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DISCUSSION NOTE

Response to David Lightfoot’s Review of The Emergence and Development of SVO Patterning in Latin and French: Diachronic and Psycholinguistic Perspectives

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In the March 1996 issue of Language (72.1) David Lightfoot reviewed my book The Emergence and Development of SVO Patterning in Latin and French: Diachronic and Psycholinguistic Perspectives. Lightfoot criticizes ideas that are not in the book and also provides inaccurate analyses of its contents. In addition, he places the work in a biological frame it does not have, thus overlooking the principal contribution of the work. The book analyzes a major syntactic change and accounts for it by referring to psycholinguistic evidence in order to have objective criteria to evaluate the difficulty of the structures involved. I explicitly state that ‘before jumping to biological evolution, we must fully understand the linguistic principles underlying the phenomena we observe’ (Bauer 1995:216), which reflects a position that recurs constantly in the work. It is therefore amazing that one-third of the review discusses the alleged biological explanation that the book proposes, referring to ‘Lamarckism with a twist’, and to the alleged assumption that I argue that X becomes Y because ‘our brains work’ in a certain way (159). By contrast, the research for the book was carried out from a ‘purely linguistic perspective’ (Bauer 1995:218), thereupon relating diachronic linguistics and psycholinguistics without any biological or evolutionary perspectives.

1. Word order change. The Emergence and Development of SVO Patterning in Latin and French analyzes the ongoing word order change in Latin and French; it then discusses psycholinguistic evidence in order to account for the observed change. The book presents a detailed analysis of the change in ordering patterns in syntactic and morphological structures in Latin and French: in the history of syntactic structures one observes a shift from OV (or left-branching/head last) to VO (or right-branching/head first) structures. Similarly, in morphology, inflectional endings have been replaced by prepositions, preposed auxiliaries, personal pronouns, and so forth. Although this change can be traced in detail in the history of Latin and French, it was not limited to this language phylum alone. Not only did the change already start before Latin (in the Proto-Italic period and earlier), it is observed in other Indo-European languages as well. In the history of these languages, one observes that OV structures, which were inherited from the protolanguage, gave way to VO structures. As noted, the book focuses on the change in Italic, especially Latin and French and discusses noun phrases that include a genitival complement, an adjective, or a determiner (Ch. 3); analyzes verb phrases, focusing on the position of the verb in relation to its complement and to adverbs (Ch. 4); and analyzes prepositional phrases, comparative constructions, and relative clauses (Ch. 5). For each structure, the unmarked order in Latin is determined and its subsequent devel-
opment is traced, as well as the original inherited structure. In contrast to what Lightfoot claims (158), notions such as markedness or frequency are discussed and defined (e.g. Bauer 1995:6-7). Analysis of morphological structures is integrated in the discussion of syntactic phrases. The change is described in terms of branching; the discussion of this concept and the definition of the notion of head, which is fundamental in order to have a consistent analysis, is presented in Ch. 2. Subsequently, psycholinguistic evidence from a number of related and nonrelated languages is discussed (Ch. 6), providing objective data to account for the observed shift.

In order to analyze the comprehensive change of word order, it was necessary to include various types of data. Evidence for this analysis comes from my own corpus of data, from grammars and other general linguistic analyses, and from others' works on aspects of word order (e.g. Marouzeau 1922–53, Linde 1923; Muldowney 1937; Adams 1977). Many of these studies focus on a specific structure in the works of individual authors. These analyses present a number of strong advantages: (1) they are highly accurate and reflect a strong Fingerspitzengefühl: on the basis of their thorough knowledge of the author and his period these observers are 'able to evaluate every deviation from the norm or from the personal style of the author' (Bauer 1995:48); (2) these analyses are factual and as a rule the authors are not proponents of a specific theoretical approach, which makes their data more reliable; (3) the analyses present a huge amount of data which so far had not been systematically compared and analyzed. In addition, the relationships between the various linguistic phenomena had not been analyzed in these studies. Yet the data are corroborative and 'their analysis and comparison reveal the underlying regularity of the grammatical structures and their development' (Bauer 1995:48). As a result the book is partly based on a long tradition of word-order studies. Consequently, characterizing the data of the book as 'random anecdotal observations', 'analytical idiosyncrasies', or 'impressions about preferred orders in various texts' (158) not only shows a lack of familiarity with the literature on the topic, but also implies that the reviewer rejects a solid tradition of careful analysis including the work of scholars such as Marouzeau and others whose reputations equal their contributions to the field. Similarly my observation that 'the verb in initial position was meant, for example, to underscore its grammatical features: mood, tense, or voice' (Bauer 1995:93) is not the haphazard impression that is suggested by the reviewer (158) but is based on comparative analysis of texts by individual authors, the results of which are presented in the pages following the statement (93–97). The reviewer continuously makes this kind of inaccurate statement, suggesting that my observations are mere random 'impressions' rather than observations based on comparative and diachronic analysis that also include and discuss evidence which may point to reverse shifts.

According to Lightfoot, the 'analytical sloppiness' seems to be 'nowhere . . . clearer than in the discussion of early Latin. B. seeks to demonstrate that there can be no external causes for the comprehensive shift from LB to RB. That is, the change was not motivated by the languages with which Latin speakers had contact: Gaulish, Germanic, Oscan, Umbrian. Each language gets a para-
graph or so' (158). Here various diachronic stages and historical facts seem to have been mixed up. First, it is obvious that for the change under consideration contact between Latin and Gaulish and Germanic was irrelevant for Early Latin. Early Latin is dated no later than 100 B.C. (Bauer 1995:55, referring to Bennett 1910) and Gaulish and more so Germanic are relevant only to later instances of Latin. It is also obvious that reference is made to Oscan and Umbrian not because these languages were contact languages, but—as is explicitly stated (Bauer 1995:51)—primarily because they were sister languages of Latin. Since they present archaic characteristics, their analysis is most relevant for our understanding of Early Latin. Also, instead of giving each ‘a paragraph or so,’ evidence from both languages is included and amply discussed in the analysis of each structure as only a brief glance at the table of contents illustrates.

Similarly, the title of the book suggests that it cannot be that ‘B argues that Latin (like other early Indo-European languages) was a thorough-going left-branching (LB) language which changed’ (Lightfoot 1996:156). I said no such thing, rather, I stated clearly that analysis of the individual phrases (NPs, VPs, comparatives, PPs, relative clauses, and so on) in Latin shows that despite many LB characteristics Latin was not a rigid SOV type: the emergence of RB structures was well under way. Each of these phrases has been analyzed, their unmarked word order captured and the inherited structure retraced. On the basis of this analysis I concluded that ‘it is difficult to define [Latin’s] typology in one word . . . Latin evolved from GN to NG in the course of its history; ever since Old Latin the adjective followed the noun in unmarked order and occurrences of its marked preposing diminished in the course of time. Originally postposed, the possessive switched to preposing, whereas the demonstrative preceded the noun from earliest time. The Latin verbal phrase, on the other hand, was clearly LB, and its syntactic reorganization, which started in Latin, was only carried out in a later period. Its final achievement occurred in Modern French. The prepositional phrase, the particle comparative construction, and the RB relative clause are early developments that took place before Latin. On the other hand, nominal case endings, verbal suffixes . . . are all elements that mark the LB nature of Latin morphology’ (Bauer 1995:166). As this quotation demonstrates, I do not argue that Latin was a thorough-going LB language. Also, Lightfoot’s reproach that I neglect the chronology of the change is inaccurate. This aspect will be further discussed in the second part of this reply.

The review also wrongly gives the impression that fundamental distinctions have not been made or that obvious observations have been overlooked. Referring to the discussion about the adverb, Lightfoot argues that ‘Latin verbs follow adverbs and PPs even when the adverb or PP is sentential, i.e. not a member of the VP headed by the verb’ (Lightfoot 1996:157). In discussing the place of the adverb, I explicitly stipulated that only verbal adverbs are discussed in those pages. Adverbs that are part of a larger unit are much freer in use, for obvious reasons, determining the meaning of a sentence or a syntactic unit. The example given in the book dulce ridentem (Bauer 1995:122) is definitely an instance of a verbal adverb. The principal observation is that verbal adverbs
tend to precede the verb in Latin. Consequently, Lightfoot’s claim of omission does not hold.

‘Adjectives and adjective phrase (APs) often have different positions, as in English (the tall man / the man taller than anybody I have met) and French (le grand homme/l’homme plus grand que moi)’ (Lightfoot 1996:157). That adjectives may have a different position according to context is not new, but one has to account for the plain fact that preposing of grand as in grand homme is exceptional in Romance since adjectives normally follow the noun and only in specific contexts precede it. A(djective)-N(oun) and NA sequences therefore need to be included in the analysis as well. Consequently, the structures that ought to be compared primarily are not the ones mentioned by Lightfoot, but rather instances of the type Fr. un produit naturel vs. Engl. a natural product, or Fr. la grande maison vs. la maison blanche. Once these structures are fully understood and their development traced, it is possible to attempt to account for other constructions as well. It is also legitimate to ask why in French, adjectives that tend to precede the noun, such as grand, follow when the adjectival element is lengthy, while in a language like German the rules for AN sequences are much stricter. Finally, the adjective in Italic became a postnominal element at an early stage. In other Indo-European languages the development is much slower if it occurs at all; compare English, which despite many RB characteristics is very strict in keeping AN sequences. My book discusses these sequences, their uses, and their diachronic development at length and also discusses specific problems related to the position of the adjective (Bauer 1995:65–77).

2. LANGUAGE CHANGE AND PSYCHO-LINGUISTICS. Although the change from left- to right-branching is observed in all grammatical structures, the rate at which it occurred varies from structure to structure: the branching patterns in noun phrases changed in the Latin period or even before, whereas the verb phrase still included left-branching characteristics in subordinate clauses in French as late as the seventeenth century. Similarly, cross-linguistic analysis reveals that the rate and chronology of the development varies from language to language. Yet despite this variation, it was found that postposed relative clauses, hence complex structures, emerged early in all Indo-European languages: ‘the RB relative clause introduced by a relative pronoun was . . . an early creation, and moreover, in modern times it is the predominating strategy in Indo-European languages’ (Bauer 1995:169). The chronology that thus emerged from this research and that is discussed at length, is vital for the rest of the book. Consequently, Lightfoot’s statement that chronology is not taken into account is completely off the mark: ‘RB structures emerged at very different times and at different rates; B is aware of this, but it is difficult to extrapolate the dating details from her text’ (158). As noted, the chronology of the change is of vital importance. Yet the exact and absolute dating of the individual changes not only is ‘not indispensable’ for the aim of the analysis (Bauer 1995:12), but also futile considering the situation in Latin where many sociolects
coexisted, each undergoing similar changes at their own rates (see e.g. Mohrmann 1962, Bauer 1995:12).

Since the reorganization of grammatical structures is not restricted to the Italic phylum, is not circular, and presents its own chronology in the individual daughter languages, it cannot be ascribed to external factors, which—different in each language—cannot trigger the same change cross-linguistically. Because of the independence of the change, its comprehensiveness and the widespread early emergence of right-branching subordinate clauses, I argue that ad hoc explanations do not suffice and that it is necessary to find a more general way to account for the emergence and spreading of right-branching structures.

Since in child language as well, chronology in the emergence of linguistic features is a central issue, diachronic evidence is related to psycholinguistic data. Relating child language and language change is not new, as shown by Jakobson’s work (1980) as well as recent work by Slobin (1977, 1986), according to whom early acquired features are stable over time. While Slobin’s work is also based on the comparison of diachronic and psycholinguistic evidence, the perspective is different, focusing on the retention of features, whereas my book focuses on language change.

Acquisition patterns in Indo-European and non-Indo-European languages are compared, including complex and noncomplex syntactic structures, as well as inflection and agglutination (Ch. 6). Comparative cross-linguistic analysis of these data shows that both types of structure—left-branching and right-branching—present the same acquisition rates as long as they are noncomplex. By contrast ‘left-branching patterns are extremely difficult to master when the construction becomes longer and more complex’ (Bauer 1995:208). Consequently analysis of acquisition patterns provides independent criteria to evaluate the complexity of the structures involved in the change which may account for it. It is clear also that to account for is used in this context rather than explain, which in Chomskyan linguistics has become one of the most abused words of the English language, when linguists, while giving a formal representation of a synchronic phenomenon, assumed that they were providing an explanation.

Since the work accounts for diachronic change by referring to psycholinguistic data the author cannot be said to ‘limit [herself] to phenomena of language change’ (Lightfoot 1996:156). Yet I refer to psycholinguistic evidence without the biological or evolutionary implications that Lightfoot seems to assume when he states that ‘B adopts . . . [an] evolutionary concept of language change’, under which ‘language changes are viewed as analogous to evolutionary change at the phylogenetic level’ (158; reference is made to Bauer 1995:15). Lightfoot does not mention that in my discussion—on the same page—of the fundamental distinction between language change that is caused by external factors and inherent change (Meillet’s ‘lignes générales’ vs. ‘divergences’) I explicitly make this distinction ‘without drawing conclusions about evolutionary correlates in the Darwinian sense—for which I am not qualified’ (1995:15). Phylogenetic change does not necessarily imply an evolutionary perspective in the biological.
sense; it simply refers to diachronic change (change over time) as opposed to changes that take place in the acquisition of language (ontogenetic change). In addition, the terminology is not used in the book in relation to language change. Finally, relating evidence from language change and child language does not a priori imply an evolutionary perspective, as reference to Jakobson’s and Slobin’s work also shows. Since there is no biological perspective involved, I do not argue that ‘the switch from LB to RB represents evolutionary progression’, as Lightfoot seems to believe (158).

Despite my explicit statements that the analysis is purely linguistic and has no biological implications whatsoever, Lightfoot goes beyond Darwinism and jumps to neurology, arguing that ‘[Bauer] explains her [sic] diachronic change . . . in terms of human biology: our brains work in such a way that complex structures in LB languages . . . are hard to acquire’ (159). Needless to say by now, brains have not been mentioned in the book nor has their functioning been involved in my reasoning. I argue that ‘if it is possible to demonstrate that modern linguistic structures are mastered with less effort and at a quicker rate than their archaic variants, then we have an objective criterion of difficulty’ (Bauer 1995:171). The criterion of difficulty is then further expanded with arguments from language borrowing and language processing (Bauer 1995:209ff). Consequently, my approach does not have the biological implications that Lightfoot ascribes to me, nor does it offer ‘pseudo-explanations which lose touch with reality and which create mysteries where there is nothing mysterious’ (156).

In fact, Lightfoot’s consistent misinterpretations of the hypotheses and argumentation of the work, his inaccurate rendering of its details, his jumping to conclusions without any basis for them in the book, his unjustified claims of omission or sloppiness fail to meet the criteria required for objective scholarship and instead create ‘mysteries where there is nothing mysterious’.

REFERENCES


Lightfoot replies: Throughout the history of linguistics there has been a tension between those who have seen their linguistic analyses as autonomous and those who relate their work to the study of mental properties and aspire to capture psychological reality, one element of biology. The latter often invoke data from acquisition, processing, and aphasia to bolster their linguistic analyses, but the autonomists are not free to do this. Bauer’s book was firmly in the realist tradition. Her book, she says, ‘accounts for [a major syntactic change] by referring to psycholinguistic evidence’. The book, ipso facto, is in a biological frame, making claims about mental properties.

Using acquisition to explain diachronic phenomena is not unusual, but Bauer uses biology in an unusual way. She invokes the evolutionary concept, in which general, linear, irreversible, and unidirectional changes are due to a natural selection process: ‘languages evolve in the direction of features that are acquired early’ (170). She speaks repeatedly of the ‘advantages’ of a right-branching system. Again she is in a biological frame, and one which is not plausible, in my view.

Her book deals with ‘the shift from archaic structures, where the complement precedes the head—left-branching (LB) structures—to modern structures, where the complement follows the head—right-branching (RB) structures’ (11). These branching properties affect both syntax and morphology, she argues. ‘[T]he change in branching occurred in all phrases’ (48), and the shift was ‘gradual and consistent’ (65). ‘Once the general and irreversible nature of the change is demonstrated, ad hoc explanations are no longer adequate. In order to account for such a comprehensive change, a more general explanation, which may even affect the fundamental principles of language, is required’ (169).

Strong stuff. Too strong, in my opinion. Dr. Bauer may wish to back off from these claims, but it is disingenuous now to deny saying that there was a comprehensive change from an LB system to an RB system, and to deny that she was offering a biological explanation.

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