

A final brief in the case against agrammatism: The role of theory in the selection of data*

WILLIAM BADECKER
ALFONSO CARAMAZZA

The Johns Hopkins University

In an analysis of the assumptions behind the use of patient groupings in neurolinguistic and cognitive neuropsychological research (Badecker & Caramazza, 1985), we have argued that clinical categories in general, and agrammatism in particular, allow no valid means for reasoning from patterns of impaired performance to an understanding of the processing mechanisms underlying normal linguistic performance. On the basis of these arguments, we proposed that categories of this sort serve no useful function and ought to be dispensed with in this research. In his response to our paper, Caplan (1986) claims that there are indeed reasons for maintaining agrammatism as an object of study, and he suggests that “one can approach agrammatism in a way that renders it an appropriate category for aphasia, and [this conclusion] leads to a number of interesting research topics regarding this category” (p. 263). We maintain that this can only be true if the goals of Caplan’s enterprise are radically different from what we take to be those of modern cognitive neuropsychology. We hope to make this clear in the space of a few pages.

1. The goals of the enterprise

In order to understand our position regarding clinical categories such as agrammatism it is essential that one have a clear understanding of the goals of our research. (We assume these goals to be standard.) Simply put, our interest is in using patterns of impaired performance to constrain theories of language processing. In order to put aphasic performance to this use, we must of course be willing to maintain certain assumptions; for example, that language processing mechanisms can be affected selectively by brain damage, and that the impaired system cannot exploit mechanisms or organization of mechanisms which did not exist prior to the disruption that caused the aphasia

* This research was supported by NIH grants NS14099 and NS07402 to The Johns Hopkins University. Reprint requests should be addressed to: William Badecker or Alfonso Caramazza, Department of Psychology, The Johns Hopkins University, Baltimore, MD 21218, U.S.A.

(Caramazza, 1984). What we showed in Badecker and Caramazza (1985) is that using information from deficits to inform us about the nature of the normal language processing mechanisms also means that we can use data from aphasia in some ways, but not in others. In particular, we explained why one cannot validly employ groupings of subjects established by clinical intuition in order to test one's models of language processing. One cannot assume, for example, that arbitrary similarities in performance on a particular set of tasks (i.e., those used for clinical classification) will ensure that the group so determined will be homogeneous in terms of the intact cognitive system that subserves language processing. However, it is just this sort of homogeneity that is required if the leveling of variation within the clinical group is not to wipe out differences that are potentially significant with regard to the evaluation of a proposed processing model. Thus, the methodological considerations engendered by the goals of our research prohibit the use of clinical categories such as agrammatism in any nontrivial way (see also Caramazza, 1986).

Our reading of Caplan's paper reveals that the goals of the enterprise he proposes are quite distinct from our own. For Caplan, the goal of such research (as we understand it from his remarks) is to take some category of subjects and to develop a theory about what it means to belong to that category. Caplan appears to suggest that only those categories that are amenable to particular sorts of linguistic analyses will be appropriate for this type of research, but that is a separate issue. What he is advocating amounts to the description of the clinical category of agrammatism as an end in itself. In our discussion of the uses of clinical groups, we suggested that description for its own sake is the only sort of research that can take agrammatism as a valid category of analysis. Indeed, this is true for any arbitrary grouping of aphasics. Nevertheless, we must hasten to add that the results of such research can have no bearing on the enterprise that we ourselves have described. That is, theories concerning the deficits commonly afflicting the members of clinical groups are irrelevant to the task of formulating and/or testing theories of normal language processing. For this reason, our proposal to dispense with agrammatism, and other clinical categories, from neurolinguistic and cognitive neuropsychological research has not in any way been challenged by Caplan's defense.

2. On the description of clinical categories of aphasia

Having appraised Caplan's interests as different from our own, we should also note that many of his arguments concerning agrammatism are flawed

even within the context of a framework specified by the narrower goals of description. In the remainder of the brief space allotted to us, we will attempt to address issues relating to the putative explanatory force of such descriptions, and to provide some indication as to why the lack of such force renders the proposed descriptive activity of limited appeal and utility.

2.1

Caplan has suggested that the clinical category he proposes to analyze is somehow immune to the charge that it has been arbitrarily gerrymandered, or that the abstraction that lies at the core of theories about this category is a fiction. Apparently this immunity derives from the fact that the focal aspect of the clinical category, the omission of inflections and function words, can be described in formal linguistic terms. In fact, Caplan refers to the work of researchers who have proposed about three or four distinct linguistic accounts, a situation that is something of an embarrassment of riches in this context. If a tenable model of language processing preserved independently each of the distinctions that these authors posit, then it might in fact be reasonable to expect that there could be at least as many functionally distinct deficits affecting roughly the same lexical items (though not necessarily in the same way or with the same consequences for language processing in general). This, of course, undermines Caplan's claims regarding the homogeneity of the clinical category. However, even if there were only one linguistic characterization of a subset of the items that are omitted or substituted in the speech of so-called agrammatic aphasics, this would not suffice, in and of itself, to justify attributing the omission or substitution of just these items to the same deficit in each case. A unique formal linguistic category subsuming both inflections and function words would not, by its existence, entail that only one processing component distinguishes these items from all others. Nor, for that matter, can we justify distinguishing the difficulty a subject may have with items of this category from a difficulty with verbs simply because the linguistic theories we employ do not treat the union of these items as a grammatical class. As Berwick and Weinberg (1984) have pointed out, the relationship between the right linguistic theory and the right model of language processing need not be that of type transparency (i.e., where each and only the categories and/or rules of the one are mirrored by categories and/or rules of the other). The computational mechanisms of the language production system may or may not make such a distinction, but it is not an issue that can be decided by stipulation. The arbitrary borders of the clinical category of agrammatism are obscured as such when stipulations of this sort remain unrecognized.

2.2

Caplan suggests that an account of the clinical category of agrammatism in terms of Garrett's (1976, 1982) model of sentence production goes beyond simple description in that it permits one to make predictions about the relationship between the omission of vocabulary items and the syntactic complexity of sentences. The gist of this account is that "the processing stage affected in agrammatism thus appears to be Garrett's 'positional level' " (p. 265). Of this theory, Caplan writes that

it does predict that there *could be* [italics added] a close relationship between a disturbance of the function word/inflectional vocabulary which arises at or affects this 'positional' level of representation of sentence form and the use of normal syntactic structures. On the other hand, a patient with such a disorder should not have trouble assigning basic sentential semantic features ... to lexical items,

There is considerable evidence that this prediction is borne out by the data. Agrammatics simplify syntactic structure above and beyond what would obligatorily result from ill-formedness due to omission of selected vocabulary elements [references omitted]. Intra-clausal structures are affected as well as the formation of sentences containing more than one clause, though the latter aspects of sentence structure (verbal complementation, relative clause formation) are more severely affected.

We argue that this is in fact circular. The data does not support this analysis because the data has been selected just to respect this association (in the same way that the omission of verbs has been arbitrarily excluded from the 'data'). Note, for example, that the simplification of sentence structure is not a necessary consequence of the omission of function words, as attested by a case study presented in Miceli, Mazzuchi, Menn and Goodglass (1983). Case 2 from that report, T.F., omitted function words in a large percentage of obligatory contexts: determiners (72%), auxiliaries (73%), preposition + determiners (49%), and clitic pronouns (100%). However, there was no evidence that the syntactic structure of his utterances was otherwise affected by these omissions (Miceli et al., 1983, p. 74):

overall his sample of 600 words consists of some 30 well-formed compound and complex sentences; he uses only three simple sentences in his entire narrative.

Of course, Caplan does not claim that there *must be*, but only that there *could be* a relation between 'agrammatic omissions' and 'sentence simplification' according to the positional level deficit account of agrammatism. Nevertheless, we wonder what the utility of such 'predictions' could be, or even whether it is appropriate to refer to this sort of association as a 'predic-

tion' of the theory. The positional level representations in Garrett's model include much more than just information about function words and inflections, yet Caplan's analysis provides no guidance as to what the necessary consequences of a deficit affecting one aspect of these representations are for the other features of the representations. This situation might in fact be corrected by additional stipulations concerning the extension of the clinical deficit; but this wouldn't change the fact that the present use of Garrett's model amounts to a convenient means for description, and not a tool for explanation.

2.3

Caplan concludes his response to our paper with the complaint that we "offer no descriptive framework or explanation of the phenomena of agrammatism at all" (p. 274). This 'failure', though, was no accident: in as much as the goals of modern neurolinguistic and cognitive neuropsychological research preclude the use of this clinical category as a starting point of analysis, there can be no legitimate framework of the sort Caplan has called for in the context of this enterprise. We simply did not address the narrower goals of mere description for its own sake. In terms of these goals, though, we simply note that it does not seem reasonable to us to expect that a description of agrammatism in terms derived from psycholinguistics, cognitive neuropsychology, or generative grammar should be any more appropriate or helpful than a description couched in the vocabulary of traditional grammar. Agrammatism has no special status in any of these frameworks, so choosing any one of these over the others is not a matter to be settled by theory. Like agrammatism itself, a framework for describing the clinical category would probably be best selected on the basis of intuitive appeal. Unlike the case of formulating a model of linguistic competence or performance, where constraining the descriptive apparatus of the theory would be a virtue, there is no obvious point to imposing such constraints on the description of this clinical syndrome.

We conclude this portion of our discussion with one point of clarification: the task of describing clinical categories such as agrammatism is entirely distinct from that of explaining individual cases of aphasic deficit in terms of the language processing mechanisms that have been impaired and those that have been left intact. In Badecker and Caramazza (1985) we argued that the latter sort of account was in fact the only truly explanatory approach to acquired language deficits. Unlike the analysis of clinical syndromes, which cannot guarantee the necessary homogeneity of the study group in terms of cognitive impairments, the single-case methodology does not admit of these criticisms. The single-subject is, by definition, homogeneous in this regard.

(For related arguments concerning single-case studies of aphasia, see Caramazza, 1986.)

3. Summary

The utility of an abstraction derives from the sorts of insights that can be validly derived from it. In the case of abstractions like agrammatism (and other clinical categories of aphasia), we have cited several considerations concerning why they do not allow one to reason from impaired performance to an understanding of normal language processing abilities. Nor are these categories of any help in understanding individual cases of deficit: the clinical category may include variation which, when averaged out in analyses of the performance of the group, only leaves a residue of patterns of impaired performance that are not amenable to an explanation in terms of the normal language system. There is no way to settle this point prior to the development of adequate models of language processing (models that are capable of accounting for the individual cases), so there is no real point to holding on to the clinical categories for this purpose either. Using the clinical category as a starting point for such analyses simply amounts to prejudging the issue. Of course, if the clinical category is not a valid tool for explaining individual cases of aphasia, then it is difficult to see just what role these categories may serve. Thus, we continue to view categories like agrammatism as impediments to useful analysis in neurolinguistics and cognitive neuropsychology, and reaffirm our position as to their place in this research: they have none.

References

- Badecker, W., & A. Caramazza (1985). On considerations of method and theory governing the use of clinical categories in neurolinguistics and cognitive neuropsychology: The case against agrammatism. *Cognition*, 20, 97–125.
- Berwick, R., & A. Weinberg (1984). *The grammatical basis of linguistic performance*. Cambridge, MA: MIT Press.
- Caplan, D. (1986). In defense of agrammatism. *Cognition*, 24, 263–276, this issue.
- Caramazza, A. (1984). The logic of neuropsychological research and the problem of patient classification in aphasia. *Brain and Language*, 21, 9–20.
- Caramazza, A. (1986). 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.
- Garrett, M. (1976). Syntactic processes in sentence production. In B. Butterworth (Ed.), *Language Production* (vol. 1). London: Academic Press.
- Garrett, M. (1982). Production of speech: Observations from normal and pathological language use. In A. Ellis (Ed.), *Normality and pathology in cognitive functions*. London: Academic Press.
- Miceli, G., A. Mazzuchi, L. Menn, & H. Goodglass (1983). Contrasting cases of Italian agrammatic aphasia without comprehension disorder. *Brain and Language*, 19, 65–97.