense to classify organisms into 'higher' and 'lower' creatures. Change is certainly directed in highly specific ways (spiders are now much better web-spinners than their ancestors were a billion years ago), but organisms or species do not get better or fitter in any absolute sense.

The main point of Lightfoot's discussion of the biological evolution of language (chapter 9) is that the innate language faculty (UG) 'may have evolved as an accidental side-effect of some other adaptive mutation' (249). His main argument is that UG may actually be maladaptive to some extent. For instance, the Empty Category Principle (ECP), or a version thereof (the requirement that traces must be governed overtly), is dysfunctional in that it prohibits subject extraction although speakers apparently NEED to express sentences that would involve subject extraction. This particular argument does not work, however, because nobody denies that useful things may have inconvenient side-effects in some contexts: The fact that we sometimes have to crouch to get through narrow openings does not mean that upright posture is maladaptive. Lightfoot admits that the ECP 'may well be functionally motivated, possibly by parsing considerations' (249). More interesting would have been the demonstration that a UG principle is completely or primarily dysfunctional. That UG arose accidentally remains a logical possibility, but it receives no support from dysfunctional side-effects of functionally motivated principles.

9. Conclusion

A lasting impression from reading Lightfoot's book is the striking contrast between its extreme narrowness (no phonology or morphology, almost no languages other than English, no interest in language use or social aspects of language) and the grand, sweeping claims he makes. For instance, he suggests that his approach represents a shift from the classical approach of Galileo and Newton with its orderly, law-governed behavior of the universe to a new, 'chaotic' vision of science, which focuses on 'disorderly, erratic behavior' (chapter 10.1).[11] I have been very critical of most aspects of his theory: his strange notions of 'language' and 'social grammar', his failure to say anything meaningful about 'nongrammatical changes' (i.e. apparently the great majority of changes), his unconstrained theoretical innovation of

[11] That population-based phenomena require a different approach has been clear to many scholars for about two centuries (e.g. Adam Smith, Thomas Malthus, Charles Darwin, cf. Keller 1994).

'diglossia', his complete misunderstanding of the neogrammarian revolution, and his irresponsible ignoring of much of contemporary work on language change.

But a more interesting question is why he should have made all these questionable moves. After all, his central thesis (that there are no principles of change) has a kernel of truth in it: language change is not teleological, it is not driven by some mystical force toward a predetermined goal. In the end, we can only understand language change by looking at what happens in the individual. This new perspective allowed Darwin to understand biological change, and it helped the neogrammarians to understand language change. But with classical structuralism, the neogrammarians' psychological and individualistic metatheory fell into oblivion, and when mental individualism was reintroduced by Chomsky, it was deprived of its social dimension. Lightfoot's theory of change can be understood as the attempt to come to grips with language change from a Chomskyan individualistic perspective while ignoring the social side of language. That attempt is bound to fail, because language change is to a large extent social in nature. So Lightfoot has to ignore the majority of changes as 'nongrammatical' and dismiss them as inexplicable (or in any event, 'beyond the purview of grammarians' (207)), even though many interesting universals of change have been formulated and have stood the test of time. Since he sees the Chomskyan revolution as the major event in the recent history of linguistics, he completely misunderstands the neogrammarian revolution of the 1870's and tries to lump the neogrammarians together with their distinctly pre-modern predecessors. With his narrow, non-social view of grammar, he has no explanation for many (tendencies of) changes that are at least in principle explainable. But it is an uncomfortable position to be forced to argue that the other linguists are 'too ambitious, too principled, and seek to explain too much' (225), so Lightfoot attributes this wrong desire for explanation to a mistaken world-view, in which history is explained by the laws that drive it. Maybe some historians and even linguists of the nineteenth century had this view, but as far as I can tell, for everybody in the twentieth century, laws of history (or in any event laws or principles of language change) have been generalizations in need of further explanation, not primitives created directly by God or by chance.

REFERENCES

Andersen, Henning (1973). Abductive and deductive change. *Language* **49**. 765–793.

Arens, Hans (1969). *Geschichte der Sprachwissenschaft*. Bd. 1. Frankfurt am Main: Athenäum Fischer.

Bennett, Paul A. (1981). Is syntactic change gradual? *Glossa* **15.1**. 115–135.

Bybee, Joan L., Perkins, Revere D. & Pagliuca, William (1994). *The evolution of grammar*. Chicago: The University of Chicago Press.

Cole, Peter, Harbert, Wayne, Hermon, Gabriella & Sridhar, S. N. (1980). The acquisition of subjecthood. *Language* **56**. 719–743.

Croft, William (1996). Linguistic selection: an utterance-based evolutionary theory of language change. *Nordic Journal of Linguistics* **19**. 99–139.
Croft, William (to appear) *Explaining language change: an evolutionary approach.*
Geeraerts, Dirk (1997). *Diachronic prototype semantics: a contribution to historical lexicology.* Oxford: Oxford University Press.
Givón, T. (1975). Serial verbs and syntactic change: Niger-Congo. In Li, Charles N. (ed.), *Word order and word order change.* Austin: University of Texas Press. 47–112.
Hall, Christopher (1992). *Morphology and mind.* London: Routledge.
Haspelmath, Martin (1998). Does grammaticalization need reanalysis? *Studies in Language* **22.2**. 49–85.
Haspelmath, Martin (to appear a). Why is grammaticalization irreversible? To appear in *Linguistics.*
Haspelmath, Martin (to appear b). Why can't we talk to each other? A review article of Newmeyer (1998). To appear in *Lingua.*
Hawkins, John A. (1990). Seeking motives for change in typological variation. In Croft, William et al. (eds.), *Studies in typology and diachrony.* Amsterdam: Benjamins. 95–128.
Heine, Bernd, Claudi, Ulrike & Hünnemeyer, Friederike (1991). *Grammaticalization: a conceptual framework.* Chicago: The University of Chicago Press.
Keller, Rudi (1994). *Language change: the invisible hand in language.* London: Routledge.
Langacker, Ronald (1987). *Foundations of Cognitive Grammar.* Vol. 1. Stanford: Stanford University Press.
Lehmann, Christian (1985). Grammaticalization: synchronic variation and dichronic change. *Lingua e Stile* **20.3**. 303–318.
Lehmann, Christian (1995[1982]). *Thoughts on grammaticalization.* Munich: Lincom Europa. (First published 1982, University of Cologne.)
Lightfoot, David (1979). *Principles of diachronic syntax.* Cambridge: Cambridge University Press.
Lightfoot, David (1991). *How to set parameters.* Cambridge, MA: MIT Press.
Lüdtke, Helmut (1986). Esquisse d'une théorie du changement langagier. *La linguistique* **22**. 3–46.
Newmeyer, Frederick J. (1998). *Language form and language function.* Cambridge, MA: MIT Press.
Osthoff, Hermann & Brugmann, Karl (1878). Vorwort. In *Morphologische Untersuchungen auf dem Gebiet der indogermanischen Sprachen.* Leipzig: Hirzel.
Pagliuca, William & Mowrey, Richard (1987). Articulatory evolution. In Giacalone Ramat, Anna et al. (eds.), *Papers from the VIIth International Conference on Historical Linguistics.* Amsterdam: Benjamins. 459–472.
Roberts, Ian (1993). A formal account of grammaticalization in the history of Romance futures. *Folia Linguistica Historica* **13**. 219–258.
Saussure, Ferdinand de (1983[1915]). *Cours de linguistique générale.* Paris: Payot.
Vennemann, Theo (1983). Causality in language change: theories of linguistic preferences as a basis for linguistic explanations. *Folia Linguistica Historica* **4**. 5–26.
Vennemann, Theo (1988). *Preference laws for syllable structure and the explanation of sound change.* Berlin: Mouton de Gruyter.
Vennemann, Theo (1993). Language change as language improvement. In Jones, Charles (ed.), *Historical linguistics: problems and perspectives.* London: Longman. 319–344.
Wurzel, Wolfgang Ullrich (1994). *Grammatisch initiierter Wandel* (Sprachdynamik: Auf dem Weg zu einer Typologie sprachlichen Wandels, 1.) Bochum: Brockmeyer.

Author's address: *Max-Planck-Institut für evolutionäre Anthropologie,*
Inselstr. 22,
D-04103 Leipzig,
Germany.
E-mail: haspelmath@eva.mpg.de