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.

Grammar and usage: A response to Gregory R. Guy

August 14, 2006

To the Editor:

In two recent contributions to *Language*—Grammar is grammar and usage is usage', 79.4.682–707, 2003 (GGUU) and 'A reply to the critiques of ''Grammar is grammar and usage is usage'' ', 81.1.229–36, 2005 (RCG)—I critiqued stochastic approaches to grammar, namely those in which grammatical constructs are linked directly to probabilities of occurrence in discourse. My arguments included the point that probabilistic generalizations derived from a sociologically and dialectally heterogeneous group of speakers are irrelevant to determining the grammatical competence of any particular member of that group; that as far as syntactic variation is concerned, since variants typically differ in meaning, the probabilities are likely to be more a function of the meaning to be conveyed than a characteristic inherent to the structure itself; and that the multiplicity of genres confounds the simplistic treatment that one finds in much of the literature defending stochastic approaches to grammar.

Gregory R. Guy (G), in a letter to *Language* (81.3.561–63, 2005), has taken issue with my position. Distressingly, from my point of view, he sees my remarks as exhibiting ignorance of the fact that 'speakers manifestly are sensitive to frequencies and manipulate them systematically' (G, 561). But in fact, I recognized explicitly that 'language users and hence their grammars are sensitive to frequency' (GGUU, 697). I did stress, however, that 'from the fact that Y is sensitive to X, it does not follow that X is part of the same system that characterizes Y'. Far from rejecting the essentials of the variationist project, I described it in a supplement to the journal *The Sciences* ('Benchmarks: 35 years of linguistics', 36.6.13, 1996) as a major breakthrough of modern linguistics, where I commented that '[t]he work of William Labov of the University of Pennsylvania and his colleagues has demonstrated that much of the everyday variability in speech is systematic, showing both social and linguistic regularities'.

I don't deny that 'variability in language is highly structured' (G, 561), nor that 'individuals are grammatically similar as a function of social proximity' (G, 562). But I do deny that the grammar of any particular individual can be derived from probabilities gleaned from corpora full of utterances where social proximity among speakers is absent. Surprisingly to me, given his other remarks, G agrees, writing that 'a corpus drawn from the *New York Times* does not offer a very close approximation to the linguistic input and personal grammars of Carl Pollard or Ivan Sag' (G, 562). But that's just my point. The corpora used in most stochastic approaches to grammar (though perhaps not in the more careful sociolinguistically informed ones) conflate utterances from speakers from a wide variety of regions and social classes.

G provides one example to rebut my 'hoary argument that perhaps syntax is different from phonology' (G, 562), namely that in Brazilian Portuguese (BP) subject-verb agreement is disfavored (though not categorically banned) with postposed subjects. While G does not provide a literature reference to track down, my immediate reaction was that he put forward a particularly problematic case in support of his position. In the first place, G ignored my point about syntactic variation and meaning. Since in BP, as in all Romance languages, preverbal and postverbal subjects differ dramatically in their discourse properties (Anthony J. Naro and Sebastião J. Votre, 'Discourse motivations for linguistic regularities: Verb/subject order in spoken Brazilian Portuguese', *Probus* 11.1.75–100, 1999), a complicating factor that G's approach fails to deal with enters the picture. Second, if the analysis referred to by G is correct, in which postverbal subjects originate in object position, then we have an independent explanation for the agreement facts. Verb-object agreement is crosslinguistically significantly less common than subject-verb agreement (Anna Siewierska and Dik Bakker, 'The distribution of subject and object agreement and word order type', *Studies in Language* 20.115–16, 1996), a fact that is rooted ultimately in the greater topicality of subjects vis-à-vis objects (*Agreement*, by Greville G. Corbett, Cambridge: Cambridge University Press, 2006). In the approach that I advocate, all of the relevant generalizations can be ac-

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commodated. The grammar of BP allows agreement with arguments in both positions, while discourse-based and functional factors interact with the grammar to derive the statistical generalizations. G's radically autonomist approach, in which all of the measurable facts are crammed into one cognitive system, succeeds in describing the empirically observable regularities, but not in explaining them.

Finally, let's turn to the question of genre. G writes that 'N's argument from the multiplicity of genres also demonstrates unfamiliarity with variationist research. For the most part, stylistic variation is quantitatively simple, involving raising or lowering the selection frequency of socially sensitive variables without altering other constraints on variant selection' (562). Far from being unfamiliar with this research, in RCG, p. 231, I called attention to it and provided a literature citation. My point in both GGUU and RCG was that once we take on more complex genre phenomena, such a simple formal approach ceases to work. In this regard, G asks: 'As for the bigger ''genre'' differences that N cites, how does he propose to generate them if they cannot be subsumed within a single grammar?' (562). Again, G's question betrays his seeming belief that if every grammar-related fact is not handled within a single system, it remains unaccounted for. In GGUU, p. 698, I provided an (admittedly sketchy and programmatic) answer to G's question, suggesting that 'the more systematic aspects of one's extragrammatical knowledge are organized in the sort of ''user's manual'' discussed by Zwicky (1999)'.

I trust that this letter contributes to the realization that variationist work on the grammar/usage interface does not entail as a necessary consequence that properties of grammars and statistical generalizations about the use of constructs provided by grammars are part and parcel of the same cognitive system.

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Editor's reply: This topic is clearly one with 'legs'; I welcome further contributions to the ongoing discussion.

Re: *Language* in the 21st century

September 18, 2006

To the Editor:

This is a response to Stephen Levinson's letter about *Language*'s editorial practices, and your remarks in a related editorial in the March 2006 issue. Levinson's main concerns are that *Language* publishes too few articles, and too slowly. He contrasts current practices in linguistics with those in neighboring fields such as psychology and anthropology.

The editorial highlights a number of steps that are being taken to address Levinson's concerns, but also links issues about publication practices to the question of whether linguistics is '(still) a traditional humanities discipline'. I think that these issues should not be linked. No matter where linguistics might belong on the academic map, it does not enjoy the institutional security of most traditional humanities disciplines, and so cannot afford to use them as a model of scholarly practices. At least in the US context, few universities would seriously consider closing their departments of English, history, or philosophy, however vibrant or otherwise these fields might be. These are among the 'Disciplines that a good university must have'. Meanwhile, most universities could drop linguistics with minimal impact on their overall profile. Since linguistics must constantly work to prove its vibrancy and effectiveness, it would help a great deal if its publication practices made this easier to demonstrate.

The vibrancy of the field of linguistics would be apparent if the most important new ideas and findings in the field consistently appeared in its leading journals, and if linguists were to regularly read those journals expecting to learn about the latest and greatest results. In fact, this rarely happens: publication cycles are very slow, often taking years from submission to publication; influential work tends to appear elsewhere, frequently bypassing peer review entirely; and mistrust of the peer-review process is widespread among linguists. It is perhaps telling that the notion of a 'journal club', which is common in some neighboring fields, is almost unheard of in linguistics.

Fortunately, it doesn't need to be this way, as shown by the many effective journals in neighboring fields (my main experience here is with psychology and neuroscience). It is conceivable that if just one leading linguistics journal were to achieve similar effectiveness then it would become so attractive to authors that other journals would soon need to fall into line. The following is a list of suggestions, largely based on observations of neighboring fields.

(1) The peer-review process works better when most individuals in a field are invested in its success. The more linguists are dependent on fair, prompt reviews of their own work, the more linguists are likely to deliver fair, prompt reviews. Senior figures in the field need to show leadership in this regard, and they do a

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disservice to the field if they choose not to include their best work in the peer-review process. (Of course, if everybody participated, we would need more journal space.)

(2) A practice that is common in cognitive science and especially in neuroscience is for a senior researcher to co-author journal reviews with a junior colleague (e.g. advanced graduate student or postdoc). I recognize that this practice strikes some linguists as appalling, but it has a number of clear benefits. It provides valuable experience for the junior colleague; it ensures that a couple of people have discussed the merits of the manuscript before the review is written, leading to a more considered opinion; it discourages ad hominem reviews; and it even speeds up the process, since the junior colleague is likely to remind the senior reviewer to move the process along. Copies of action letters and all reviews should be e-mailed to all reviewers, as is standard in other fields. This provides additional guidance for new reviewers, may help to discourage unfair or excessively lengthy reviews, and may help to give authors consistent feedback.

(3) Publication cycles must be faster if journals are to increase their impact in the field. This would be helped if submission and review were fully electronic (no exceptions). This can include the automated and frequent e-mail reminders to late reviewers that are now commonplace in many journals. If editorial micromanagement is adding weeks or months to the process, then it should be reduced, particularly where expert associate editors can be relied upon. Proofs or pre-prints of accepted articles could be made available on the journal's web site as soon as they are ready, even before they are assigned to a specific print issue. Many journals now follow this practice, which can often remove half a year or more from the effective publication cycle.

I sometimes hear linguists remark that reviews take more time in our field than in fields like psychology or neuroscience because the papers tend to be longer. Although length may play some role, I think that its contribution is exaggerated. In my own experience of submitting to journals in different fields, linguistics journals take an average of seven to eight months per review cycle, whereas journals in psychology and cognitive (neuro)science take an average of three to four months per review cycle. (Multiply these numbers by three to get an approximate time from submission to publication.) This is for articles of similar length, on similar topics, and possibly even drawing on the same pool of reviewers, so there must be something that the linguistics journals could do better. One step could be for journals to demand shorter review times. If we agree to do something in four months, it will likely drift to the bottom of the proverbial pile, but if we agree to do something in one month, it may not make it to the pile at all.

(4) Linguistics needs more individuals to participate in the editorial process, for shorter periods of time. Most leading journals in cognitive science have a regular rotation of editors and associate editors, helping to ensure energy, new ideas, and collective investment in the editorial process, not to mention increased trust in the fairness of the process. In linguistics the professional associations set a good example in this regard, with somewhat regular turnover at *Language* (LSA) and the *Journal of Linguistics* (LAGB), but at many other journals in our field, editorial turnover is very rare.

(5) Finally, and this is probably both unpopular and difficult to implement, there should be some consequences for (ir)responsible participation in the peer-review process. The March 2006 editorial raises the suggestion of expediting the review process for *Language* by relying on a cadre of trusted, prompt reviewers. This sounds like a powerful disincentive for trustworthy, prompt reviews. What might be the impact on the field if, say, instead, linguistics journal editors were to pool information and create some kind of 'credit rating' system? Would young scholars be motivated by being able to list on their CV an 'A+ professional service rating from the LSA', say, for delivering 80% of reviews on time? Would you hesitate to hire a colleague who has a C-rating? Or what might the effect be if late reviewers had their own submissions sit in editorial purgatory for the length of time that they were late themselves?

I think that Stephen Levinson's letter raises issues that are very important to the health of the field of linguistics. It does not much matter whether we consider linguistics to be a 'traditional humanities discipline' or not. If linguistics does not function like the most vibrant fields, and in ways that make this obvious to administrators, funders, and students, then it will increasingly lose in the competition for resources.

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Editor's reply: I welcome this interesting perspective and especially appreciate the very pragmatic stance on the place of linguistics in

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the academy. I agree that the question of where we stand has less significance for the health of the field than how we stand. As for the suggestions for best practices to follow, I am pleased to report that we do send copies of my editor's decision letter plus the associate editor's and referees' reports to all referees on a paper for the very purpose noted above, that is, referee 'education'. And even though we are not fully electronic and automated in our submission and review process, we do make good use of this medium to effect savings of time (and money). I concur with the observation about the importance of senior leadership not only in submitting their work but also in serving as referees and taking the task seriously—I confess to being somewhat dismayed that some choose not to take part in the review process, but for the most part, participation by senior figures is high. Finally, the idea of a rating system for referees that could be listed on CVs and thus is at least somewhat public—all editors seem to have their own private 'black list'—is an intriguing one; reactions to this, anyone?

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