How new languages emerge (review)

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How new languages emerge. By DAVID LIGHTFOOT. Cambridge: Cambridge University Press, 2006. Pp. ix, 199. ISBN 0521676290. $29.99.

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1. ESSENTIAL CLAIMS. This eight-chapter book summarizes and updates much of Lightfoot's work over the past forty years. The basic premise is that 'There are no principles of history', which is 'a function of many interacting factors' (166). Consequently, 'To explain language change, one needs to understand grammatical theory, language acquisition, discourse analysis, and social variation in grammars' (viii).

Formerly, L (1999:225) claimed that explaining language change requires only '(a) an account of how trigger experiences have shifted and (b) a theory of language acquisition that matches PLD [primary linguistic data] with grammars in a deterministic way'.[1] The strongest statement in the current work is 'structural change is contingent, resulting from changes in the grammar or in the use of grammars of earlier generations that have the effect of changing the availability of grammatical cues' (164).

Noam Chomsky's distinction between E(xternal) languages (like 'English') and I(nternal) languages (individual grammars) is crucial. L maintains that the proper approach to language change is via I-language acquisition (14).

Following are L's core claims:

1. From the biological point of view, there is one human language, evidenced by contrast to the properties of the communication systems of other species (3).

2. The capacity for variation is biological, particular variations environmental. Variations are secondary to the universal properties of human language (4f.).

3. E-language is a nonsystematic composite of the I-languages/grammars in speakers' brains (12) that is in constant flux (13).

4. E-language constitutes the PLD in the child's environment (13).

5. The PLD is the triggering experience for the acquisition of an I-language out of elements provided by the internal biological program, also called universal grammar (UG), or human language (9–11, 13).

6. Changes in E-language cause changes in the PLD/triggering experience, which in turn cause changes in I-language, and vice versa (6f., 13).

7. Language change is a group (E-language) phenomenon that spreads gradually through the population, from one place and group to another (13). One change in I-language alters the triggering experience for a few other children, and so on (163).

8. I-language is acquired by CUES, which 'constitute the set of available grammatical structures'; some occur in all grammars, others (points of variation) only in certain I-languages (84).

9. 'The cues are not in the input directly, but they are derived from the input, in the mental representations yielded as children understand and ''parse'' the E-language to

[1] Many have been skeptical of a deterministic theory. See now Pires & Thomason 2008.

which they are exposed' (158). 'Representations are elaborated step-by-step in the course of acquisition, and the cues needed become increasingly abstract' (80).

10. Cues/structures of I-language, not sentences, are triggers, for example, the $_{VP}$[V DP] structure is a trigger to set the parameter for a VO (verb-object) language. UG specifies parameters and a cue for each (78).

11. Learners seek abstract structures from unambiguous input data. A structurally ambiguous sentence does not constitute a cue (78).[2]

12. A 'robust' (never defined) cue takes priority over occasional irrelevant or ungrammatical input utterances (82).[3]

13. Complex structures, like embedded sentences, are never cues (78). This is DEGREE-0 LEARNABILITY: children access only simple clauses and the topmost elements of an embedded clause (complementizers and infinitival subjects) (125).

14. Children identify perhaps a thousand cues and do not evaluate different systems against sets of E-language sentences (81).

15. Learned use (113f.), or PERFORMANCE CHANGES (e.g. Willis 1998:39), like more/fewer passives (132), also alter triggering experiences.[4] Such variations triggered the OV-to-VO shift and split genitives (114).

Most of L's claims about cues and especially acquisition are debatable (Arbib 2007).

2. SYNTACTIC CHANGE. In connection with the loss of verbal inflections (102–10), L concludes that there is a one-way implication: 'rich morphology entails V-to-I movement' (105). Therefore, loss of V-to-I typically lags behind loss of inflections.[5] Conversely, movement of DPs is not triggered by case morphology, but languages with rich case tend to have freer word orders (109), prompting (Paul Kiparsky's) generalization laconically summarized by L: 'positional syntax if little case morphology and non-positional syntax if rich case morphology' (110).

On pp. 90–102, L reiterates his account of the history of the English modals and concludes that the changes that brought about the elimination of V-to-I raising were (i) the recategorization of the premodals from the V-class to T (a change in I-language), and (ii) the increasing use of periphrastic *do* (a change in the use of grammars) (100).

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[2] The different possible representations of an ambiguity are pivotal in reanalysis (e.g. Roberts 2007:233).

[3] This does not exclude optionality. One kind represents the use of more than one grammar (111), each of which is 'discrete and either has a given property or not' (89). Options like *Peter said Kay left* and *Peter said that Kay left* are different because in both a complementizer is present, the former with a null exponent, which L implies children analyze as *that*-deletion (83).

[4] A change of this type is currently in progress. As adults use fewer exceptional case-marking (ECM) constructions in the spoken language, the cue is becoming less salient, and I have observed that younger speakers frequently either reject or have no judgments on ECM.

Salience is also affected by CLUES, or bits of E-language that facilitate the formulation of I-language cues (Miller 2004a). Changes in E-language can create cumulative clues that increase the salience of a cue. For instance, in Middle English of the thirteenth century, there were at least the following clues for infinitival complements with accusative subject to reflective and causative verbs (Miller 2002:Ch.7).

   (i) small clause + BE
  (ii) past passive participle + *to* BE (= passive infinitive)
 (iii) *to*-infinitive as causative complement

For L, these would all be the same cue (ECM), despite their differences. They came about at different times. With each clue, infinitives with PRO subject as complement to these verbs became less salient. For Lightfoot and Westergaard (2007), these clues would now be called MICROCUES.

[5] Since the English loss of V-to-I requires totally different motivation (main text below), the relevance of a lag is questionable.

These two changes altered the triggering experience: 'the expression of the $_I$V cue in English became less and less robust in the PLD' (99). Nothing in the triggering experience REQUIRED V-to-I: 'the expression of the cue dropped below its threshold, leading to the elimination of V-to-I movement' (100). See now Lightfoot & Westergaard 2007.

Languages that shifted to VO (123–36) had strong verb-second properties or relatively free word order in main clauses, in contrast to stable XP-V languages, like Japanese or Korean (125). For Old English, the main cue for matrix $_{VP}$[XP V] came from some V-final sentences and the raising of modal verbs to C, stranding the main verb in final position (131).

L claims (131ff.) that there was a steady decline of matrix XP-V through the *Anglo-Saxon chronicle*. Old English had the option of moving V to a higher functional position or leaving it sentence-final. This gradual change lasted several hundred years and eventually altered the robustness of the cues, leading to new grammars in Middle English. During the same time, there were almost no changes in XP-V embedded clauses, which follows from degree-0 learnability. When embedded clauses were finally affected, the change was rapid and catastrophic, reflecting the reanalysis to V-XP.

L argues that the XV/VX options were due to extraposition rather than coexisting grammars (134ff.), but does not mention Kiparsky's (1996) alternative of loss of the leftward movement operations, assuming Kayne's antisymmetry hypothesis (1994). It becomes clear in his discussion of creoles (139–52) that L rejects Kayne's linear correspondence axiom, according to which SVO is the universally unmarked order and all other orders derived by movement and adjunction operations.[6]

In general, L treats Old English as if in a vacuum. There is no mention of the fact that the change from OV to VO had been in progress for over a millennium in Germanic, as documented by Smith (1971), Eythórsson (1995), and Kiparsky (1996). For instance, in all of the early Germanic languages, including Old English, the verb was fronted to C in negatives, direct questions, and imperatives (Eythórsson 1995), for example, *Wæs þū, Hrōðgār, hāl!* 'Be thou, Hrothgar, well!' (*Beowulf* 405). Because children hear many negatives and imperatives, I suggest that the cue for $_C$V (i.e. V in C) should have been very salient and in partial conflict with the $_{VP}$[XP V] cue, since stable head-final languages never permit sentence-initial imperatives. Such conflicts kept the shift long in progress. For example, $_C$V was generalized from Neg to certain other operators in an expanded CP (ForceP), then to topics in general. This corresponds roughly to the microcues of Lightfoot & Westergaard 2007. That is, I assume changes in the verb-attracting features of lexical items in CP, then of CP itself. Each of these microchanges in turn removed a little more of the evidence for a V-final cue, the effect being a DRIFT of the type L rightly dismisses as causal (37).

In Germanic, some functional categories took their complements on the right (Smith 1971). Kiparsky (1996:152) sees the shift to VX as 'a simplification of the grammatical system: in the new grammar, **all complements of all heads, lexical and functional, are licensed on the right**' (boldfacing his). It should be acquisitionally simpler, I submit, for

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[6] L argues that Berbice, whose source is V-final Dutch and Eastern Ijo (Kwa), is SVO, not because SVO is unmarked but because in both of the input languages VO order is common in matrix clauses (145ff.). Therefore, the $_v$[V XP] cue is more robust than the $_v$[XP V] cue, especially since the negative marker came from Ijo, where it followed the verb. L concludes that 'Creole children, like all other children, scan their environment for cues. They interpret what they hear, heterogeneous or impoverished though it may be, as expressing cues and they converge on grammars accordingly. They are not subject to any bias built into UG of the type that Bickerton and Roberts have suggested' (148). Whether or not SVO is unmarked, L's account is reasonable.

a child to acquire a language that has all functional and lexical heads and complements linearized predictably. As cues to the contrary are whittled away over time, the few remaining counter-cues become less salient and prone to being ignored as irrelevant.

3. CONTACT PHENOMENA. L makes no mention of the role of contact with Scandinavian in the word-order shift, apart from an explicit denial (in connection with French!) (136). But Nordic played a role, as argued by Trips (2002:107, 113f.): only 24 percent of object pronouns in main clauses are postverbal in the southeastern Trinity Homilies (?a. 1200), but 68 percent are postverbal in the west midland *Ancrene Wisse* (?a. 1200) and Katherine Group (?ca. 1200), and 90 percent in the northern *Ormulum* (ca. 1180). The Scandinavianized north was more advanced in the shift to VO than the south, as expected (Thomason 2001:113, 172ff.). Trips concludes (2002:117) that VO was introduced into the Midlands by Scandinavian contact (cf. Allen 2000). This is too strong,[7] and also ignores the ongoing change since early Germanic, but the alacrity with which the change was completed argues for a contact catalyst. Differences between the languages were reduced by mutual accommodation (changes in use), which accelerated the change in progress.

The only contact account L unequivocally admits is the northern borrowing of V2 from Nordic (136–38). This is consistent with his statement that there are 'very few well-established cases [of contact-motivated changes] in the literature' (35).

It is strange that a book about the emergence of new languages did not pay more attention to the role of language contact, this constituting the epitomous alteration of triggering experiences. McWhorter (2002) argues that English lost more of its Germanic heritage than any other Germanic language, including Afrikaans, and attributes it to second language acquisition by Scandinavians. Moreover, as a reflex of convergence among languages in contact, Miller (2004b) argues that the major structural innovations in English were shared with East Norse (Old Jutland Danish and Middle Swedish). These include reflexive *-self* (in English and East Norse first in prepositional phrases),[8] omission of the complementizer *that* in relative and other complement clauses, the phrasal genitive with case marked only on one noun (not modifiers or appositionals),[9]

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[7] It is denied by Roberts (2007:397ff.) based on Old Icelandic, but the primary contact was with Danes, and the early Jutland Danish *Jyske Lov* 'Jutish Laws' were heavily mixed/VO. Moreover, early Icelandic law manuscripts (ca. 1150–1200) contain much VO and even V2 in subordinate clauses. The literary texts examined by Hróarsdóttir (2000) thus remained more conservative.

[8] This in no way invalidates the Celtic hypothesis (see Filppula et al. 2008:95ff., with references). Celtic may have been responsible for the initial loss of the Germanic reflexive and the idea for the innovated construction.

[9] L (114–23) discusses the change of two constructions to group genitives: (i) *Ælfredes godsune cyninges* to *King Alfred's godson*, (ii) *the clerkes tale of Oxenford* (a calque on the (Anglo-)French type *Williamesprest de Reigny* 'priest of William de Reigny') to *the clerk of Oxford's tale*. It is not clear that such group genitives 'became possible only when *'s* was construed as a clitic, which case marked DPs' (120), since in modern Swedish, which has a group genitive, *-s* continues to lack the free occurrence of English *'s* (Börjars 2003, Börjars et al. 2008). What is missing in L's account is the motivation for children to posit a caseless grammar rather than a grammar with one morphological case (genitive) on nouns and two nonsubject cases on pronouns.

With very few residues (Allen 2003), the *Ormulum* had a group genitive, for example: *Daviþess kinn* (l. 3570+) 'David's kin' but *Davið kingess kinn* (l. 309) 'David (the) king's kin'; *Herodess daʒʒ* (l. 7004+) 'Herod's day' but *Herode kingess daʒʒ* (l. 6992+) 'Herod the king's day'. Note especially *þurrh þe Laferrd Cristess dæþ* (l. 13,826) 'through the Lord Christ's death'. As long known, the *Ormulum* preserves (for metrical reasons) traces of inflection on modifiers (*forr þine nede* (l. 4401+) 'for thy need') except in the group genitive (*i þin herrtess hus* (l. 7374) 'in thy heart's house'), precisely as in Middle Swedish, for example, *vtan min fadhers wiliu* 'without my father's consent' (Norde 2001:259f.). I suggest that the group genitive originated by reduction of case/concord across NPs in the northeast, the motivation being the slightly different inflections in the contact languages.

P-stranding generalized to case-needing (WH) words (first in the northerner Richard Rolle, also in Danish and Swedish), and P-stranded passives.

In L's account it is entirely accidental that the languages in contact made the same innovations around the same time. But contact was the catalyst that altered the triggering experience that provided the cues for these shared innovations.

In my account of complementizer deletion, the similarity of OE *þæt/þat* and Nordic *aþ/at*, and the need to choose between them in northeast England, prompted avoidance by omission as a form of accommodation—a change in use that provided the cue for complementizer deletion. The shared innovations spread southward to mainstream English and, via the East Norse settlers, eastward to Jutland and Sweden.

The virtual crosslinguistic confinement of P-stranding (vs. topicalization) with case-needing relativizers to English and East Norse suggests an extreme change in the triggering experience. The fact that (i) Old English and Nordic required stranding with relative complementizers that differed formally and (ii) English had two *that*s, one a pronoun that admitted piedpiping, the other a complementizer that did not (van Gelderen 2004: 83), could have prompted generalized P-stranding to simplify acquisition of which lexical items required it and which disallowed it in both languages.

P-stranded passives (type *they were lied to*) are even more rare and effectively confined crosslinguistically to English and more limitedly Danish and Swedish, where P-stranding was never generalized to pronouns. As in Icelandic, stranding occurs but the moved pronoun is a topic, not a subject. This implies that the innovation followed the loss of case distinctions in the noun, which, as is well known, occurred earliest in the northeast (Allen 1995). Again, there is a reasonable case for a shared innovation.

In the context of 'why the linguistic environment should have changed in the first place' (165), L inquires, 'Why should the use of periphrastic *do* . . . have spread from the southwest of England as verbal inflections were being lost . . .?' (166). The question is left unanswered. Van der Auwera and Genee (2002) argue that there is no contradiction between the reanalysis and contact hypotheses. They agree with Miller & Leffel 1994 that causative *do* lasted longer in the east where French survived longest and the cue remained robust. But they also claim that it makes sense for the reanalysis to occur in the west under the influence of Welsh or the southwest in contact with Cornish (McWhorter 2009). Brythonic Celtic had always had a periphrastic (P) auxiliary, such as Welsh *gwneuthur* 'do', and in those areas (including the West Midlands, for example, Herefordshire) nonemphatic affirmative *do* survives to this day (Kerry Linfoot, p.c.; see also Filppula et al. 2008:57ff.). In sum, Brythonic speakers had a robust cue for the equivalent of *do*, while for English speakers in the same area the cue for causative *do* was not robust. The greater salience of the cue for P*do* (periphrastic *do*) prevailed among speakers who knew both languages. When P*do* spread eastward through monolingual anglophones, subsequent changes occurred.

4. SYNTACTIC ANALYSES IN THE NINETEENTH CENTURY. Ch. 2 contains mostly correct observations about nineteenth-century linguistics and comparative-historical methodology.[10] In his discussion of linguistic 'laws', L ignores the fact that the German tradition frequently used *Regel* 'rule' rather than *Gesetz* 'law', especially in connection with Grassmann and Verner. It is also incorrect to say about historical syntax that 'no compa-

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[10] A few minor details: Gothic 'daughter' is *dauhtar*, not *dauhtor (28), and Vedic 'father' is *pitā́* (nominative), *pítar* (vocative), not *pitār (28). Schleicher's family relationships date to 1850, the genealogical arrangement to 1858, and the family trees to 1860 (Koerner 1972:262), not 1861 (22). Despite L (172), Saussure did not discover laryngeals per se, and the *Mémoire* dates to 1878, not 1879 (Szemerényi 1973:251).

rable historical developments are postulated. Instead, their discussion of syntax is confined to catalogs of clause types in various languages with no diachronic links specified' (32).

Take Accusative and Infinitive (AI), for example. To motivate AI with noncontrol verbs, Brugmann (1897:§807) speaks of *eine Verschiebung der syntaktischen Gliederung* 'a shift of the syntactic boundary' (cf. Delbrück 1897:471, Kühner & Gerth 1904: Vol. 2.2, §475.3) with control verbs, that is, *I order them* [x *to do this*] → *I order* [*them to do this*].

Since Brugmann and others had no difficulty imagining a boundary shift for control verbs, why couldn't the reverse boundary shift have occurred with noncontrol verbs? That is, why not *I believe* [*you to be intelligent*] → *I believe you* [*to be intelligent*], analogous to subject-to-object raising? Since there was nothing in syntactic theory to prevent this form of reanalysis, the tradition must have had some reason to dismiss it. That was indeed the older account. Along with his eighteenth-century predecessors, Grimm (1822:Vol. 4, 129ff.) maintained that the accusative of AI was due to government by the matrix verb. Grimm's hypothesis was rejected because in Latin and Greek AI occurs in many constructions where case assignment by a matrix verb is impossible or irrelevant.[11] Consequently, Grimm's analysis was replaced with one that (a) better matched the synchronic state of affairs in Latin and Greek; (b) gave priority to AI with control verbs, which was historically accurate since many IE languages had AI with control verbs but lacked AI with noncontrol verbs; and (c) upheld the theoretical consideration that a clause boundary should be a barrier to government and case assignment. In that sense, the analysis was THEORY-DRIVEN. While the data forced the rejection of subject-to-object raising for the older IE languages, a notion of barriers forced an alternative analysis that provided special motivation for noncontrol AI. That is, Brugmann and his colleagues treated AI as EXCEPTIONAL case marking.

Much can be said about the interesting nineteenth-century syntactic analyses. It is erroneous to assert that they did not exist.

5. SYNTACTIC RECONSTRUCTION. L restricts syntactic reconstruction to structural identity (174). But even that must be further constrained, for example, by cognate lexical or grammatical roots (Miller 2002:164–67, Pires & Thomason 2008), because identity can be created by parallel innovation, borrowing, or areal diffusion. This is also an argument against parametric linguistics as a tool for reconstructing phylogenetic relations among languages as posited, for instance, by Guardino and Longobardi (2005). Unrelated languages may share or borrow the same parameter (Pires & Thomason 2008). Finally, without knowledge of the history of the parametric shift that changed Old English from a language like modern French to an exceptional case-marking (ECM) language (Miller 2002:Ch. 7), proper reconstruction could not be conducted by this method. Consequently, L is most likely correct that relationships among I-languages

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[11] Such well-known Latin constructions as (i) show that raising to an object position is irrelevant as an account of the accusative case of embedded subjects (Miller 2002:22ff.).

(i) meliust   nōs   adīre
    better.is us.ACC   to.go.INF
      'it is better that we (we'd better) go up'   (Plautus, *Menaechmi* 1091)

Nevertheless, Whitney (1877:§451) was right that subject-to-object raising works for English (Miller 2002: 146–49).

defined by shared cues will never 'match the historical relationships provided by historical linguists comparing words' (184).

6. CONCLUSION. L's discussion of how languages change is extremely interesting, even if most of the content has been published elsewhere. The book is aimed at faculty and graduate students who are not necessarily specialists in syntactic theory or even linguistics (viii), although I frankly doubt that nonlinguists could wade through Ch. 3.[12] The attempt to interweave more and less theoretically dense chapters sometimes interrupts the nexus of leading ideas. Curiously, L's theoretical discussions have more substance than the historical illustrations. If traditional accounts suffer from a plethora of data and little analysis, this book goes overboard in the opposite direction.

In the final tally, the I-language account of change seems to be on the right track, despite L's debatable assumptions about acquisition, and has attracted a following in the scholarly community. His ideas about change will continue to promote lively debate.

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[12] The extended discussion of deletion of the complement of an adjacent overt word is fairly lucid up to p. 51. Consider the example (i) from p. 52.

    (i) a. Kim is/'s happier than Tim is/*'s.
       b. Kim is happier$_i$ than Tim is ~~what$_i$~~.

Assuming that (ib) is the source of (ia), L says, 'if *is* cliticizes on to the subject noun and becomes part of that noun, it has been moved from one unit to another and no longer heads a phrase of which *what* . . . is the complement and no incorporation is possible' (52). He assumes that the deleted copy remnant is incorporated 'into the element of which it is the complement' (52). Without any discussion of incorporation or how it works, L states, 'Our UG principle (10) that deletion is incorporation solves the poverty-of-the-stimulus problem' (54). But his UG principle (10) reads: 'Something is deleted if it is (in) the complement of an adjacent, overt word'. There is no mention of incorporation—unless that is somehow to be gleaned from the *in* in parentheses? In any event, the unexplained use of incorporation complicates the issue in very unilluminating ways. Consider the following examples (52).

    (ii) a. I wonder where the concert is/*'s on Wednesday.
       b. I wonder where$_i$ the concert is ~~where$_i$~~ on Wednesday.

Given *Where's the concert?* and the echo question *The concert's* WHERE*?*, the generalization for (iib) is that cliticization would move *is* so that it is no longer adjacent to the copy remnant *where* to license its deletion. In other words, it is impossible to generate *I wonder where the concert's*; the only possibility with cliticization would be *I wonder where the concert's where*, which would crash for lack of remnant deletion. It is not clear what, if anything, incorporation has to do with any of this, given that cliticization and incorporation are not the same thing.

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