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*Letters to Language*

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## LETTERS TO LANGUAGE

*Language* accepts letters from readers that briefly and succinctly respond to or comment upon either material published previously in the journal or issues deemed of importance to the field. The editor reserves the right to edit letters as needed. Brief replies from relevant parties are included as warranted.

### Comments on Plag et al.

January 10, 2009

Dear Editor,

I would like to add a comment on the excellent article by Ingo Plag, Gero Kunter, Sabine Lappe, and Maria Braun in *Language* 84.4.760–94 ('The role of semantics, argument structure, and lexicalization in compound stress assignment in English', December 2008).

It was not noted in the article that compounds ultimately derived from French, such as *attorney general*, *inspector general*, *notary public*, are uniformly rightward stressed. Such compounds employ the characteristically French use of postnominal modifiers, which is generally not a feature of English. This generalization may have been missed by Plag et al., since their list of compounds cited includes only *attorney general* and *inspector general*.

This observation may also resolve the anomaly observed by Plag et al., as well as some other linguists, between the leftward-stressed *Fourteenth Street* and the rightward-stressed *Fifth Avenue*. *Street* in *Fourteenth Street* is derived from a Germanic form (though ultimately, of course, from the Latin *strata via*) and is therefore the unstressed partner in its leftward-stressed compound, whereas *avenue* is derived from French, and compounds in which it is involved would be rightward stressed. Note that similar compounds using words derived from French, such as *Grand Concourse*, *Lincoln Circle*, *Washington Mews* (from OFr *mue*), and even *Gasoline Alley* are all rightward stressed. Note also that such fused compounds as *Northgate*, which employs the Germanic-derived *-gate*, meaning 'street', are automatically leftward stressed.

This lingering preference for discrimination between Germanic and French forms is not an isolated instance of archaic constraints on stress assignment. It can also be found in the preference for Norman genitive forms employing *of* as in *the king of England* and *the Archbishop of Canterbury* in expressions of some dignity derived from the Norman conquest of England. Expressions referring to these digni-

taries employing Germanic genitive forms—as in *England's King* and *Canterbury's Archbishop*—would even now call attention to themselves. In fact, in the conclusion to Gerard Manley Hopkins's great poem 'The wreck of the *Deutschland*' the use of Germanic genitives in 'Our thoughts' chivalry's throng's Lord' makes a nationalistic point about the Englishness of the returning Messiah to England.  
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*Plag et al. reply:* Thank you very much for the opportunity to react to Edmund L. Epstein's interesting letter about our article. We very much appreciate Epstein's idea that etymology may also provide an explanation for the aberrant stress pattern of some of the compounds in our corpus. In fact, it had not escaped our attention that left-headed compounds such as *attorney general* or *inspector general* are probably constructed on the basis of corresponding French structures. We could even add other left-headed formations, such as *procurator fiscal*, *court martial*, or *president elect*, which, however, did not occur in our corpus. As the *Oxford English Dictionary* shows, most of these words are first attested in the sixteenth century and, interestingly, have competing right-headed forms at that time.

Nevertheless, we do not believe that the alleged French origin of compounds or compound constituents (as, for instance, *avenue* or *alley*) can explain the high number of right-stressed compounds in English, or the stress difference between compounds headed by *street* and those headed by *avenue*. Restricting ourselves, like Epstein, to names of thoroughfares, we must observe that even compounds with right constituents of Germanic origin, like *lane* or *road*, systematically have their stress on the right. Corpus studies have shown that about 30 percent of all noun-noun structures have rightward stress (see Kristina Kösling & Ingo Plag 2009 ('Does branching direction determine prominence assignment? An empirical investigation of triconstituent compounds in English', *Corpus Linguistics and Linguistic Theory* 5.2, to appear) for documentation and references), and it seems that their stress pattern cannot be explained with reference to etymology. Rather, there seem to be more general mechanisms at work that would ultimately also be responsible for the stress pattern observable in the names of thoroughfares that Epstein

cites. Our article in *Language* 84.4 explored some of these general mechanisms and our research team will continue to do so in future publications, which deal, for example, with the still underexplored role of constituent family information in compound stress assignment.

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### Response to Beckman's review

January 15, 2009

To the Editor:

In my *The phonology of tone and intonation* (Cambridge University Press, 2004), two revisions are made to Janet Pierrehumbert and Mary Beckman's (P&B) well-known description of Tokyo Japanese (*Japanese tone structure*, MIT Press, 1988) on the basis of new data.

The first is to take the pitch rise at the beginning of the accentual phrase to be a complex initial boundary tone LH, instead of an initial H and a final L, with the final L showing up as the first tone of the next accentual phrase. The advantage is that the last accentual phrase doesn't come with a final L. This simplifies the description of interrogative contours, which end in H%, not LH% (figure 10.5, panel b; now also rows 1–4, column 2 in figure 7.4 in Jennifer Venditti's chapter on 'The J-ToBI model of Japanese intonation' in *Prosodic typology: The phonology of intonation and phrasing*, ed. by Sun-Ah Jun, Oxford University Press, 2005, pp. 172–200). In unaccented accentual phrases, LH H% adequately describes the rise-plus-rise, and after accented accentual phrases, in which a fall occurs due to the pitch accent H\*L, the representation LH H\*L H% accurately describes the falling-rising pattern that we see. An additional L before H%, as described by P&B, is in the way in the unaccented case, and superfluous in the accented one.

The second revision consists in assuming that all tones associate one-to-one without spreading or contouring. The advantage is that we can correctly characterize the fate of unassociated tones: peripheral tones (L of the phrasal LH, declarative L%, and interrogative H%) are pronounced, despite the lack of a tone bearer, while internal ones (H of phrasal LH and L of H\*L) are deleted. B's comment in her

review of my book (*Language* 84.3:641–43) misses both points of this analysis when she notes that a third final-pitch shape can be observed and states that 'Pierrehumbert and Beckman's earlier error was corrected when their account was adapted for the X-JToBI annotation conventions which Venditti[, Kikuo Maekawa, and Beckman ('Prominence marking in the Japanese syntax intonation system', *The Oxford handbook of Japanese linguistics*, ed. by Shigeru Miyagawa and Mamoru Saito, Oxford University Press, 2008)] adopt in describing the use of other boundary shapes in the expression of different types of focus in spontaneous speech'. Venditti (2005:184) reports a third boundary-pitch contour, an 'explanatory rise-fall boundary movement', but offers no data that allow the prediction of my analysis to be tested; neither do I see any serious implication of the final-peak contour for the points made in my book.

A large part of the book is devoted to English. In particular, Chs. 14 and 15 contain analyses of the prosodic phrasing and the intonational melodies, summarizing my work over many years on the status of the phonological phrase as the domain of pitch-accent deletion (earlier known as stress shift), on the intonational phrase as the only tonally marked constituent of English, on the lack of evidence for an intermediate phrase, on the phonological status of pitch falls, on the optional nature of right-edge boundary tones, on the morphological status of downstep, on the right-alignment of trailing tones of prenuclear pitch accents, on the need to have OT-edge alignment as a phonological representation by the side of association, on the status and analysis of prenuclear fall-rises, and more, all of which positions are to varying degrees at variance with positions adhered to in the MAE-ToBI analysis. Not responding to any of these points, B notes that I misrepresent ToBI by suggesting it can be used for annotating larger speech corpora and that break index '4' is to be used after sentences, and suggests that these 'mischaracterizations' are due to the fact that I don't fully take account of my past thinking on some major issues in intonational phonology. I'm sure there are many things wrong with my past and present thinking, but not saying what my book is about is unlikely to reveal them. Worse, it suggests that the analytical issues I discuss are somehow not so important.

The review concludes by inviting me to 'develop a stance' on the ideas expressed in Pierrehumbert, Beckman, and D. Robert Ladd's (PBL) chapter on 'Conceptual founda-

tions of phonology as a laboratory science' in *Phonological knowledge: Its nature and status* (ed. by Noel Burton-Roberts, Philip Carr, and Gerard Docherty, Oxford University Press, 2000, pp. 273–303) on the undesirability of a two-module view of phonetics and phonology (never mind Ch. 4, on discreteness and gradience, or Ch. 5, on a theory of the meaning of gradient phenomena in intonation). I agree with PBL that man is a nondigital organism and that what we perceive as discreteness in phonology may ultimately be understood as states that arise from gradience, but I also believe that it will be a lot easier to reach that understanding if we have the right phonological analyses.

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*Beckman replies:* A phonological analysis is a model of how speech patterns are represented and processed in the minds of a community of speakers. A model simplifies and is, by definition, wrong. A useful initial simplification is to analyze variation in performances (readings) of controlled texts. There should be enough data to build a computationally explicit account of how the tone targets posited for each utterance type interact with other model parameters to generate such effects as the phrasal pitch span expansion characteristic of the incredulity question type analyzed in P&B. (The P&B analysis of this and other contours was adopted at the 1995 workshop that resulted in the J-ToBI conventions, as described in Venditti 2005.) Venditti and colleagues (2008) describe how the newer X-JToBI conventions differentiate the many more boundary-tone sequence

types attested in the hours of transcribed speech in the Corpus of Spontaneous Japanese (CSJ). My review noted that G's model cannot capture the contrast between the much more frequent focus-marking rise that is transcribed as L%H% in the CSJ, and P&B's question contour, which is reanalyzed as L%LH% in X-JToBI to reflect the different 'phonetic alignment' of the inflection point.

G's extensive work on English and other dialects of Germanic contributed greatly to the development of the AM framework. The arguments for his summary analyses in this book, however, focus on informal observations of patterns of 'OT-edge alignment' between morphosyntactic structures and tone sequences for read speech. Chs. 4–6 introduce the forms and functions of other model parameters, but the interaction of tone targets with these other model parameters is not treated in the later chapters in the formally explicit way that we saw, for example, in G's own earlier work on downstep in Dutch. My review noted this focus, pointing out that no studies of information structure in spontaneous speech are even cited. I also noted that G misrepresents the MAE-ToBI consensus by stating that the conventions were developed to annotate 'text corpora'. This misrepresentation highlighted to me an apparent divergence in our approaches to evaluation at later stages of model building. That divergence seemed far more fundamental than any of the specific differences between G's analysis of English and the other AM models that played a larger role in developing the consensus.

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#### CORRECTION

In the references of Ingo Plag, Gero Kunter, Sabine Lappe, and Maria Braun's article in the December issue of *Language* ('The role of semantics, argument structure, and lexicalization in compound stress assignment in English', *Language* 84.4.760–94, 2008), the author of the 1980 article 'Stress in English N+N phrases: A further complicating factor' (*English Studies* 61.264–70) was mistakenly given as Geoffrey Richard Sampson; the correct name of the author is Rodney Sampson, Professor of Romance Philology at Bristol University, and no relation to Geoffrey Sampson.