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.

Reviewing procedures

January 19, 2007
To the Editor:

Concerning Colin Phillips's suggestions for speeding up the review cycle at *Language*, I would like to point out that the Cognitive Science Society, which publishes the journal *Cognitive Science*, already employs a reviewer rating system. Associate editors (AEs) optionally assign two scores to each review, one for usefulness and one for timeliness. Members of the editorial board can also recommend that an individual be barred from reviewing. Reviewers and editors can see reviewers' average scores and scores for individual reviews. According to editor Art Markman (p.c.), the editorial board uses these features somewhat sporadically, however. Adopting a similar system, along with electronic submission and electronic nagging, might help bring the review cycles for *Language* more in line with those in related fields.

I believe that review cycles might be shortened even more if at least some of the correspondence about suggested revisions were to be handled by AEs, rather than being left entirely to the editor. AEs can report to the editor once they are ready to make a final recommendation for publication or rejection. That way, once all the reviews for a manuscript are in, an AE can respond directly to the author, rather than preparing a report for the editor, who then needs to reread the (new or revised) submission and the reviews before responding to the author. The editor could still handle each new submission, at least long enough to decide whether it should go out for review at all and if so, which AE should be in charge. And the editor could consider each submission once the AE had made a recommendation, and accept or reject papers, if necessary against an AE's recommendation, for example, based on comparisons with other submissions.

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Editor's reply: This proposal and the comparison with another field are welcome and interesting. To a large extent, these suggestions codify the system we actually use. On the one hand, they do envision greater rigor in the rating of reviewers (which we do on an informal basis in the editorial office) and in the use of electronic submission (which we require, though some of our work still is based on hard-copy submissions since some reviewers and other readers (like me!) still prefer hard copy) and electronic nagging (which we do as polite but insistent reminders, on a regular basis but perhaps not as often as could be done). On the other hand, they point to a greater degree of flexibility in the end stages of the review process than I personally am comfortable with; in order to achieve the evenness across all submissions alluded to above, I have felt that I need to take an active role in regard to the ultimate decision on each and every paper. I certainly welcome the discussion on process, and other editors may indeed see the editor's role differently. These suggestions can then be seen as changes in the review procedures that the next editor of *Language* might want to consider; for the remainder of my term, though, given that I have worked with a particular system for five years, I expect to continue in the manner I have.

Grammar and usage: The discussion continues

January 31, 2007
To the Editor:

In a recent edition of *Language* (82.4.705–6, 2006), Frederick Newmeyer (N) replies to my letter to *Language* (81.3.561–63; G 2005) addressing his earlier work on grammar and usage (*Language* 81.1.229–36, 2005 and 79.4.682–707, 2003). N's letter highlights the differences between our approaches, but does not always respond to the arguments I raised. No doubt N was constrained, as I am, by the letter format and the consequent limitations on space, but we must avoid the peril of talking past each other. Hence in this letter I attempt to remedy omissions in G 2005, and respond to all of the issues he raises.

First, to address my own lacunae: I acknowledge the citation in N 2005 of literature on differences between genres, and commend N for his recognition of the important contributions of variationist sociolinguistics; I was previously unaware of his article in *The Sciences*. Regarding my example of subject-verb agreement in Brazilian Portuguese, some published

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references, for the basic facts of agreement with preposed vs. postposed subjects, are the following: 'The social and structural dimensions of a syntactic change', by Anthony J. Naro (*Language* 57.1.63–98, 1981) and 'Um estudo da concordância verbal de terceira pessoa em Florianopolis', by Isabel Monguilhott and Izete Lehmkuhl Coelho (*Variação e mudança no português falado da região sul*, ed. by P. Vandresen, 189–216, Pelotas: EDUCAT, 2002); and for possible explanations see Coelho's 2000 dissertation *A ordem V DP em construções monoargumentais: Uma restrição sintático-semântica* (Federal University of Santa Catarina) and Ana M. S. Zilles's 'A posposição do sujeito ao verbo no português falado no Rio Grande do Sul' (*Letras de Hoje* 35.1.75–96, 2000). Finally, I lament 'distressing' N with my discussion of inherent variability, but a close reading of the passage he cites in this regard shows that I did not accuse him of ignorance of speakers' systematic sensitivity to frequency, but of assuming a form of the grammar that ignores this sensitivity; I took this to be an uncontroversial characterization of N's position.

As to the use of corpora, N and I agree that motley corpora including speakers of disparate 'dialects' are uninformative (except perhaps with respect to the most general features shared by all). However, my point was that well-constructed corpora reflecting socially realistic communities of speakers are indeed useful: the patterns of variation observed therein are not epiphenomenal compilations of disparate individuals, each of whom is internally invariant; rather, variable patterns found in the group data are also found in the speech of the individuals in the community. Hence a reliance on corpus data is not a principled argument against stochastic grammar in general; the method is not invalidated by the fact that it is sometimes poorly executed.

A principal argument for probabilistic grammar offered in G 2005 is the 'stochastic generalization'—the observation that the same explanatory principles are often required for both categorical and variable processes. I presented two examples, one from phonology, involving the obligatory contour principle, and one from syntax, relying on the concept of movement across AGR to accomplish agreement. Both illustrate the broader claim: there are principles of grammar that categorically prohibit certain outcomes in some languages or contexts, but in other cases inhibit these outcomes (i.e. significantly reduce their frequency of occurrence) without ruling them out entirely. N (2006) does not address the first example, and offers his own account of the second, touching on semantic, discourse, and functional issues, which I discuss below. But N does not directly address the point of the examples, which was to argue that the stochastic generalization is evidence for the underlying grammatical unity of variable and invariant phenomena, given that the same grammatical principles appear to underlie both.

Although N does not explicitly answer the various questions I posed about how he would treat stochastic generalization, I take his position to be summarized in his statement that the grammar defines options, and 'discourse-based and functional factors interact with the grammar to derive the statistical generalizations' (2006:706). I understand this to imply that, in N's view, cases of stochastic generalization are indeed accidental; categorical grammar-internal principles would only be evident in variation data as a product of this interaction, which could, in principle, obscure them entirely.

With respect to syntactic variation, N cautions us that different structures may be associated with different meanings, so that speakers may actually intend to say different things when they select one form or another. This is an issue that has long been discussed among variationists. The prevailing consensus is that, while certain structures may have different meanings in some of the contexts they occur in, there are often other contexts in which they function as alternants. Therefore, productive variationist analyses can be conducted, given careful attention to contexts and meaning. Diachronic change offers perhaps the clearest indication of broad meaning equivalence: if one syntactic structure displaces another across time (e.g. constructions with periphrastic *do* replace earlier forms of question formation and negation in English between 1300 and 1800), this suggests that they function as 'different ways of saying the same thing', to use Labov's shorthand definition of a linguistic variable. They may indeed have contexts in which, during the course of the change, they can be used to communicate different meanings or discourse functions, but to impute these meanings or functions to the structures as a whole would lead us to the apparently absurd conclusion that a diachronic shift entails a shift in the meanings uttered, and utterable, by the entire population. Thus, whatever meaning a speaker of Shakespeare's day could signal by saying *Knew you him?* as opposed to *Did you know him?* was something that English speakers collectively began to avoid, and can now no longer express

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at all. The case discussed in G 2005 and N 2006, the order of subject and verb in Portuguese, is involved in such a change: VS order, formerly very common, is now rare in Brazilian Portuguese (see Fernando Tarallo's 'Turning different at the turn of the century: 19th century Brazilian Portuguese', *Towards a social science of language*, ed. by G. R. Guy et al., 199–220, Amsterdam: John Benjamins, 1995).

On the matter of 'grammatical' differences between genres, the question that I posed in G 2005 was genuine: 'how does [N] propose to generate them if they cannot be subsumed within a single grammar?' (p. 562). If speakers generate utterances in one genre that would count as ungrammatical in another of their genres, how do we represent this? I don't have a position on this, but N construes the question as rhetorical, and incorrectly imputes to me the belief that 'if every grammar-related fact is not handled within a single system, it remains unaccounted for' (2006:706). I think, rather, that this is a substantive and underexamined question. Most linguists would no doubt accept that multilingual speakers possess multiple mental grammars, but are distinctive genres grammatically analogous to different languages and generated by different mental grammars, or are they generated from one grammar by the same means we devise to handle other kinds of variation?

N highlights one clear difference between our positions as follows: 'from the fact that Y is sensitive to X, it does not follow that X is part of the same system that characterizes Y' (2006:705). This statement is strictly speaking true, but instantiations of it are rare in ordinary experience; in practice, both in linguistics and elsewhere, we normally take mutual sensitivity as prima facie evidence of systemic connectedness. Thus phonological processes of assimilation, deletion, epenthesis, and so forth are sensitive to syllabic position and metrical structure, and we ordinarily assume that this is because all of these are part of the phonology. By contrast, they are insensitive to syntactic properties of the utterances in which they occur such as argument structure or recursion, and we commonly take this as evidence that syntax and phonology are systemically autonomous at some important level. They are also insensitive to whether a speaker is artistically talented, introverted, or good at *Trivial Pursuit*, which we take as evidence of the cognitive independence of language from other mental systems. So the simplest initial assumption about the grammatical sensitivity of linguistic variation is that they do indeed arise from the same cognitive system. The alternative position starts out with Occam's razor against it. I was therefore surprised to find N characterizing my position as 'radical', one 'in which all of the measurable facts are crammed into one cognitive system' (2006:706). I thought I was adopting a conservative, default hypothesis. In any case, it should be clear that Y's sensitivity to X most certainly does NOT imply N's position, which is that X and Y (in this case, grammar and usage) are not part of the same system.

In sum, the basic disagreement is a question of assumptions about the form of grammar. I see a world in which orderly heterogeneity in language reflects a continuum of linguistic generalizations ranging seamlessly from categorical to probabilistic, and postulate that it is governed by a coherent cognitive system capable of handling probabilistic as well as categorical operations, governed by a single set of principles. I advocate formal models of grammar that make explicit, testable, quantified predictions about use. N evidently sees the same world, but postulates a fundamental division of cognitive systems into a categorical grammar and an ill-defined 'user's manual', from which the quantitative generalizations emerge epiphenomenally, without any necessary connection to the principles contained in the grammar. He 'admits' (ibid.) that his treatment of orderly heterogeneity is 'sketchy and programmatic', which makes empirical comparisons of these contrasting visions difficult. Absent such comparisons, N concludes that variationist work does not 'entail as a necessary consequence' (ibid.) the position I advocate. I hope he would agree that neither does any of this discussion necessarily entail his position. We must turn therefore to other standards, such as a preponderance of evidence. Readers can judge for themselves where this lies.

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Editor's reply: I suspect more remains to be said on this important and interesting matter; perhaps others want to join the debate? Tune in next issue . . .

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