

## TOPIC...COMMENT

### *Nobody goes around at LSA meetings offering odds*

I believe that if you try hard enough, and steel yourself against the initial pain, you can almost bring yourself to almost enjoy finding that you have been refuted; there can be something salutary or even cathartic about the experience of having your pronouncements and predictions wrecked by the chariot of history as it thunders past, trampling over what you have done. There is an element of masochism in this, no doubt, but at least anyone who gets refuted isn't being *entirely* ignored, and that's worth something.

Nonetheless, I must confess I was taken aback at how fast my claims were covered with wheel ruts and hoofprints after the appearance of 'Footloose and context-free' [*NLLT* 4 (1986), 409-414]. Almost as soon as it was published, I learned that my best efforts at chronologizing and attributing the discovery that there were non-context-free human languages had not been good enough. Ger de Haan wrote to inform me that a crucial publication had been overlooked.

Readers of the saga will recall that Riny Huybregts was mentioned in passing as the originator of an argument from Dutch that introduced the most interesting new class of data to emerge in the whole affair, but I noted that the facts he cited were not sufficient to establish a valid argument about the character of the Dutch string-set. Shieber was cited as having later developed a valid argument by extending the Dutch argument into the realm of Swiss German. Well, quite unknown to me, a book published in Holland in 1984 contains something highly relevant.

The book is *Van Periferie naar Kern*, edited by Ger de Haan, Mieke Trommelen, and Wim Zonneveld [Foris, Dordrecht, 1984]. The title is in Dutch; the title page is in Dutch; the preface is in Dutch; the heading of the preface where it should say "Preface" is in Dutch (it says "Voorwoord"); 12 of the 17 papers included are in Dutch; even the Foris catalog description is in Dutch. I mention all this in a pathetic effort to mitigate the blame that will attach to me for being unscholarly and ill-read in this matter.

Dutch happens to be a language in which my unfettered human capacity for free expression and comprehension of thoughts is rather

fettered. In fact, all I can convincingly say in Dutch is that I would like to have een broodje met warme worst en een koffie met melk alstublieft. (This ability once saved me from starving to death during a GLOW conference in Amsterdam when no one would have lunch with me because of my position on the bounding of movement rules). So left to my own devices, I would not have tried to read any of *Van Periferie naar Kern*, thinking that it was all in Dutch and thus beyond my competence. I would guess that the title has something to do with the fabled but unexplicated core and periphery distinction. It doesn't seem to imply that there might be any mathematical linguistics inside.

But in fact, among the five papers in English is one by Riny Huybregts entitled 'The weak inadequacy of context-free phrase structure grammars.' In this paper, Huybregts answers the arguments that Gazdar and I had made in 1982 concerning the failure of the case made in his 1976 paper (for references, see 'Footloose and context-free'). He not only fleshes out the Dutch argument with some new data involving subcategorization of adverbs that appear to defeat one strategy we employed, but he also brings up the Swiss German data (specifically, Zürich German or *Züritüütsch*), and makes an argument based on those facts.

This gives Huybregts clear publication priority over Shieber. Below the voorwoord, indicating when it went to press, the volume has the date 'januari 1984'. (Sometimes, as here, Dutch looks remarkably like English as typed by the guys at the Computer Center.) Shieber's independent publication on *Züritüütsch* appeared in the August 1985 issue of *Linguistics and Philosophy*, though it is not possible to tell when the paper was received, because *Linguistics and Philosophy* is a journal which, to its lasting shame, still does not print the date of receipt with each article published (see 'Stalking the perfect journal,' *NLLT* 2 (1984) 261-267).

Huybregts' first publication of the *Züritüütsch* argument can be tied down to an even earlier date, in fact. His paper is marked with a postposed ° on the contents page, and at the bottom of that page it says, "(De met ° aangeduide bijdragen werden op 20-22 december 1983 als lezingen gerepresenteerd op Biltstraat 200)". Now, this is Dutch again, but I'm not so inept at my craft that I can't puzzle it out with a little help from the dictionary. It means, plainly, that the with ° indicated contributions were upon December 20-22 1983 as readings represented upon Biltstraat 200.

Biltstraat 200? The book is coy about this. "De Biltstraat, het

woord zegt het al,” begins the voorwoord disingenuously. Of course the word doesn’t say it all; who are they trying to fool? But I have figured out that it’s an address in Utrecht, the address of the Instituut A. W. de Groot voor Algemene Taalwetenschap, where Huybregts gave his paper as a lecture just before Christmas 1984.

This leaves little doubt that Riny Huybregts has an unassailable claim to being the discoverer of the Swiss German argument that languages with the peculiar word order properties that Dutch and Züritüütsch seem to have will never submit to context-free phrase structure description. Indeed, he is perhaps the first person to publish a valid non-context-freeness argument about a natural language – though I have not yet gone over his work with a fine-toothed comb looking for errors and loopholes, and this really needs to be done.

It is a pity, in fact, that Huybregts’ result did not undergo the scrutiny of referees and appear in a recognized international journal; perhaps the social pressure on him to regard the result as “unimportant” because it was about weak generative capacity was sufficiently strong that he did not want to risk making the work too visible. I, for my part, consider Huybregts’ result significant and interesting, and I regret the fact that although I regularly read seven or eight journals and attend numerous conferences, neither I nor anyone I knew in the field was aware of Huybregts’ result until Ger de Haan sent me his book and told me where to look. Our techniques for communicating results should be working better than this.

There is a second respect in which the views I confidently enunciated in ‘Footloose and context-free’ turned out very rapidly to elicit a refuting knuckle-rap from the cosmos. I suggested in that column that mathematicians constituted a group of academics who behaved in a much more orderly and responsible way than their bumbling, amateurish, and ungracious linguistic counterparts. I was thinking of the chaotic publication policies of linguists (“see Snodgrass, unpublished manuscript”), their erratic grasp of what was or was not a result (“Snodgrass’s example is dubious at best”), and their habit of squabbling wildly over not only whether something had been shown but even what we were supposed to be talking about in the first place (“Snodgrass’s arguments do not even bear on the issue”). And I was moved by one recent sterling example of comradeship in mathematics to suggest that mathematicians behave much better.

Dream on. Mathematicians, as I guess I knew all along, are people too. And sometimes...

Well, let me begin at the beginning.

In 1986, in the March 20 issue of *Nature*, it was reported that two mathematicians had announced a proof of Poincaré's conjecture. Such a proof, let me make it clear, would be Big News. The conjecture, about as old as this century, says that what is true of the 2-dimensional surface of the (3-dimensional) sphere is also true of the 3-dimensional surface of the 4-dimensional analog of a sphere: each is the only simply connected surface with that number of dimensions. (A surface is simply connected if and only if any loop on it can be shrunk down to a point without cutting through the surface.) The problem is celebrated, and quite fascinating to the sort of person who thinks so abstractly that they see no difference at all between the shapes of a ring doughnut and a coffee mug, or between either one and a needle or a door key.

The two mathematicians who claim to have proved the conjecture are Colin Rourke of the University of Warwick, England, and his graduate student Eduardo Rêgo, now of the University of Oporto (Portugal). And all unseemly hell has broken loose about the alleged proof. According to the *New York Times* of September 30, 1986, Rourke and Rêgo "have circulated, withdrawn, revised and reissued several versions since January", and there is still no refereed publication in sight. Their latest version was a 123-page manuscript issued in late September 1986.

And has sweet reason and respectful acknowledgement been their reward for their willingness to get their latest results out to their colleagues via semipublication? Not a bit of it. Rourke told *Nature* and *New Scientist* about the result before there was consensus in the topological community that it was really a result, and other mathematicians are clearly incensed. Joan Birman of Columbia University remarks that "There's some skepticism in the community because there have been many false proofs [of Poincaré's conjecture]" (*Science News* 129.14, April 5, 1986, p. 215). Indeed there have. In fact, Wolfgang Haken spent some time struggling with the problem fifteen years ago, using exactly the techniques Rourke and Rêgo were now using, but was able to show that at a certain point the approach failed and the result could not be achieved. So mathematicians were indeed skeptical. "Too sketchy to confirm," was the sort of comment they made to the *New York Times*.

Michael Freedman, of the University of California, San Diego, told the *New York Times* that people are betting "as high as 200 to 1" that the proof is not correct. This is even more cruel than I can remember linguists having been; nobody goes around at LSA

meetings offering *odds* that the *Barriers* framework can't account for English parasitic gaps. And keep in mind that Michael Freedman was presented with the Fields Medal in August 1986 for proving that the 4-dimensional case of the Poincaré conjecture is true; if even he cannot understand the proof of the 3-dimensional case, we are in deep trouble.

Meanwhile, Rourke defends himself against the charge of having laid claim to an important result on the basis of an unverified proof. He apparently feels that the mathematics community ought to buckle down and figure out that his proof is correct. "They're sort of sitting back and waiting for someone else to do the work," he is reported as saying; "Mathematicians are sort of lazy." (Rourke sort of uses the *sort of* construction sort of excessively.)

Rourke's position, in other words, is that the duty of mathematicians, when faced with a couple of hundred pages of a proof so difficult that Michael Freedman cannot understand it, is not to toss it back and demand a better exposition, but to sit down and do the hard work of verifying that it really does all make sense, and then hand the credit to the author of the obscure material from which they painstakingly hewed out the crucial nuggets of truth. If they fail, he retains the right to accuse them of having been too "lazy" to discern the merit of his leading ideas. But if they succeed, he can claim full credit for the proof that thereby emerges, and point to the clarified version as simply a restatement of his own original thinking. Does this sound at all familiar to you?

The whole episode sounds to me a lot like the behavior we regularly see in linguistics. The unrefereed manuscripts circulated to a circle of initiates; the fixes and alterations as initial objections are raised; the vague and sketchy arguments; the early announcements of victory; the rapid onset of doubt in the community at large; the indignant rebuttals; the bitter accusations of a biased or lazy profession; it's all so familiar. "His [Rourke's] approach is not technically complete. He hasn't been careful about the technical details," says William P. Thurston of Princeton. "There just wasn't a proof there," says Robion Kirby of Berkeley. This is the way linguists talk about each other (with good reason, of course, in many cases).

Thus it might look from this example as if mathematics is not quite the perfect exemplar of scholarship that I might have appeared to seem to tend to imply that it was. They bicker, just like us, and can't even manage to settle what is or what is not a proof or a result.

Well, not quite. Mathematicians are not gods, to be sure, and they get mired down in vicious disputes sometimes, like scholars in any other field. In fact, there was a dispute in algebraic topology that led to a year-long period in which conflicting proofs had been published, each entailing that the other was erroneous, and one Rutgers University mathematician (on the side that proved to be right) became so disgusted with the state of things that he resigned his tenured post and left the field. But I still think there is a contrast to be pointed up; mathematicians still don't run quite as shabby an operation as theoretical linguists.

In November 1986, Rourke visited Berkeley to spend a week presenting his supposed proof. Heavies from Southern California came up to listen and participate, among them David Gabai of Cal Tech, who tells me that about three months of his research time during 1986 was spent in trying to understand Rourke's work. On the fourth day of Rourke's presentation, which was confusing and even sloppy at many points, Gabai asked a question about a particular diagram on the board, in an attempt to clarify exactly how it was to be understood in the light of the reply to an earlier clarificatory point. Rourke attempted to address the matter, but as he worked, it suddenly became clear that the example, when analyzed closely, was a counterexample. A series of entities each greater than some of the others had to exist, but clearly could not exist. The lemma could not be established, and the proof had collapsed. What's more, the problematic part of the proof was exactly the bit that Wolfgang Haken had warned about years earlier. Rourke mumbled something about being sure the difficulty could be overcome and the hole patched, but no one took this seriously; the whole episode is over, the proof is dead.

For in mathematics, the force of counterexamples is different. They are not rebutted with rhetorical defenses. Linguists dismiss facts as "mere facts"; they distinguish between counterexamples and mere exceptions; they point out loftily that apparent counterexamples and unexplained phenomena should be carefully noted, but it is often rational to put them aside pending further study when principles of a certain degree of explanatory power are at stake; they assign selected phenomena to the marked periphery; they digress into philosophy of science and point out that there have been cases in history of apparent counterevidence that turned out not to be such; they will do anything, up to and including telling flaming lies, to avoid being in the situation that up to time  $t$  they thought that  $P$  was

a principle of linguistic theory but at a later time  $t'$  a counterexample was adduced which showed that  $\sim P$ .

I was certainly wrong if I implied that mathematicians always behave in a civilized and organized way while linguists are slugging in out with abuse, vagueness, and obscure methodological defense mechanisms. But I think it's still true that we would be better off if linguists behaved a bit more like mathematicians at their best, and I think it still looks as if mathematicians at their worst don't sink quite so low as linguists.

In the world of linguistics, Rourke and Rêgo would have been published without question in any of the student linguistic society conference volumes, and quite possibly in a refereed journal. What's more, they would probably have replied to the subsequent critical rejoinders, accusing the putative refuters of myopic empiricism. I'll lay odds on it.

Received 3 February 1987

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Note

The views expressed in TOPIC...COMMENT are those of the author. They should not be construed as representing either the editor or the publisher of *NLLT*.