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GUIDELINES FOR NEUROLINGUISTICS

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1. Introduction

In earlier papers[1,2], I have discussed both the particular advantages and difficulties of using neurolinguistic (NL) data to test linguistic theories. In this paper, I will return to that discussion--using examples from the recent literature--to show what steps can be taken to maximize the advantages of NL (versus normal, psycholinguistic) data and to avoid the danger of spurious conclusions.

2. Review

As I would like to see them defined, both NL and psycholinguistics use language performance data to test linguistic theories. For example, they both use the relative processing complexity of different types of sentences--as measured, for example, by reaction times, interference tasks etc.--to decide which particular account of syntactic structure might be more plausible. The difference between NL and psycholinguistics is that the former type of data come with some additional information about the functioning of the CNS. In practice, this means that most NL data involve the language performance of brain-damaged persons.

The use of NL and psycholinguistic data to test formal linguistic theories is not a sideshow. Most linguists are serious about viewing their discipline as a branch of cognitive psychology and their theories as hypotheses about actual corresponding mental structures, however hesitant and presumptuous our current efforts in this regard may be. It is necessary, therefore, that we begin to accept the empirical challenge posed by this orientation and actually put linguistic theory to the test of performance, however naive our efforts in this respect must be at this point in our ignorance.

It is in this sense that NL is perhaps even more of an "ultimate" test of linguistic theory than psycholinguistic data for the obvious reason that it involves a more concrete link to the object of linguistic study--that is, the language mind/brain. And with this more explicit link come greater problems. It is no coincidence that psycholinguistic work, which does not involve a concrete discussion of brain structures, is more developed, varied and interesting than NL work: NL data present almost daunting problems of collection and interpretation.

As with psycholinguistic data, NL data necessitate "bridge

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theories"[3] between the observed language performance and the conclusions to be drawn about linguistic theory. That is, to say anything interesting about NL performance data with regards to the formal theory one inevitably ends up talking about "damaged real-time mechanisms", "default heuristics" and so on. It can all get pretty metaphorical.

Added to the above problem, which holds for both NL and psycholinguistics, is the fact that data from aphasia--the principle type of NL data--are both overdetermined and paradoxical[2]. That is, many possible explanations suggest themselves for a given aphasic behavior, while, at the same time, the observed language performance of brain-damaged persons is often internally inconsistent (eg, relatively "simple" items can pose a greater processing difficulty for the aphasic than "complex" ones).

And, yet, the NL temptation refuses to go away and wait until we have a more solid base from which to proceed with this ultimate check of linguistic theory, as might be counseled by more conservative cognitive psychologists. People keep wanting to try and do NL--in the sense of the above definition--perhaps because these naive efforts themselves will help create such a solid base. At least, that is what I would like to think...

Next, I will discuss some recent NL work as an illustration of these problems, and then in a later section I will make suggestions about necessary guidelines for NL which can be drawn from the problematic nature of this work.

3. Recent NL Work Involving Agrammatism

In this section, I will be discussing three different areas of work.

3.1. Grodzinsky's Use of Agrammatic Data to Support Trace Theory

I have discussed Grodzinsky's work[4,5] elsewhere[6] in some detail. Briefly, what Grodzinsky says is that the so-called "agrammatic" speech and comprehension deficits of some aphasic patients can be best described by reference to a loss of the ability to co-index traces (in the sense of Chomsky[7]), and that the agrammatic data therefore provide evidence for the trace theory itself.

The interest of such claims is obvious: Trace theory represents one of the most abstract (and, thus, controversial) attempts to systematize disparate syntactic facts imaginable. If Grodzinsky is correct in his claims, what he has provided in effect is concrete evidence of the linguistic equivalent of something like the early Theory of Relativity!

On the other hand, Grodzinsky's work might be criticized from several points of view. The nature of the problems of the type of aphasic patients he principally relied on has been questioned[8], and it has been claimed that such patients exhibit a wider set of problems than Grodzinsky's interpretation implies.

More importantly, perhaps, is the fact that Grodzinsky must also posit a fall-back strategy by which the patient guides his/her performance in the absence of the ability to co-index traces. Grodzinsky is forced to base this "default" strategy on (totally plausible) assumptions about the canonical sentence structure of English. (Please see the above references for details.)

Finally, and crucially in my opinion, Grodzinsky very nimbly avoids clear statements about such questions as the exact mechanisms damaged in the loss of the patient's ability to co-index traces and why such mechanisms should be susceptible to selective impairment in the first place. In other words, Grodzinsky avoids constructing a "bridge theory" in the above sense. (Such a "bridge" might be based for example on a hierarchy of difficulty implied by the grammatical theory...) This is not necessarily to Grodzinsky's discredit. Silence is understandable given the number of unknowns involved.

However, as I have discussed elsewhere[1], in order to be fully convincing, NL work must ultimately provide such a plausible "bridge", and this must be of such a nature as to maintain a direct connection between the cause(s) of the aphasic performance and the conclusions about the formal theory one wants to draw. (Please see my discussion of Whitaker's[9] use of noun facilitation data to argue for the lexicalist hypothesis in my earlier paper[1] for a more complete discussion.) Performance data which follow predictions somehow implied by the formal theory but which, in fact, have non-linguistic causes are of no use.

3.2. Grodzinsky and Marek vs. Caramazza and Zurif

In a recent salvo not unrelated to Grodzinsky's claims regarding agrammatism, Grodzinsky and Marek[10] have offered a re-reading-cum-methodological critique of Caramazza and Zurif's seminal 1976 paper[11].

Caramazza and Zurif used comprehension performance (as evaluated by a picture-pointing task) on reversible and irreversible passives to conclude the following.

(1) So-called agrammatic patients are (contrary to clinical impressions) as impaired in syntactic comprehension as in production.

(2) The apparent superiority of agrammatic comprehension is due to the fact that such patients rely on knowledge about the world (ie, a passive like "The apple is eaten by the boy" can not be reversed) in lieu of genuine syntactic decoding.

Now, Grodzinsky and Marek do not quarrel with either of these conclusions, which they state seem "intuitively correct". Rather, they take issue with the methodology of Caramazza and Zurif's paper (eg, the type of stimulus sentences and the number and type of distractor pictures) to conclude that, in effect, their influential 1976 paper has no empirical basis.

Interestingly, Caramazza[12] has responded to this somewhat belated critique by accepting--more or less--the methodological critique but by responding that the conclusions Grodzinsky draws in the work reviewed in the sub-section above are no more firmly based than the conclusions he and Zurif drew in 1976, and that--in any case--the real methodological no-no in this type of work is to set up claasifications of aphasic behavior (such as "agrammatism") and then go around fitting observations of aphasic performance into these categories to suit a linguistic agenda.

One might be tempted to ask in the end why Grodzinsky and Marek felt a need to return to Caramazza and Zurif's earlier paper--especially when they do <u>not</u> appear to disagree with the latters' conclusions! I hope to show below that the answer has to do with the lack of a "bridge theory" in Grodzinsky's work as discussed in 3.1. and the empirical status of his own default principle.

But first I will outline one more area of work which will become relevant to what follows.

3.3. Arguments on Arguments in Agrammatism

In a very recent paper[13], Shapiro and Levine have reported the results of an experimental investigation into the effect of strict subcategorizational complexity versus the thematic-role complexity of verbs on sentential processing by (among others) agrammatic patients.

In brief, they found that for simple sentences agrammatics are sensitive to the lexical information about arguments associated with verbs to the same extent as non-brain-damaged persons, and that this sensitivity follows the predicate argument structure (PAS) of the verb (ie, its thematic-role composition) rather than its subcategorizational complexity. (For example, <u>ceteris paribus</u>, alternating datives, which are subcategorized NP NP or NP PP, do not interfere more with sentential processing than non-alternating datives, like "donate", which are subcategorized only for NP PP.)

In my dissertation[14], I discussed the role of number of arguments on normal and aphasic processing complexity primarily in terms of strict subcategorization. That was in 1980. Shapiro and Levine's results are especially interesting in 1990 because of the fact that they seem to dovetail with the recent desire of Chomsky and others (Shigeo Tonoike, personal communication) to do away with sucategorization in the lexicon altogether. In other words, if there is no strict subcategorization in the mental lexicon, it can not influence language performance. As Shapiro and Levine[13] state (p. 23), "The mental lexicon is...one component of the stored linguistic knowledge...the sentence processing system operates on this..."

Shapiro and Levine's conclusions are buttressed by the fact that in earlier work[15] on non-brain-damaged subjects the same results were found, which leads to the following summary in terms of the work described in the preceding sections:

However naive it may sound, Shapiro and Levine's work does include a sort of "bridge theory" for the fact that normal and aphasic processing difficulty is directly proportional to PAS complexity.

Now, this is interesting because, as Shapiro and Levine[13] say (p. 40), "at first glance" their findings support Grodzinsky's claim that access to the lexicon and a verb's thematic information is normal in agrammatics. (Recall that Grodzinsky claims that the agrammatic's difficulties stem from an inability to co-index traces.) However, they make no claims about the actual cause of the difficulty with verbs manifested by agrammatics or how agrammatics' putative loss of ability to co-index traces relates to their findings.

4. Argument Gaps, Data Gaps

Above I have discussed several lines of NL work, because I think that taken together they reveal the typical state of affairs in NL while, at the same time, hinting at possible improvements in research methodology. To summarize the issues raised by these studies, this time in the chronological order of the studies themselves:

First, Caramazza and Zurif's 1976 paper "showed" that "agrammatic" performance was also impaired for comprehension and that their production <u>and</u> comprehension problems implicate a damage to the syntactic component. Apparently, this finding rubbed Grodzinsky and Marek the wrong way, since they have a stake in claiming that a good part of agrammatics' syntactic ability (ie, lexical information regarding the thematic roles assigned by verbs) is intact...

Next, Grodzinsky argues for his analysis of "agrammatism" on the basis of something like a simplicity metric. In a 1989 paper[16], he claims that his analysis is the "less radical" one (p.480). But of course, the "less radical" nature of his analysis--ie, his idea that "only" the agrammatic's ability to interpret traces and not his/her lexical representations is damaged--also gives his theory its NL punch. That is, by saying that the notion trace (or its absence) is sufficient and necessary to characterize agrammatic performance, Grodzinsky thus provides independent motivation for a very important part of current linguistic theory.

The objections to Grodzinsky are many, however, and I have made some of them in my earlier paper[6]. Here, I would like to stress the metatheoretical aspects of the problem: First, as Caramazza[12] argues, the concept of agrammatism itself is far from being an empirical given. And tests/statements concerning agrammatism are likely to confound different clinical pictures. Combining data from such different patients is bound to lead to spurious results/conclusions.

In addition, Grodzinsky provides no hint as to why the ability to manipulate trace is susceptible to selective impairment--even in the most basic sense. This is probably the case because the way trace affects the normal processing of sentences is not known. Another problem with his conclusions, as Caramazza[12] points out, is that Grodzinsky's trace-related analysis of agrammatic comprehension is not very useful for characterizing such patients' production problems--which are, after all, the original basis for the category of "agrammatic Broca's aphasia"!

Finally, the difficulty so-called agrammatics have with the lexical category VERB continues to appear as one of the central issues in all this work. Shapiro and Levine's study is convincing in several respects: it provides a link with normal performance; it provides a metric of performance difficulty based on a, perhaps naively, convincing "bridge theory" (ie, that processing difficulty is directly proportional to the number of items--thematic roles--to be processed); and it manages to imply something that is a real NL conclusion--ie, it provides prima facie evidence for the non-existence of subcategorization frames in the lexicon, which parallels independently motivated arguments in formal syntax (my conclusion).

The bad news about Shapiro and Levine's work is that they show only that agrammatic performance parallels normal performance on simple sentences. (They admit that when more complex sentences are tested differences may emerge.) Their results also leave us with no explanation for agrammatics' difficulty with verbs. As a result of these two points, Shapiro and Levine's work bears an unclear relation to the claims made by Grodzinsky: they may support his "less radical" analysis of agrammatism or they may not: it all remains to be seen.

Summarizing, if "agrammatism" is a coherent category, we have some evidence that its production and comprehension problems are syntactic in nature. Grodzinsky's account of that very syntactic nature is tantalizing but hardly convincing. At the same time, Shapiro and Levine's data could--at best--be taken as partial support for Grodzinsky or--at worst--as a contradiction of even Caramazza and Zurif's tentative conclusions!

This state of affairs is typical of NL at its best and most interesting. Like the projects of NASA, those of NL are just too ambitious/premature/difficult to lead us to expect anything different. However, from our very failures in this regard come guidelines which can help future NL work toward better results.

5. Guidelines for Neurolinguistics

As a result of the above discussion, I would like to suggest that neurolinguists consider the following methodological conditions in conducting NL research.

5.1. Avoid Normal-Aphasic Dissociations Wherever Possible

NL data suggest a plethora of possible causal explanations. As a result, it is easy, all too easy, to leap at this or that characteristic of the formal grammar to explain a given performance. But such explanations are always more convincing if it can be shown that the same factor operates to constrain linguistic performance by non-brain-damaged persons in a parallel way. The desired parallel may come from adult processing or even language acquisition (with a "bridge theory" that the order of language disintegration is--sometimes--the reverse of acquisition).

It is this connection with normal processing that strikes me as weakest in Grodzinsky's theory of agrammatism, which is not at all to say that the role of traces on parsing tasks has not been studied for normal adults or in language acquisition. It has. I am simply unaware of any attempt by Grodzinsky to make an explicit link between this work and his theory of agrammatism.

Ironically, it is just this link which provides the strength of Shapiro and Levine's paper, which has a real NL implication (namely, that perhaps strict subcategorization features in the lexicon are superfluous), although these authors do not mention it!

For the present, it would probably be too strong a constraint to say that all NL theories must establish a link between the causes of normal and aphasic performance complexity, but this is surely a desirable future goal...

5.2. Bridge Theories Must Be Built before They Are Crossed

Another weak point of Grodzinsky's theory of agrammatism is that he provides no hint of why traces might be susceptible to selective impairment. I can respect Grodzinsky's desire to avoid the kind of metaphorical flow-chart thinking that characterizes so much NL discussion, and his theory of agrammatism has value even if only as description. Still, if this missing link could be provided his theory would be more convincing.

Now, it has to be admitted that current "bridge theories" are pretty naive affairs. For example, in my dissertation and in Shapiro and Levine's work the idea is simply that processing complexity increases proportionally with the increase in the number of arguments--somewhat on analogy with tasks like the memorization of lists of random digits of varying length. A further aspect of this "bridge theory" is its assumption that in brain damage more complex abilities are lost before simpler ones--another naive assumption, no doubt, but an unavoidable one for the present, and one often born out by clinical experience.

However, on the plus side, no matter how naive the "bridge", its value as an explicit beginning should not be underestimated. It is from such beginnings that the link with normal processing, for example, can be established. Also, such explicit "bridges" might remove the necessity for ad hoc "default" startegies.

Another adavantage of having an explicit "bridge theory"--no matter how naive--is that this is an essential step in making sure (ie, testing) that the causes of a given aphasic performance are actually relevant to the linguistic conclusions one wants to draw. We do not want to draw elaborate conclusions about morphological complexity, for example, if a visual-field limitation is at the base of the observed linguistic behavior. (Again, please see my earlier paper[1)] for a more complete discussion of this point with respect to noun facilitation.)

5.3. One Is More

Caramazza[17)] has argued very cogently against the tendency to accept aphasic categories in an uncritical way and then draw inferences about a given patient's behavior therefrom, or--equally dangerous--lumping patients with the "same" clinical classification together for experimental purposes. Recall that this was part of Caplan and Futter's[8)] critique of Grodzinsky's theory.

It is hard to imagine how anyone with clinical experience could object to Caramazza's arguments. Still, his insistence on the relative value of one-patient studies may be a hard pill for cognitive scientists trained in statistics to swallow. It is precisely here that the value of the link to normal performance discussed above in 5.1. becomes all-important as a check.

But it seems to me that the danger is less in one-patient studies than in erroneously-assembled group studies. We must assume that everyone's brain and the range of possible damage types must reflect species-wide facts.

5.4. Modality Neutrality to the Max

In the early days of psycholinguistics it was assumed/hypothesized that linguistic competence was neutral for performance modality. That is, the same set of rules were assumed to subserve both production and comprehension for reasons of economy etc. It is useful to remember this assumption when attempting to do NL because purely linguistic effects (= the goal of NL research) in one modality (eg, handwriting) are suspect, though not impossible. While it would be too severe a constraint to demand that all NL conclusions be based on performance data which are paralleled in several modalities, as a guideline such a goal would be highly desirable. One hopes that Grodzinsky, for example, will give us a more explicit statement of how the inability to co-index traces which he hypothesizes for agrammatic comprehension actually functions in agrammatic production...

5.5. Last, Not Least

Finally, the very goal of NL can be a stumbling block. The more-or-less hidden agenda of neurolinguistics operates to preselect data with all the opportunities for bias that that implies. One antidote is simple but time-consuming: a thorough consideration of the battery of tests used by speech pathologists. But there is a short cut:

I always try to imagine that a given set of data which has linguistic implications (say, in favor of the lexicalist hypothesis) is actually evidence for the very opposite (in this case, generative semantics). I have found that, such is the depth of our ignorance about the relation between performance and brain, that almost any set of data can at least temporarily be viewed as evidence for two opposing conclusions. Such was the case with noun facilitation, as I tried to show in an earlier paper[1]. If the evidence is solid, however, this Necker-cube-like effect will disappear, and in the process the points discussed above in 5.1.-4. will have been clarified.

6. Conclusion

There are probably plenty of other, equally necessary guidelines which could help constrain NL theorizing.

My point in this short paper has not been to emphasize the difficulty of NL as a field, however, but rather to illustrate the very complexity which makes the field interesting. And makes it the logical endpoint of all linguistic research.

In a future paper I hope to discuss in greater detail examples of the practical application of these five guidelines.

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References

1) Bisazza, J. A. On the value of neurolinguistic data, Annual Bulletin, Research Institute of Logopedics and Phoniatrics

(Faculty of Medicine, University of Tokyo), 19:227-48, 1985.
2) Bisazza, J. A. The problematic nature of neurolinguistic data, Annual Bulletin, Research Institute of Logopedics and Phoniatrics (Faculty of Medicine, University of Tokyo), 20:141-60, 1986.
3) Botha, R. P. The Conduct of Linguistic Inquiry. The Hague: Mouton, 1980.
4) Grodzinsky, Y. The syntactic characterization of agrammatism, Cognition, 16:99-120, 1984.
5) Grodzinsky, Y. Language deficits and the theory of syntax, Brain and Language, 27:135-59, 1986.
6) Bisazza, J. A. Thoughts on Grodzinsky's theory of agrammatism, Annual Bulletin, Research Institute of Logopedics and Phoniatrics (Faculty of Medicine, University of Tokyo), 22:133-40, 1988.
7) Chomsky, N. Lectures on Government and Binding. Dordrecht, Holland: Foris, 1981.
8) Caplan, D. and Futter, C. Assignment of thematic roles to nouns in sentence comprehension by an agrammatic patient, Brain and Language, 27:117-33, 1986.
9) Whitaker, H. A. Unsolicited nominalizations by aphasics: The plausibility of the lexicalist hypothesis, Linguistics, 78:62-71, 1972.
10) Grodzinsky, Y. and Marek, A. Algorithmic and heuristic processes revisited, Brain and Language, 33:216-225, 1988.
11) Caramazza, A. and Zurif, E. B. Dissociations of algorithmic and heuristic processes in language comprehension: Evidence from aphasia, Brain and Language, 3:572-82, 1976.
12) Caramazza, A. When is enough, enough? A comment on Grodzinsky and Marek's "Algorithmic and heuristic processes revisited", Brain and Language, 33:390-9, 1988.
13) Shapiro, L. P. and Levine, A. B. Verb processing during sentence comprehension in aphasia, Brain and Language, 38:21-47, 1990.
14) Bisazza, J. A. The Processing Complexity of Nouns and Verbs: Psycholinguistic and Neurolinguistic Issues. Ann Arbor: University Microfilms, 1980.
15) Shapiro, L. P., Zurif, E. and Grimshaw, J. Verb processing during sentence comprehension: Contextual impenetrability, Journal of Psycholinguistic Research, 18:223-43, 1989.
16) Grodzinsky, Y. Agrammatic comprehension of relative clauses, Brain and Language, 37:480-99, 1989.
17) Caramazza, A. On drawing inferences about the structure of normal cognitive systems from the analysis of patterns of impaired performance: The case for single-patient studies, Brain and Cognition, 5:41-66, 1986.