

Skeptical Linguistic Essays

Chapter 10 Junk Refereeing: Our Tax Dollars at Work

Recall from Chapter 9, page 10 the remark: “I would also have wished to cover the black art of refereemanship, where even the minimal constraints imposed on scholars by the fact that their names will be publicly seen above their words are absent. Here truth counts for little, and rhetoric holds sway.” This passage, which owed a great deal to Geoffrey K. Pullum, was, I will here argue, a bit prescient, supported by the subsequent appearance of the following successful ¹ anonymous referee subpassage which was part of the most negative report (Overall Rating: Poor) received for my NSF Proposal SBR-9808169 (Diversity Among English Objects). ² To facilitate its analysis and to justify my claim that this is junk refereeing, I have numbered the parts therein by associating a prefixed angle bracketed numeral with each or with major clauses within them.

“<1> In my opinion, the broader impact of this work will be negligible, <2> simply because the whole approach is founded upon assumptions that have not been current in the field for some time now. <3> The whole domain of the data presented here is now considered by 99% of researchers in the field <4> to involve complex relations between (at least) (i) phrase structure configurations (possibly of a quite ornate type), (ii) argument structure configurations and properties (highest/lowest argument, particular thematic relations), and (iii) more detailed semantic properties. <5> This observation about theoretical assumptions should not be taken as mere trendiness; <6> far from it, for the general consensus concerning (i-iii) has not been arrived at by accident, but rather through 30 years of looking at semantics-syntax interactions, <7> and it is in some clear sense “correct”. <8> Additionally, I think this whole body of work has shown a greater depth of insight and explanation than could ever follow from the highly one-dimensional notation-crunching approach presented here, <9> Consequently, the various generalizations that are presented in this proposal, ... strike me as completely uninformative and totally lacking in any insight-<10> they are merely mechanical descriptions.”

Beginning with section <1>, one notes that it consists exclusively of unchallengeable speculation about the future. Such could be offered about any proposal whatever. Does such speculation belong in a putative scientific referee report?

Section <2> asserts, more or less correctly, that the approach of the proposal is founded upon currently non-popular assumptions (roughly, those of relational grammar). This might be relevant to a work on the

sociology of the field. But what is its relevance to an evaluation of the scientific worth of the ideas and a proposal based on them?

Section <3> tells us that 99% of those studying the domain covered by the proposal have certain beliefs. No evidence of polling, sampling, etc. is offered so one is safe in concluding that this remark involves hyperbole using made up numbers unsupported by any actual data. But how would even the truth of the concocted sociological claim bear on the scientific evaluation of the proposal? Is one really to accept the implication of an invariant correlation between correctness and popularity? Is there no chance the putative one per cent are right about any point of disagreement?

Section <4> spells out the putative belief of the putative 99% of workers. One could only wonder if all of them accept all these with the same degree of confidence.

Section <5> reassures us that the observation in section <4>, which seems like an assertion of mere trendiness, is not. This reveals a laudable twinge of guilt rather rare in junk linguistics. It has occurred to the author perhaps that what (s)he has said so far has no actual substantive content, since it contains no specific critique of the logic or factual content of the proposal.

Section <6> reveals why the seeming appeal to mere trendiness is not. It is because the purportedly generally accepted trendy view was not arrived at by accident (whatever that would mean) but rather by thirty years of looking at semantics-syntax interactions. Again the remark fills a gap which should be filled by something of substance. That is, the twinge of guilt has failed to lead to anything of value.

Section <7> makes it final. The trend which took thirty years to develop is 'in some clear sense' *correct*. It would be petty to ask 'what clear sense'. For it is always pretty convincing to be told flatly by an opponent O that O's position is correct.

In section <8>, the referee actually criticizes the proposal being refereed, and in three ways. First it is said to be inferior in depth of insight and explanation to the views supposedly held by 99% of the researchers in the field. But no insight or explanation of anything superior to that in the proposal is cited. So criticism one here is empty. Second, the proposal view is disparaged as being 'highly one-dimensional' and third equally disparaged as involving 'notation-crunching'. Let us suppose that one could assign some content to the notion of being 'highly one dimensional' and even agree further that the proposal has this property. It

would still need to be shown that having said property is undesirable, a task the referee does not attempt. As for ‘notation-crunching’, this also is unexplained and a bit obscure. I infer (but who knows) that the referee is, via a pun on the known form ‘number-crunching’, referring to the fact that the proposal utilizes the standard relational grammar notation for some grammatical relations of numerical signs like ‘1’, ‘2’, etc. Again though, why this choice of notation is worse than another (what other?) is not specified, nor is it addressed why or how a non-substantive choice of notation could be relevant to evaluating a scientific proposal.

Section <9> lets on that the proposal contains generalizations but rejects them as ‘completely uninformative’ and ‘lacking in any insight’.³ These claims are though entirely unsupported. The referee does not discuss the actual generalizations, does not cite any counterexamples to them or any way in which they are insufficiently general. No specific reason is given that they could be stated better in some other terms. The putative criticism is then empty and the identical words could equally be levied against any proposal whatever in any field, even one containing thoroughly established scientific laws. Not even the law of contradiction in logic would be safe from such pseudocriticism.

Finishing with a flourish, section <10> demeans the generalizations of the proposal still further. Here they are said to be ‘merely mechanical’. The chief characteristic of this phrase is that it is undefined. But, curiously, ‘mechanical’ is a word used in formal studies as a positive requirement, e.g. as when one speaks of a ‘mechanical procedure’, an algorithm; see Partee, ter Meulen and Wall (1993: 515-516). Why would the referee think that being mechanical is a *flaw*? The point would seem to be that unwilling or unable to actually discuss and analyze the content of the proposal but nonetheless determined to reject it, the referee is forced to invent pseudoproperties permitting then rejection of the proposal on the ground that it fails to have them, if it is stipulated that they are good, or has them, if it is stipulated that they are bad. That is, starting from a different cast of mind, a referee might have written: “What a wonderful proposal! Its generalizations are mechanical and even reveal *extensive* notation-crunching.”

The reader’s report I have quoted pretends to be an instance of proposal refereeing but is, I have argued, junk refereeing. The referee does not actually analyze the material in the proposal but contents him/herself with such empty and meaningless criticisms as that the approach is founded on ideas which are not currently popular, is one-dimensional (dimensions undefined), involves notation crunching, unsupported assertions that some opposing view is ‘in some clear sense correct’, unanalyzed claims that ‘its generalizations are completely uninformative and totally lacking in any insight’, and finally states that the

descriptions are merely mechanical. It is obviously much easier to write such things than to find genuine counterexamples to the claims or to provide actual alternatives covering the same facts which do not appeal to the notions utilized in the work supposedly being reviewed, substantive tasks this referee scrupulously avoided.

To conclude, the reader's method is not only an instance of junk refereeing, it can be taken as a model of that enterprise. Believe me, esteemed reader, if the need arises, you can adopt this approach and apply it to reject any proposal you wish. It doesn't have to be one which e.g. 99% of the researchers in the field can confidently (though without evidence) be said to already know has got to be wrong.

Notes

1 By 'successful' I mean that it succeeded in its goal of preventing funding of the proposal.

2 Since I tout in these essays the virtue of skepticism, to avoid any suspicion that I am leaving out or distorting parts of the referee report which would undermine the present criticisms, I present the report in full. NFS proposals being public documents, it should be possible for those who move beyond skepticism to real suspicion to obtain a copy of the original from the NSF.

"SBR =9808169

Paul M. Postal "Diversity Among English Objects"

Overall Rating: Poor

I

The intellectual merits of this proposal lie in the fact that it may bring to light new facts about English objects, and may uncover new generalizations about distributions in the data.

In my opinion, the broader impact of this work will be negligible, simply because the whole approach is founded upon assumptions that have not been current in the field for some time now. The whole domain of the data presented here is now considered by 99% of researchers in the field to involve complex relations between (at least) (i) phrase structure configurations (possibly of a quite ornate type), (ii) argument structure configurations and properties (highest/lowest argument, particular thematic relations), and (iii) more detailed semantic properties. This observation about theoretical assumptions should not be taken as mere

trendiness; far from it, for the general consensus concerning (i-iii) has not been arrived at by accident, but rather through 30 years of looking at semantics-syntax interactions, and it is in some clear sense "correct". Additionally, I think this whole body of work has shown a greater depth of insight and explanation than could ever follow from the highly one-dimensional notation-crunching approach presented here. Consequently, the various generalizations that are presented in this proposal, such as (70), (73), (76), (88), (92), strike me as completely uninformative and totally lacking in any insight—they are merely mechanical descriptions. The one putative “argument” in the present proposal for the irrelevance of thematic relations is extremely unconvincing.

In terms of the assumptions that the author makes that strike me as out of touch with current practice, it is stated that GRs are given in terms of phrase structure representations by such statements as “subject is specifier of ” and “object is complement of V”. With regard to objects, the focus of this proposal, the latter statement hardly seems to touch on the wide range of object properties that have been described in the late GB and Minimalist literature, where various structural relations to abstract components of an overt verb-form during the course of a complex derivation have been considered.

Similarly, lexical alternations are considered in the 1982 LFG mode of rules, rather than the underspecification/Lexical Mapping Theory approach which has been current for around 10 years now, in which rules are replaced by underspecification regarding the surface expression of arguments, and where direct connections to thematic properties are part of the explanatory vocabulary.”

3 It is of course not in the slightest relevant here to specify the content of the generalizations contained in the proposal. The present goal is not to defend the proposal. Nonetheless, just so the reader is not left floating totally in the dark, two of the informal generalizations were:

(i) got passives are only possible for 2s.

(ii) only the heads of 2s incorporate

Principle (i) was intended to distinguish e.g. (iva, b, c), given the undifferentiated (iiia, b, c), while principle (ii) was intended to differentiate e.g. (via, b) given the undifferentiated (va, b) :

(iii)a. The servant was sent to her accidentally.

- b. She was sent it accidentally.
- c. It was sent her accidentally.

(iv)a. The servant got sent to her accidentally.

- b. *She got sent it accidentally.
- c. *It got sent her accidentally.

(v)a. That machine answered my question.

- b. Those remarks answered my question.

(vi)a. question-answering machines

- b. *question-answering remarks