

## Letters to the Editor

I do not believe that Geoffrey Pullum, in his attack on my *Antilinguistics* (*Computational Linguistics*, 17(2), 240) is trying to protect his livelihood. I hardly think he is in any danger there. But I do think he is protecting the intellectual pride of himself and his colleagues. That is reasonable and to be expected. What is not reasonable is that in doing this he does not report, let alone try to answer, a single one of my general arguments or, with one possible exception, any of my specific points. His method of review is to mock remarks that he has stripped of all their reasoned context, and to scorn conclusions without even hinting at the existence of the large amounts of evidence I present in justification. (Luckily for my confidence there have been other reviewers who have shown greater appreciation of my rational powers.)

Most serious of all Pullum's mistakes is perhaps that several of his complaints are based on the very assumptions that I question in the book. His talk of Quirk's "monumental descriptive work on modern English" without mentioning any part of my case against it is one example. His contempt for my failure to master primary sources is another. I should probably have emphasized my argument about this more. But it is there, on pages 2, 195n, and 258. It is surely significant that Pullum uses p. 195n, not to explain my position, but to attack the book's poor production.

He is quite right to criticize the mistakes in names (I apologize to the people concerned), the faulty index, and missing arrows. It was careless; I should have checked the proofs better. Pullum is careless himself. He misquotes a sentence from p. 56; and in the index it is only the proper names that have two figures too high (and from p. 170, not p. 180 as he states)—all the other entries are correct.

In the only case where Pullum really attempts a proper discussion of the material to show that "when by chance Gethin gets hold of the linguistic ball for a moment he unfailingly drops it") his criticism is typical of the barren formality that I complain of: I should have located the ambiguity of *Flying planes can be dangerous* in the transitive/intransitive contrast in *fly*, not in *-ing*. He thus abandons reality, the words actually used, for the sake of an abstraction. And I am not simply insisting on a principle here, for in the process Pullum gets it wrong. The transitive/intransitive difference in the meanings of *-ing* is not the crucial one, and may not be there at all, as can be seen in, for example, *The burning sun.../Burning wood (is wasteful)*, where (*burn*)*ing* is transitive in both cases, but has different meanings. At the same time I cannot think of any sentence where there would be any transitive/intransitive confusion through the use of an infinitive, indicative, or imperative. Can Pullum?

It is sadly revealing that he suspects that my attack on Quirk's grammar is prompted by a desire to settle scores with prestigious linguists at British universities. I have no scores to settle with anyone. Is he so used to academic in-fighting that he cannot believe that I have no personal quarrel, only a general quarrel with the attitudes and assumptions, purposes and pretensions, methods and thinking, of academic social 'science'?

I am impatient because while social 'scientists' claim authority, they have failed, I believe, to deliver real results, and yet at the same time exercise intellectual dominance over the rest of the community. I repeat something I say in my book. If academic experts think their work has any importance, that it can affect people's lives in any way,

they have a duty to debate their work and ideas with any nonexpert members of the public that are interested. There would be a great many more people actively interested than there are now if they were not normally cowed by the sort of attitude displayed by Pullum in his review. I anticipated what is happening. It's fine for academics to savage each other. But immediately somebody attacks them from outside they close ranks.

If this letter is published I hope he will have the courage to respond with a serious attempt to rebut in a reasoned way at least some of my broader propositions: linguistic analysis serves no useful purpose; there is really no such thing as structure (as opposed to practical logic) in language; linguistics has not improved the learning of foreign languages; thought is essentially independent of language but is in practice corrupted by it; etc.

But if Pullum has real courage he will seek a much wider forum than this journal, a forum where he should discuss these matters without being assured of the sympathy of virtually all his 'insider' readers as he can be here. If he can persuade a more general journal, or a newspaper, to publish his opinions on these matters (and to explain to use why computational linguistics is important), I shall be happy to respond. It might also induce both of us to be a little more temperate.

Amorey Gethin  
Cambridge, England

### **What is *Semantic Structures* About?**

It is not clear how useful it is to reply to Yorick Wilks's dismissive review of my *Semantic Structures* (SS) (*Computational Linguistics* 18:1, March 1992). Nevertheless, I will attempt to say briefly how I think Wilks has misconstrued the enterprise.

Wilks describes SS as "a large project to construct semantic or conceptual expressions of word meaning on which inference can be done." A substantial part of his criticism flows from the book's failing to meet this description in a way that satisfies him (about which more in a moment). But constructing formalized semantic expressions for words is only one of the goals of the book. The general goal is "the characterization of the mental resources that make possible human knowledge and experience of the world" (p. 8). More precisely, "What are the innate units and principles of organization that make human lexical and sentential concepts both possible in all their variety and also learnable on the basis of some realistic combination of linguistic and nonlinguistic experience?" (p. 11). In addition to conceptual structures being an interface between language and inference, they can be compared with "conceptual structures derived from sensory modalities" (p. 11). In short, I conceive of the inquiry as thoroughly psychological, and not just a technical work on "handcrafting lexical codings," as Wilks puts it.

SS itself spends only one chapter on philosophical and psychological foundations before moving on to technical details of formulating the relation between lexical structure, syntactic structure, and conceptual structure. As the text makes clear, the reason for this is that I have written two previous books dealing in much more detail with philosophical and psychological foundations: *Semantics and Cognition* (Cambridge, MA: The MIT Press, 1983) and *Consciousness and the Computational Mind* (Cambridge, MA: The MIT Press, 1987). The first chapter of SS is a precis of the positions developed and defended in this previous work.

One of Wilks's main complaints about SS is that it does not make sufficient mention of the AI and CL literature on lexical and conceptual semantics. Indeed, he insinuates

that if I had paid attention to this literature, I would have seen that my approach was pointless, since everyone in AI and CL gave it up long ago.

I have two responses to this criticism. The first is that life is short, and one has to choose what to read out of an inexhaustible literature in many fields. One is always at one's peril at having missed something important. If I have missed something important, I am ready to accept responsibility for it and try to do better next time.<sup>1</sup> This work merited citation in the section on aspectual structure on p. 28, and I apologize to James for the omission. On the other hand, I did read a fair amount of the AI and CL literature during the seventies and early eighties, when it appeared to be similar to what I was doing; there are not a few references to this literature in *Semantics and Cognition*.<sup>2</sup>

However, I felt at the time that the empirical justifications of most AI-CL work were insufficient: it was too often difficult to say why one particular solution was better than any other, and which decisions about representation were being made on a principled basis. I therefore decided that, while the AI-CL tradition might produce interesting simulations of limited aspects of language understanding, it was not going to teach me much about the overall nature of human language, and so I abandoned contact. (In a sense I feel vindicated by Wilks's saying that no one does this sort of work anymore: he is telling me that I was right in thinking it wouldn't work.)

SS, by contrast with much of the AI-CL tradition, attempts to justify every step of formalization and to distinguish between essential and inessential aspects of the formalism. It appeals to a wide range of linguistic data, not just the fixed given corpus of many AI implementations. Like other work in the linguistic tradition, it is deeply concerned with accounting for what does not happen—hence its preoccupation with constraining the theory so as to explain the ungrammaticality of sentences that are in principle plausible. Wilks, however, seems to project onto the book his own goals for linguistic analysis, and apparently does not recognize the difference in methodology as flowing from a different outlook. As a result, he sees nothing but a lot of effort spent on what seem to him insignificant details.

But many of the seemingly insignificant details offer a great deal to our understanding of the organization of language. SS spends considerable effort showing how to simplify lexical entries of verbs and prepositions so as to account for the range of syntactic frames in which they can and cannot occur, as well as for the differences in meaning they carry in different frames. In addition, important classes of lexical items are unified. For example, causative verbs, treated in almost everyone's theory as expressing a conceptual primitive CAUSE, are shown to form a natural class along with verbs of helping, permitting, trying, resisting, and entailing. In the process of treating these relations formally, the alleged primitive CAUSE dissolves into a combination of more general predicates and features. Finally, three chapters of the book are devoted

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1 Wilks takes me to task for not citing my Brandeis colleague James Pustejovsky; he refers to a book allegedly published in 1991 that actually was completed only at the end of 1991 and is still not published. In fact, the only work of Pustejovsky's that was in citable form in 1989, when SS was completed, was his work on event structure, which finally achieved real publication in *Cognition* in late 1991.

2 In particular, Wilks heaps scorn on my brief mention of "preference rules" in SS, citing the AI-CL tradition as knowing better. However, as SS states, there is much more extensive discussion of preference rules in *Semantics and Cognition*, and also in my book with Fred Lerdahl, *A Generative Theory of Tonal Music* (Cambridge, MA: The MIT Press, 1983). SS moreover observes (p. 284) that the conceptual indeterminacies expressed by preference rules "play a relatively minor role in the relation between conceptual structure and syntax. . . . This is why the present work, concerned most directly with the syntax-*semantics* correspondence, has not made much reference to formal devices such as preference rules, graded conditions, and 3D model stereotypes."

to analysis of syntactic adjuncts that carry thematic roles, such as the *with* of *cover with a cloth* (which proves not to be an instrumental), four different varieties of *for*, depictive and resultative predication, and the curious construction occurring in *Bill belched his way out of the restaurant*. If one's concerns are at the level of "handcrafting lexical entries," these issues are perhaps not of great importance. But from the point of view of linguistics, these case studies establish important boundary conditions on the expressive capacity of human grammars; they show us the microstructure of language at a higher degree of resolution than was previously possible, a degree that was certainly not attained by the AI-CL approaches of the 1970s.

Now, it is no surprise that the linguistic and computational enterprises should converge on certain basic elements of formalism. Everyone finds it convenient to use capitalized English words to stand for meanings of things they don't yet know how to formalize, e.g., IN, FOR, WITH, MOUTH, CAUSE. Everyone needs a means to express how characters play roles in events; constituent structure notation and function-argument notation are among the most natural ways to represent these relations. The fact that everyone uses these notations tends to mask the differences in goals to which the notations are being applied. However, what distinguishes one theory from another is how one goes beyond these obvious points: when one *can* find a further decomposition of a word or a phrase (as happens frequently in SS, for example with causatives), what is it like? What is the repertoire of constituent types and function types out of which concepts are built? How are these mapped productively into linguistic expressions? If one is a theoretical linguist or psycholinguist, one may further ask: Is this repertoire psychologically and biologically plausible? Does it make learning possible for the child? And so forth.

Alternatively, if one is a computational linguist, the goals may include questions such as: Can this theory be built into an operating computer program? Can it yield a system of automatic concept construction from text? And so forth. There are two possible ways these latter goals might be related to the linguist's. First, the two sets of goals might be orthogonal, yielding different and unrelated solutions, in which case there is not too much point in paying attention to each other's work. Alternatively, it might just be that the best (or only) way to make a computer process and acquire language is essentially to do it the way people do it. In this case it makes sense for computational linguists not to treat the results of linguistic theory as "imaginary procedures" and "fantasy encodings" (to use Wilks's terms), but as a more careful approach to common problems. That doesn't mean linguists are always right—there are plenty of wheels being spun in linguistics, for sure—but the goals, results, and difficulties recognized by linguists maybe ought to get a little more respect. Under such conditions, when computational linguists are testing theories that linguists can regard as linguistically plausible, it is by all means worthwhile as well for linguists to reciprocate.

Ray Jackendoff  
Brandeis University, Waltham, MA 02254-9110

I wish to reply to Vitale's recent article in *CL* 17:3, which describes the fairly well-established method of name pronunciation that is currently used in a number of speech synthesizers including the ones at DEC, Bellcore, and AT&T Bell Laboratories. It is not entirely clear why it would be appropriate to discuss this topic in *Computational Linguistics* given that the method has been fairly well-established for many years.

Nevertheless on page 259, Vitale suggests that the discussion is appropriate because previous researchers have failed to disclose the details of the method:

“It should be stressed that there have been other attempts to implement similar algorithms, although few descriptions of such implementations are available.”

It seems to me that there are plenty of adequate descriptions in the literature including Church (1985, p. 252). I believe that this description is more than adequate for someone skilled in the art as evidenced by the fact that it has since been replicated in several different places, and has been described in at least one review article on speech synthesis (Klatt 1987, p. 773). In fact, the details of the method have been fully disclosed in sufficient detail to satisfy the United States Patent Office (Church 1989). In short, I don't know what more I could have done to disclose the details of my work.

Ken Church  
AT&T Bell Laboratories

#### References

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