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.

A historical corrective

January 22, 2005

To the Editor:

These remarks concern intellectual credit for certain ideas in Ch. 6 of my book, *Skeptical linguistic essays* (New York: Oxford University Press), published in January 2004, credit which should have been given but due largely to faulty memory on my part was not. I am greatly indebted for this opportunity to set the record straight in the pages of *Language*.[1]

In going through some old boxes of papers in mid-2004, I discovered a handwritten letter to me from a former lifetime member of the Linguistic Society of America (LSA) named Lester A. Rice. The secretariat of the LSA has since informed me that, unfortunately, Mr. Rice died in 1994. He was apparently one of the students in a large syntax class I gave at an LSA summer institute at Indiana University in the summer of 1964. I reproduce here most of his letter, including everything relevant to the issues at hand. I note that he indicates in his letter that he had dropped out of linguistics and was then living in Spain.

Letter from Lester A. Rice to Paul M. Postal (May 15, 1986)

Dear Paul:

With regard to Terry's and your *The vastness of natural languages*,[2] which I haven't seen and know only through Abbott's review in Lg.,[3] I would like to add a couple of observations which I believe bear on vastness and even greater vastness.

Notice that in indirect quotes in English the quoted material appears in well-formed English, generally. However, in direct quotes there seems to be little or no constraint that the quoted material be well-formed, or in English, or limited in any way as to length, or even be 'linguistic' (this letter if we include written material). In particular, I can cite a sentence recently published in an article in Lg.,[4] slightly paraphrased:

> The man asked the woman what she had for lunch and she answered [sakatsho:ri nii] and he replied [A:ke ki' na:'a tsi nikanutaraku] and a further discussion of soup followed.

Since there is no restriction on what follows the words 'answered' and 'replied', we are free to insert at each of these points the entire set or collection (for instance) of the sentences of Mohawk; or its power set; or the former arranged and conjoined in all possible permutations; etc. And in general A SIMPLE SENTENCE of any one NL [natural language] can contain, in this way, at once, ALL OTHER NLs, in addition perhaps to much else.

Even on a level of sentence production and interpretation, folklore tales permit a narrator to create personages and their dialog; in this way a linguistically sophisticated narrator has the power and knowledge in principle to represent or imply representing in some fashion all NLs. (In finite terms, this would mean by token and by 'Selbstverstandlichkeit'.) Thus, 'He said ''X''', where X represents all earthly NLs past present and future, plus all NLs which by their nature will never cross the human mind, is merely a single S of English. It's an impeccable and simple S of English. Each NL must at once contain every other NL.

Paragraph two of the letter makes abundantly clear that Mr. Rice had observed one of the key factual bases for the conclusions of Ch. 6 of my book. He had noticed in particular that there are well-formed sentences in natural language L some of whose parts are not composed of elements drawn from any conceivable lexicon or morpheme listing of L. He gave such an English sentence in which the two parts not drawn from the English lexicon each involve elements of the Iroquoian language Mohawk. In Ch. 6 I cited several other classes of facts which support parallel inferences about natural language and concluded inter alia that on these bases alone, natural languages cannot have generative (proof-theoretic) grammars in the technical sense; see for example Geoffrey K. Pullum and Barbara C. Scholz, 'Contrasting applications of logic in natural language syntactic description' (*Logic, methodology and philosophy of science 2003: Proceedings of the 12th International Congress*, ed. by Petr Hajek, Luis Valdes-Villanueva, and Dag Westerstahl, Amsterdam: Elsevier, 2005) for an account of the difference between proof-theoretic grammars and model-theoretic grammars, which are

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inherently in no way incompatible with the cited data types. That is, Mr. Rice's observations form part of the foundation for an argument that adequate natural language grammars must be proof-theoretic.

To the extent that this is a valid conclusion, Mr. Rice certainly deserves credit for first noticing, or at least first making explicit, an important part of its factual basis. Although I cannot say that I did (or did not) draw on some recollection of Mr. Rice's letter at the time I was writing Ch. 6, that is irrelevant. His 1986 insight is a fact independent of how my memory dealt with it. I sincerely regret that appropriate recognition was not provided in Ch. 6 at a time and place where it would obviously have been most appropriate.

It might be added that some of the conclusions that Mr. Rice drew in his third paragraph are in fact incompatible with the argument of *The vastness of natural languages*, which Mr. Rice cited. Since he knew of the latter work only via its review by Barbara Abbott in *Language*, this is not surprising. But the claim of that book was that the sentences of each natural language form a collection too vast to be a set, a collection standardly called a PROPER CLASS. That work also claimed that each individual sentence, even the infinite-sized ones it argued to exist, had to be regarded as a set. It is not a coherent idea to specify that a set could in any sense contain a proper class as a 'part'. If such reasoning is correct, and the logic justifying it underlies the reasoning of the main argument in *The vastness of natural languages*, then Mr. Rice was simply wrong to conclude that a particular natural language sentence could contain as a part ALL of the sentences of some other natural language. It is not precluded, however, that a single (infinite) sentence could contain infinitely many sentences from other languages, even nondenumerably many, at least it is not precluded by any publicly available reasoning known to me.

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[1] [Editor's note: Although this letter was received some eighteen months ago, I held off publishing it until the review of the author's book could appear, thinking that the letter would thus have greater impact. See pp. 442–46 of this issue for Robert Borsley's review of said book.]

[2] [Editor's note: By D. Terence Langendoen and Paul M. Postal, Oxford: Blackwell, 1984.]

[3] [Editor's note: Reviewed by Barbara Abbot, *Language* 62.154–57, 1986.]

[4] [Editor's note: The article in question is 'On the nature of noun incorporation', by Marianne Mithun (*Language* 62.32–37, 1986), and the cited material is adapted from her ex. 2 and surrounding text (p. 34).]

Editor's reply: I am glad to be able to publish this piece and commend Dr. Postal for taking the trouble to put this bit of linguistic history before *Language*'s readership.

On Siegel on the bioprogram

March 16, 2006

To the Editor:

Jeff Siegel's letter ('On the language bioprogram hypothesis', *Language* 82.1.2–4, March 2006), attacks the 'four tenets' of bioprogram theory but adds nothing to criticisms made and refuted many times already.

The first tenet is that creoles were created 'in one generation' by children exposed to 'a highly variable and undeveloped incipient pidgin' (2). Nowhere have I claimed that this generation must be the first locally born generation. Siegel's version of history (that G1, the original immigrant generation, communicated via their native languages and Hawaiian, largely ignoring pidgin, while G2, the second locally born generation, spoke pidgin and G3, the second such generation, spoke creole) thus actually supports rather than refutes the single-generation claim.

Siegel must therefore try to refute the second clause of the tenet by claiming that G2 created a stable, expanded Hawaiian Pidgin English (HPE) and this, rather than an 'incipient' pidgin, formed G3's input. Unfortunately, there is not a shred of evidence that a stable, expanded HPE ever existed. The primary data, *Language in Hawaii from 1789 to 1960: The documentary sources*, by Julian M. Roberts (Manoa: University of Hawaii, mimeo, 1995, containing over 2,000 contemporary citations), directly refute Siegel's claim. They attest only a primitive, unstable, unstructured HPE, plus something indistinguishable from modern Hawaiian Creole English (HCE; in contrast to the real, unstable HPE, its sentences are regularly structured and contain such typical HCE features as the *bin* tense-marker, finite purposive clauses headed by *for*, and pleonastic pronouns). Moreover, whenever speakers' ages are reported, they are invariably described as

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'children' or 'adolescents', just as the bioprogram would predict.

Since there was no stable, expanded HPE, Siegel has to pretend that early HCE was 'really' an expanded pidgin, developed 'because of a large influx of immigrant workers speaking . . . Japanese, Korean, and Spanish' (2). Yet Roberts's data contain not a single attestation of any immigrant speaking this 'stable HPE', and none of the immigrant speakers (some of whom arrived before 1910) that I interviewed in the 1970s spoke it (see *General phonology and pidgin syntax*, by Bickerton and Carol Odo, Manoa: University of Hawaii, mimeo, 1976). Never before has anyone even suggested the existence of a pidgin spoken only by native-born speakers.

The second bioprogram tenet Siegel attacks is that children had to go beyond their deficient (pidgin) input and tap innate faculties to create a language. Siegel's sole argument repeats his claim of a stable HPE. But the fact that HCE took pidgin as its primary input but supplemented it from innate sources is well supported by evidence; space precludes more than a single, if telling, example. HPE event-sequence markers (it had no true tense) were *pau* 'finished' (used for past or previous events) and *baimbai* 'later' (used for future or subsequent events). HCE preserved these but did not alter either their clause-external positions or their quasi-adverbial status, instead superimposing a new tense-aspect-modality system identical to systems found in other creoles (see Bickerton 1981, *Roots of language*, Ann Arbor: Karoma).

The third bioprogram tenet relates to similarities between creoles known since F. Adolpho Coelho's 'Os dialectos romanicos o neo-latinos na Africa, Asia e America' in 1880–86 (*Boletim da Sociedade de Geographia de Lisboa* 2.129–96, 3.451–78, 6.705–55), which it explains as resulting from an innate default grammar. Siegel assumes that if any creole violates any bioprogram prediction at any point, the hypothesis collapses. He ignores my numerous descriptions of the many factors, social, demographic, and historical, that interact with the bioprogram to produce a spectrum of creoles from 'most radical' (e.g. Saramaccan) to 'least radical' (e.g. Bajan, Reunionnais) (see especially Bickerton 1984, 'The language bioprogram hypothesis', *Behavioral and Brain Sciences* 7.173–221). Hawaii, as I have always stated, lies toward the 'least radical' end of the continuum, and contra Siegel I have never claimed that this resulted from decreolization. A majority of schools taught in English even before the creole began, something found in no other creole-speaking area. What is remarkable is not that HCE lacks some creole features, but that, given its long and deep exposure to English, so many creole features still emerged.

The final bioprogram tenet, in Siegel's version, is that creole features did not derive from substrate features. I have never denied that phonological, lexical, and semantic features often derive from substrates, and—under exceptional circumstances—even syntactic features may be adopted: the Bantu sentence-final negative particle found in Palenquero is one example. All I claim is that the core syntax of creoles—including such features as determiner systems, tense systems, and verb serialization—derives from universal rather than substrate sources.

Siegel states that my arguments against substrate influence in HCE are based 'mainly on comparisons with Japanese and Filipino languages' (3). This is false. My main argument has always been the syntactic similarities between HCE and other creoles. Siegel assumes, like many substratists, that if a form exists in a substrate language and a creole, the second can derive only from the first. Thus Siegel regards the nonpunctual marker *stay* in HCE as deriving from Portuguese *estar* rather than from its nearer and more obvious source in English *stay* (Siegel 2000, 'Substrate influence in Hawai'i Creole English', *Language in Society* 29.2.197–236). But the lexical source of the item is trivial as compared to its other aspects. Far more important is that in its syntax and semantics HCE *stay* is identical with other nonpunctual markers (e.g. *de* or *(t)a(n)*) in creoles without any Portuguese influence). There are many more cases in which feature X, found in creoles A and B, has a possible substrate source in A, but not in B. Where, then, did feature X in B come from? Since languages work within a narrow range of possibilities, the logical conclusion is that the presence of X in A's substrate is coincidental (or had a merely reinforcing effect) and that X derives from some form of innate template for language in both A and B.

Finally, I must comment on the editorial reply, which mistakenly suggests that the bioprogram hypothesis was once 'received wisdom' in creole studies, but has been superseded there, if not elsewhere, by 'advances' in that field. In fact, the bioprogram has always been a minority position. Moreover, in contrast to the history of generative grammar that the editor cites, there have been no theoretical 'advances' in creole studies since the bioprogram first appeared. To the contrary; creolists continue to recycle the same arguments—substrate versus superstrate, diffusion versus polygenesis—that were debated in the 1960s. No plausible alternative to the bioprogram has yet emerged.

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Contra Siegel and the editor, continued acceptance of the bioprogram by mainstream linguists results not from ignorance of recent work, but rather because it is the only truly explanatory theory in creole studies (no other theory explains the similarity among creoles, the least predictable, and therefore the most interesting fact about them) and because it makes good sense in light of all we know about language. In any real science, standard practice is not to nitpick at an explanatory theory, but to accept it provisionally while seeking either to improve it or to replace it with something better. The purely negative response of Siegel and other bioprogram critics is not merely unscientific—it also serves to isolate creole studies from the rest of linguistics and stall it in a quagmire of outdated notions.

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Editor's reply: As in all cases, I welcome this further discussion. The *Language* 'rules of engagement', so to speak, are a bit murky, however, on the matter of who gets to have the last word in this most interesting debate; in principle one would like each side to have an equal number of opportunities to present its case, but it is not clear who spoke first here: Dr. Siegel with his letter or Dr. Bickerton with his earlier published accounts of his hypothesis that Dr. Siegel reacted to. Accordingly, I am choosing to let the discussion stop here, at least as far as the recent pages of *Language* are concerned, in that each party has now had a say in the journal. With both points of view now represented, readers can judge the arguments, check cited sources, and decide for themselves. (I do note that the discussion can be seen as continuing, however, in an article by Dr. Siegel in *Studies in Language* 30.4, 2006).

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