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THOUGHTS ON GRODZINSKY'S THEORY OF AGRAMMATISM

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1. Introduction--At Last, Real Neurolinguistics!

The theory of agrammatism presented by Yosef Grodzinsky first in 1984 may be wrong, but in the larger scheme of things it doesn't really matter. The point is that this is one of the few attempts one can cite of neurolinguistic work which uses the linguistic symptoms of aphasia to argue for or against formal linguistic theories. The attempt itself is enlightening and, hopefully, it will encourage others to take the plunge. As I have urged elsewhere, neurolinguistics should be construed as the field which makes the ultimate test of linguistic theory, which in turn is most widely construed as a theory about the knowledge of language which people really have in their heads.

Prior to Grodzinsky's theory of agrammatism, one can not cite many examples of neurolinguistic work in the above sense. In 1972, Whitaker[1] attempted to argue for the lexicalist hypothesis and against the generative semantics view of derived nominals in a paper which I was able to further buttress in my own work[2,3]. Then, in 1977, Mary Kean[4] proposed a controversial analysis of agrammatic symptoms which depended crucially on a particular level of phonological description within generative grammar. She claimed that clitics, or strings which did not have the structure #...#, were missing in agrammatism. While Kean's work did not lead directly to a choice between competing theories, it did supply neurolinguistic evidence that a good part of an entire level of description in one particular theory was on the right track.

Now along comes Grodzinsky[5,6] with the most ambitious neurolinguistic claims so far. Briefly, he claims that the syndrome of agrammatism can best be described using a theory of grammar which employs constructs such as the rule Move Alpha and empty categories, such as traces, as in the so-called "Government and Binding" theory of Noam Chomsky. Agrammatism in English-speaking aphasics consists of "telegraphic" speech which is notably lacking in function words and inflections, though word order is relatively well preserved. In addition, the syndrome may also include difficulty with comprehending certain constructions, such as "reversible" passives, in which AGENT and THEME NPs can be reversed without yielding a semantically anomalous result.

Grodzinsky's point is that the above symptoms can be described with greatest generality as a result of the agrammatic's failure to co-index traces with their source NPs, plus the operation of a "Default" heuristic which is invoked by

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the damaged brain to compensate for the syntactic failure. Take the following sentence, cited in Grodzinsky[6] (p. 147).

(1) John is hit by Bill.

In GB theory, this sentence would have the structure shown below in (2) according to Grodzinsky.

(2) e is hit John by Bill

After movement occurs, this structure becomes (3).

(3) $John_i$ is hit t_i by Bill

Grodzinsky claims that it is the agrammatic's inability to co-index the trace with the moved THEME in sentences like this which accounts for his/her difficulty in comprehending reversible passives. Put another way, the trace which would assign a theta-role of AGENT to the first NP in this passive sentence is "invisible" to the agrammatic in speech comprehension. At the level of speech production, Grodzinsky[6] claims that telegraphic speech in agrammatism is the result of a tendency to delete items which are not specified lexically at S-structure in Chomsky[7].

There is just one further claim which Grodzinsky has to make to cover the speech and comprehension data available for agrammatism. Namely, since the preposition "by" in examples like the above unambiguously assigns a theta-role (from the VP via PP) to its NP, an explanation is required for why agrammatics can not use this information--plus some kind of elimination process--to arrive at the correct theta-roles for a sentence like (1). Grodzinsky's solution is to posit his Default principle which has its origins in performance expectations about the canonical sentence patterns of English.

Basically, he says that when an agrammatic can not assign a theta-role to the initial NP of such a sentence he/she falls back on an expectation for the initial NP to be the AGENT based on English canonical structure. This assumption on the part of the agrammatic patient then conflicts with the assignment of the theta-role of AGENT to the second NP by the preposition "by", and the patient is forced--in Grodzinsky's account--to guess which one is the real AGENT. Hence the random performance shown by agrammatics when faced with comprehension tests such as those discussed in Caramazza and Zurif[8].

The above summary of Grodzinsky's theory of agrammatism necessarily leaves out many interesting points, such as his account of the role of morphological conditions in determining the form of agrammatic speech.

There has been a fairly intense debate over Grodzinsky's theory and the data which it is or should be based on. (Not to mention the debate over what constitutes "agrammatism", which seems to be going in the direction of denying the unitary nature

of this syndrome, and whether comprehension and production deficits always go hand in hand in agrammatism.) My purpose in this short paper is not to rehash those discussions or add to them. Rather, I would like to point out some of the questions that have occurred to me as a result of reading Grodzinsky's work and trying to flesh out his proposals with the performance processes implied by his theories--what Grodzinsky[5] (p. 106) might refer to as the "processing antecedents" of agrammatic behavior.

The general point I will be making is that many of the questions that occur about Grodzinsky's theory are the kinds of questions that have occupied psycholinguists and neurolinguists since the 1960s. These questions are, in fact, the result of the fact that we still know very little about the steps involved in even the simplest mental processing of linguistic behavior. They arise whenever anyone wishes to use verbal behavior as evidence for or against aspects of the formal theory of language.

The fact that Grodzinsky's theory raises such questions is a point in its favor as much as an indication that his theory is still incomplete, possibly even incorrect. Maybe if enough theories force us to think about the "processing antecedents" of language behavior we may eventually learn something about them!

2. "By" by What Right?

As noted above, a key part of Grodzinsky's argument is his claim that the English agrammatic is able to correctly interpret the assignment of a theta-role by the preposition "by" in the passive construction. Actually, this is not so much a claim as a response to data such as those presented in Friederici[9] which seem to so indicate. Grodzinsky[6] (p. 147) says that this may be due to the fact that passive "by" attaches directly to the S node--i. e., is not inside the VP. He also notes in a footnote on the same page that Rizzi[10] claims that this "by" is retained in agrammatism just because it assigns a theta-role. Grodzinsky then states that both his and Rizzi's proposals are at least compatible with the agrammatic data which seem to indicate a sparing of passive "by".

The questions which occur in this account are the following.

Why should attachment at S entail the sparing of a preposition? Alternatively, why should attachment within VP entail impairment? Again, these questions are not criticisms; the dissociative impairment in the two types of prepositions appears to be part of the data. But what is it about this configurational difference that leads to such a profound consequence in the case of agrammatism? Are these claims hiding an implication about agrammatics' inability to deal with a certain level of complexity under each node? An answer to this question is one thing we would need before completely accepting Grodzinsky's theory.

Rizzi's proposal seems more hopeful in this respect. We could imagine that somehow the brain provides greater resistance to damage for prepositions which assign theta-roles on the basis of their importance to linguistic behavior. Still, as Grodzinsky[6] (p. 147) states, "it is hard to find data to distinguish the two alternatives [i. e., his and Rizzi's] at present."

But here another problem may arise. Many authors, such as Obler[11], claim that in its relatively severe state agrammatism is characterized by an almost complete lack of verbs. If this is true--and I am not sure that the data warrant such a conclusion, but it seems likely--then theories like that of Kean mentioned above can not account for it. (Interestingly, Grodzinsky[6] (p. 139) rejects Kean's theory on the grounds that her theory predicts that agrammatics with a comprehension deficit will interpret passives as if they were actives, since all of the clues of the passive morphology would be deleted from the phonological level of their impairment as non-words under her theory.)

In fact, both Kean's and Grodzinsky's theories seem to me unable to predict a lack of verbs at the relatively severe stage of agrammatism. Grodzinsky might want to claim something regarding the susceptibility of verbs to impairment on the basis of their complex role in theta-role assignment, but this would take us back to the question of why Grodzinsky claims that the function of "by" as a theta-role assigner is available to agrammatics--to say nothing of conflicting with the promising idea advanced by Rizzi about why passive "by" is spared in agrammatism!

(An aside: I have done work[2,3] on noun facilitation as a result of brain damage which seems to implicate the complexity of the number of arguments associated with verbs in the fact that they are more easily impaired as a result of brain damage. However, my data in this work were from dyslexics with parietal lobe damage, and the patients concerned did not resemble agrammatics in other ways.)

Of course, Grodzinsky makes the claim that the agrammatic can not arrive at the correct theta-role of the initial NP in an English passive because the trace (left behind after movement) which would assign this theta-role is invisible to him/her since it is lexically unspecified--empty in fact. On the other hand, the data seem to force Grodzinsky to also say that the theta-role assigned by the passive "by" is available to the agrammatic. According to Grodzinsky, this is "because" the passive "by" is lexically specified and still "visible" to the agrammatic "since" it is attached at S and not within VP. (Whew!)

But there are plenty of cases in English where "by" does not assign a theta-role (a point Grodzinsky would acknowledge), and in the passive it can assign a theta-role only because this theta-role has been had from PP which got it from VP--the real

theta-role assigner, if I'm not mistaken. Now the question is, even granted that somehow the passive "by" is undeleted in agrammatic production/comprehension, how is it that patients with a syntactic impairment in the area of theta-role assignment can still follow the complex path by which the VP in a passive sentence dribbles the AGENT role to a following PP, which then hands it off to "by", which then slaps it on its NP--wham, bang, that's all there is to it, folks! To say that all this is possible by the agrammatic patient just because "by" is not deleted in agrammatism because it attaches at S (Grodzinsky) or assigns a theta-role (Rizzi) seems to beg the question...

Maybe the mental processes involved in decoding the theta-role of AGENT in passives are not all that complicated in real language performance--but that's just the point. We don't know what the real processes are.

Grodzinsky wants to say that agrammatics can understand the theta-role assigned by "by" based on the aphasic data ("by" seems retained by agrammatics) and the fact that "by" is a theta-role assigner in the formal theory. However, there is another possibility of prosaic, processing providence. Namely, this "by" may be retained and understood for a reason or reasons other than the possibility that agrammatics are able to grasp its function as a theta-role assigner. An alternative might be that the semantic denotation of "by" (in one of its most common usages) plus a following animate, even human, NP are enough to clue the agrammatic on the function of the NP in question. If agrammatic patients have trouble transferring theta-roles from traces to moved NPs, as Grodzinsky claims, why is it all that much easier to grasp an overt preposition's assignment of a theta-role as described above? In this sense, it might help to test agrammatics for their comprehension of the following sentence.

(4) John was surprised by the book.

In (4), the inanimate nature of the NP in the "by"-phrase might disrupt the interpretation of its function if something other than a strict understanding of theta-role assignment is involved in its apparent comprehensibility by agrammatics.

These questions are closely related to the interaction between the Default principle Grodzinsky proposes and agrammatics' interpretation of "by", since the former conflicts with the theta-role assignment by "by".

3. Default versus "by"

Grodzinsky[6] (p. 145) states that his Default principle follows from non-linguistic, performance considerations, and he also states that in agrammatism the theta-role assigned by passive "by" is available to the patient. The reason he wants to make these two claims is that the conflict that would then arise in the agrammatic's mind about which NP was the real AGENT would

explain the random assignment of this role by agrammatics in comprehension tests with reversible passives and other stimuli. There is a slight catch, though...

Namely, why should a performance heuristic of only statistical force have equal weight in the patient's mind with the theta-role unambiguously assigned (by hypothesis) by the preposition "by"?

This point is worth reflecting on, since it seems to imply or even entail that the patient has "lost confidence" in his/her interpretation of the theta-role assigned by "by". Perhaps only the semantic content of "by" is available to the agrammatic, and the force of its syntactic function is lost--resulting in parity with a heuristic which would merely hint that a first NP is most likely an AGENT.

Friederici[9], though cited by Grodzinsky, actually makes a claim like this. In any case, her paper used German stimuli and subjects. In another paper which Grodzinsky[6] (p. 147) also cites, Friederici, Schoenle and Garrett[12] do use English stimuli and subjects to reach the conclusion that agrammatics retain prepositions in their semantic functions but not in their syntactic ones. Unfortunately, these English stimuli did not include the preposition in question, namely "by".

4. Conclusion--The Ghost of Psycholinguistics Past

Finally, there is one question which, if answered, would contribute greatly to the acceptance or rejection of Grodzinsky's theory of agrammatism. It is the following.

Do normals generate traces in the process of producing or comprehending language?

Grodzinsky, perhaps understandably, does not go into this question or even raise it in his writings on agrammatism that I am familiar with. It is certainly a can of worms to go into, since it is even less likely to have a clear answer than some of the other questions raised above. (In addition, most of the data available in this regard are more related to language acquisition than adult processing.) However, Grodzinsky's theory of agrammatism does clearly entail that the normal comprehension of sentences like reversible passives must involve the indentification of traces.

Now, the competence-performance distinction is another can of worms, but it is a necessary distinction in psycholinguistic research. Unfortunately, this distinction makes it quite possible to believe that traces need not be involved in language performance at all, however necessary they may be in the description of competence.

Here, the historically-minded may note a certain sinister

similarity with a much-discussed psycholinguistic theory of the past--namely, the "derivational theory of complexity". This theory had it that the transformational history of sentences was reflected in their processing complexity and other effects, such as the relation of surface versus underlying clause boundaries on click perception. The debates over the DTC were never really settled in psycholinguistic terms, probably for the reason that there were just too many unknowns in the "theory" of linguistic performance. The debate was simply rendered moot by the radical changes in the formal theory which greatly reduced the content of the transformational component of the grammar. It would probably be self-indulgent to believe that the uncertainty over the DTC in psycholinguistic experiments contributed to these changes in the formal theory, but at least there is the synchronicity to take heart in.

Now the question is, given our current "theory" of performance, is it going to be possible to settle the issues raised above and by others in regard to Grodzinsky's theory of agrammatism? Probably not...

But, then again, 20 years from now we may be able to look back on the "trace theory of agrammatic difficulty" and see it as paralleling changes--advances even--in the formal theory which rendered it moot.

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