On considerations of method and theory governing
the use of clinical categories in neurolinguistics
and cognitive neuropsychology:
The case against agrammatism*

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Abstract

The pervasive use of clinical categories of aphasia in neurolinguistic and cog-
nitive neuropsychological research reflects the assumption that these patient
groupings represent disruptions of the normal language-processing system
along theoretically significant lines. This premise is examined here with particu-
lar reference to the status of 'agrammatism'. It is argued that there are compel-
ling reasons to question the coherence of agrammatism as a psychological
entity. To overcome these objections, the clinical intuitions on which this
aphasic category is based must be replaced by objective criteria for selecting a
theoretically significant patient grouping. To reach this goal, it is especially
important that a theoretically motivated distinction be made between within-
and across-category variation. It is argued that in the case of categories like
agrammatism, there are serious methodological obstacles which make such
goals unreachable. It is argued, therefore, that our theories should not take
categories like agrammatism as psychological givens, especially if the purpose
of our research is to reach an understanding of the mechanisms of language
processing or of individual aphasic deficits themselves. Furthermore, it is ar-
gued that the single case methodology for the study of aphasia can address
these goals by taking patterns of performance on particular linguistic tasks as
basic units of analysis, and that this approach avoids the methodological pitfalls
faced by studies which take clinical categories as starting points.

*This research was supported by NIH grants NS14099 and NS07402 to The Johns Hopkins University. We
would like to thank a number of people for favoring us with their helpful criticisms and comments at various
stages of our research. These include Howard Egeth, Roberta Goodman, Gary Hatfield, Paul Hofer, Steven
Lapointe, Michael McCloskey, John Marshall, and especially Domenico Parisi for discussions which provided
much of the impetus for writing this paper. Requests for reprints should be addressed to William Badecker or
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Introduction

It is commonly held by neurolinguists and cognitive neuropsychologists that the analysis of impaired performance that is consequent to brain damage plays a crucial role in the study of normal cognition, as well as in research to understand the nature of cognitive disorders themselves. However, an assumption that must be made sense of in order for this claim to be considered truly tenable is that, just as in the case of normal performance, impaired performance stands in a direct relationship to the normal language-processing system. How can such an assumption be justified? In principle, the relationship between impaired performance and the normal cognitive system must be specifiable in terms of the operation of the normal system under particular conditions, in the same way that normal performance on an experimental task must be explicable in terms of the functioning of the processing mechanisms under those conditions imposed by the experiment. In both cases the available modes of explication must be the same; the only difference between the avenues of inference from impaired or unimpaired performance to the normal processing system will lie in the types of conditions imposed on the processing system. For example, a typical psycholinguistic experiment might require a subject to read sentences and to provide some indication of successful processing while simultaneously performing some secondary task that is designed to compete for particular processing resources. In order to interpret the data from such an experiment, one must hypothesize what components are involved in performing the primary task (i.e., what mechanisms are employed, what memory resources are required, and so forth) and how the functioning of this system produces the observed patterns of performance when the processing demands on this system are affected by the secondary task. Similarly, the performance of a brain-damaged individual on a linguistic task must be understood as the product of the same normal processing system (i.e., the same mechanisms, the same memory resources, etc.) under conditions defined in terms of a hypothesized locus (or loci) of impairment to this system. That is, the logic of the situation requires that patterns of impaired performance be explained in terms of a (computationally explicit) processing system under specified conditions, and in this case, the “specified conditions” consist of modifications of a restricted sort to the proposed cognitive system (Caramazza, 1984; in press). The constraint that this variety of account imposes on the construction of cognitive theories is basically that candidate models of normal processing must be capable of producing various observed

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1We refer here to a functionally defined locus (or loci), as opposed to an anatomical site.
patterns of impaired performance when subjected to particular modifications (of the restricted sort).

These issues aside, a decision is yet to be made concerning the legitimate means for selecting evidence for or against one cognitive model or another. That is, within this framework one must still determine the meaningful groupings of data to submit to analysis. The typical choices in neurolinguistics are those patterns of performance identified with the clinical categories of cognitive deficit such as agrammatism, Wernicke's aphasia and the like. But just what is the status of such categories of impairment? Supposedly, these categories represent fractionations of the processing system along theoretically significant lines: yet they come to us, not out of any well developed theory, but as groupings based on clinical intuition. This fact is not without methodological consequences. The grouping of data which follows from patient classification influences which aspects of the data will be treated as a consequence of the 'type' of deficit, and which will be taken to represent insignificant variation among tokens of the deficit. Importantly, the difference between what is 'significant' and 'insignificant' in this context is intended to be definable in terms of the 'correct' cognitive linguistic theory. So, when a specific performance pattern is indentified as constituting a privileged reflection of the deficit category, it is just this evaluation that will (partially) delimit the set of processing accounts that are consistent with the patient grouping. Can the clinical categories, as we conceive them, support this methodological burden?

Quite typically, data that is grouped along the lines of these clinical categories is used as evidence for or against particular accounts of language processing. But while the willingness to reason this way reflects a general confidence researchers have in the correctness of these categories, the patient groupings are nonetheless pretheoretical constructs. Ultimately, though, their use in testing models of performance demands that these categories be given an explicit articulation and explication within a theoretical framework. Thus, even if we were to grant that an individual pretheoretical category, because of its intuitively interesting character, could be of use in furthering our understanding of cognitive systems, eventually we must repay the debt we incur by adopting this position. The category must be shown to be more than an arbitrary grouping: one must demonstrate that it leads to significant

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2It may, in fact, be more accurate historically to say that these categories are constructs of (incorrect) nineteenth century theories of language (Arbib, Caplan and Marshall, 1982; Marshall, 1982); but by the same token, these are not theories that current practitioners of neurolinguistic research would advocate explicitly. Hence, we will continue to refer to the categories as pretheoretical constructs in order to emphasize the covertness of the theories from which they ultimately originate, as well as the status they are accorded as virtually a priori starting points for neurolinguistic studies.
generalizations regarding the normal processing system. Furthermore, the debt cannot be ignored since an error on this point might allow characterizations of performance based on these groupings to obscure the true nature of individual deficits and/or to provide illusory support for poorly conceived models of processing. For these reasons, researchers should question the utility of studying categories of deficit such as, for example, agrammatism, deep dyslexia or phonological dysgraphia in light of the following issues. What is the basis for identifying the various categories as particularly salient or theoretically significant? Do the means by which these particular categories are individuated provide us with any reason to suppose that they are proper (i.e., theoretically significant) groupings? Can we in fact show that they are not arbitrary groupings? In this paper we will direct our attention to agrammatism, but the lessons we draw from this case will have clear relevance for the other neuropsychological and neurolinguistic categories as well.

Over the past few years, numerous studies have been directed toward answering the questions: "How is agrammatism to be explained? What is the essence of this (symptom) syndrome?" However, for all the disagreement to be found in this literature, and despite the fact there is not an objective set of criteria that may be said to define just those non-normal linguistic features that must be present in an individual’s performance in order to characterize his or her speech as agrammatic, there is one point that appears to have nearly universal support. The belief that the term 'agrammatism' does indeed individuate a natural category of performance is held tenaciously by nearly everyone who has addressed him or herself to the explanation of agrammatism. (This is even so in the case of research with the stated goal of determining the functional components of normal linguistic processing.) What justification is there for granting this special power of individuation to the clinical impression on the basis of which agrammatism is taken to exist? The assumption that the category so defined is something other than a reified impression is firmly entrenched in neurolinguistic research, so it is well worth

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3 Agrammatism is generally identified with abnormal speech production involving the omission of function words (e.g., the auxiliary verbs do, have, and be; determiners like the and a(n); and prepositions such as of, for and into) and certain bound morphemes (such as inflectional endings—e.g., plural -s, the past tense and participial morphemes -ed and -en, and comparative -er). In addition to these characteristics, which Caplan (1985) describes as the cardinal manifestations of agrammatism, reduced phrasal length and complexity (Goodglass, 1976), the omission or "nominalization" of main verbs (Saffran, Schwartz and Marin, 1980; Caramazza and Berndt, 1985) and the failure to use word order properly to indicate intrasentential semantic relations (Saffran et al., 1980) have also been identified as features of agrammatism. In addition, several studies (e.g., Caplan, 1985; Grodzinsky, Swinney and Zurif, 1985) suggest that certain receptive disorders (specifically, asyntactic comprehension) should be considered as a (possibly necessary) characteristic of agrammatism. In this paper we will concentrate on the more conservative, production-oriented interpretation of agrammatism.
considering its consequences. In this paper, we question the wisdom of this assumption and argue, instead, that if the goal of this research is to identify and describe the mechanisms underlying linguistic performance, then categories such as agrammatism are a theoretical hindrance which ought to be dispensed with. And, lest we be taken as expressing a view of agrammatism that is held only with respect to the project of developing an understanding of the systems underlying normal processing, we state at the outset that we believe our arguments to be validly directed also toward that brand of study which purports to examine agrammatism of and by itself. Doing away with agrammatism as a pretheoretic concept will, at the very least, have the salutary effect of dislodging the covert theoretical claims which follow from the reification of this category; but we will argue that the implications of this move include some rather specific and positive methodological consequences as well. Our purpose will be to show that, properly construed, the move to eliminate agrammatism as an aphasic category constitutes, not anarchy, but the next logical step toward the stated goals of cognitive neuropsychology and neurolinguistics.

The paper is organized as follows. We begin by examining the motivation for positing agrammatism as an aphasic category, along with certain very basic questions about this category. For example, what sort of category is it? If we wish to study agrammatism, what (or even who) do we study? How can one go about objectifying the notion of agrammatism? We explore in detail two competing solutions to the latter problem—the ‘definitional’ and ‘empirical’ approaches—and we identify the problems with each of them. We point out that efforts to provide an empirically motivated delimitation of agrammatism must deal squarely with two basic requirements. We need theoretically driven criteria for selecting instances of the category, and we need some principle(s) on the basis of which we could decide what is to count as a consequence of being an instance of agrammatism. We argue that the logic of the situation renders these problems equally intractable.

Next, we examine agrammatism on a less abstract plane. We discuss examples of so-called agrammatic speech that have appeared in the literature and argue that the linguistic variation that can be observed in these examples reveals the heterogeneity of the category (hence its theoretical arbitrariness). Then, we provide a more explicit characterization of the notion of psychological syndrome, and of how agrammatism can be understood in terms of this characterization. This launches our discussion into the problems faced by any research which takes categories like agrammatism as a starting point. The pervasiveness of the methodological arbitrariness of such studies is argued exhaustively.

The paper concludes with a brief discussion of an alternative method for
using neuropsychological data for the understanding of normal cognition, and this alternative is discussed specifically in terms of its success in avoiding the methodological pitfalls that were detailed in previous sections.

1. Approaches to agrammatism

Consider the origins of agrammatism: Why is agrammatism posited as a category? Where does the assumption that agrammatism exists come from? As we have already said, the primary motivation for this patient grouping, and for the category of deficit identified with this grouping, is clinical impression; but there is more to the story than this. Based on certain tacit notions of how linguistic capacities are functionally organized, the intuition that there are commonalities among members of this group has been transformed into the stance that these commonalities are theoretically significant. Why should we accept this clinical impression as meaningful, though? Few attempts have been made to justify treating agrammatism as more than a reified grouping, and we would argue that the reason for this is that the linguistic notions which underpin the clinical grouping are vague and insufficiently tied to any tenable psychological theory of language and language processing. Thus, they constitute dubious candidates for providing an empirical foundation for this category as an ultimate theoretical entity. Thus, it is unclear from the start as to what is entailed by membership in this category, or whether there is any informational content to claims that agrammatism is a coherent category at the psychological levels of analysis we take ourselves to be addressing. Despite these difficulties, though, the legitimacy of this aphasic category has seldom been attacked. With this in mind, we turn to the central questions of this paper: What sort of a category is agrammatism? Does this category have a specifiable core? When we encounter variation in the patterns of performance of patients classified in this way, how do we distinguish significant variation from noise? Answers to the latter, admittedly more general question are particularly relevant to determining how we can frame answers to the preceding ones since they will in part determine whether it is possible to provide objective evidence for any hypothesis concerning agrammatism, including its putative existence as a psychological entity.

We don't know of any authors who have specifically advocated dispensing with agrammatism as a category, but this would appear to be a consequence of the holist position (e.g., Schuell, Jenkins and Jaminez-Pabon, 1964), whose advocates argued that aphasia is one-dimensional and can be properly classified in terms of severity of aphasia and whether it co-occurs with other deficits. Clearly, such objections to the specific category of agrammatism are much different from our own.
To begin, it would be useful to consider how these issues arise in the context of the following questions: Can we enlist any available principles to objectify the notion of agrammatism (i.e., principles to supersede clinical intuition entirely in the identification of instances of agrammatism)? Is it possible to do so? There are, we believe, two conceivable approaches to this problem, though, upon close inspection we find neither tenable. The first, which is identified with the symptom-complex conception of agrammatism, is to treat the omission of function words (e.g., articles, auxiliary verbs) and inflectional morphemes (e.g., tense markers) both as the symptom in terms of which agrammatism is loosely identified and as the central item in the constellation of symptoms which make up this syndrome (performance deficit). With this as a starting point, we might then apply empirical means, such as the logic of dissociation, to discover some invariant pattern in the hodge-podge of symptoms; thereby we may replace the clinical intuition with the requisite means of objectification. We will refer to this as the empirical approach. Alternatively, we can simply define agrammatism as the omission of these closed-class words and inflections, so that regardless of the individual patterns of deficit that accompany this symptom, the omission of these items must be taken to indicate agrammatism. Apart from the issue of whether this latter means for objectifying agrammatism comports with our conception of the category, this definitional approach invites other obvious objections. Nevertheless, it should be clear that, if data from agrammatism is going to provide an objective means for studying the mechanisms underlying language, and if we continue to take the theoretical significance of this category as guaranteed, then one or the other of these approaches to displacing clinical subjectivity must be employed. The investigation of cognitive mechanisms cannot proceed from a subjectively delimited starting point that, by its nature, defies objectification. Nor, for that matter, could we simply content ourselves with the thought that, by restricting our goals to “understanding the syndrome agrammatism of and by itself,” the problem of objectively specifying this object of study can be circumvented. If it isn’t possible to provide such a specification, if agrammatism is a taxonomic fiction, then certain common research questions, such as “does agrammatism entail asyntactic comprehension,” are meaningless (Caramazza, in press).

5 That is, even when we observe these omissions in the speech of a “fluent aphasic”, the aphasia would be characterized as having an agrammatic component.
1.1. The definitional approach

Consider first the definitional approach to agrammatism. What are the consequences of strictly identifying the omission of function words and inflections as agrammatism? The most obvious consequence of this definitional approach is that the existence of agrammatism, as a syndrome, would not be an empirical matter. We could not provide evidence for a psychologically objective category—i.e., one whose existence is owed to the structure of the underlying cognitive mechanisms—because any instance of this symptom would, by definition, be an indication of the syndrome. The claim that this symptom is, in principle, dissociable from other aspects of a patient’s performance could not be disproven, and, as such, the judgement would be vacuous. Note that we do acknowledge that distinctions in vocabulary type are important to the characterization of aphasic performance. If a patient omits main verbs 30% of the time and function words 60% of the time, then these are features of performance that we need to deal with, though not necessarily in these exact terms. What we don’t want is for the distinctions we make just for classifying this pattern of performance to provide the sole basis for identifying one aspect of this pattern as having a theoretical prominence. By assigning one such aspect of the pattern the status of a psychological category with immutable boundaries, we simply establish the existence of this aphasic syndrome by fiat. It is not a reasonable expectation that this reified category of performance would be of use in testing our theories of language processing since the measure on which it is based could not be guaranteed to conform to the divisions established by the cognitive system underlying this processing.

Although this definitional approach is seldom advocated outright, in practice it is not uncommon for aphasia research to focus on an idealized data base whereby agrammatism qua the omission of function words and inflections has emerged as a reified entity. For instance, “examples” like (1a)–(3a) are purported to be agrammatic utterances corresponding to intended sentences like (1b)–(3b).

(1) a. Rolly Fingers...Sparky Lyle different impression pitch(ing) strength. (Kean, 1981)
   b. Rolly Fingers and Sparky Lyle have different impressions of their pitching strengths.

(2) a. Sparrow nest pine tree. (Kean, 1981; Lightfoot, 1982)
   b. The sparrow nested in the pine tree.

(3) a. Boy look cat. (Grodzinsky, Swinney, and Zurif, 1985)
   b. The boy looked for the cat.
In all of the published transcripts of so-called agrammatics that we have examined, and in our own records of subjects whose speech would (by anyone's standards) fit into this clinical category, we find no data like these. Thus, it should be cause for great concern that we find theories which set out to explain agrammatism, when by this category they include fictitious data like (1a)–(3a). A theory which successfully characterized this reified category is no more likely to be applicable to the group identified by clinical impression than one which is falsified by this putative evidence. (What other relevance this sort of theory could have is entirely obscure.) We should also point out here that our arguments are not meant to imply that, in principle, a patient with a pattern of performance like that indicated in (1a)–(3a) cannot exist—i.e., a patient whose language deficit is exhaustively characterized in terms of the omission of grammatical morphemes. It is an empirical issue as to whether there is a deficit fitting reasonably to this idealization. We simply hold that there is no clinical or methodological warrant for positing this reified syndrome as a psychological entity, and that to hypothesize such a syndrome in the absence of both strong theoretical motivation and attested occurrences is more an exercise of will than of scientific method.

1.2. The empirical approach

Suppose, though, that we don't start out by defining agrammatism in this way. How might we go about formulating a theoretically motivated specification of this putatively psychological category that would provide an objective basis for identifying instances of the category? Can't we simply examine the performance of the members of this category and derive, by empirical means, an objective characterization of agrammatism? A primary consideration in this regard is how to determine the extension of agrammatism within the aphasic population. In order to apply this "empirical" method, it must not be an arbitrary matter as to whose performance we examine. The difficulty we face in practice is that we must assume the theoretical validity of the clinical impression despite the fact that we lack both necessary and sufficient operational criteria for identifying members of this category. In Goodglass's (1976) discussion of agrammatism, the criterion for classifying patients as such in most current discussions (the omission and/or misselection of function words and inflections) is noted to be inadequate for the purpose of delimiting the grouping based on clinical means. He reports from Goodglass and Mayer's (1958) comparison of patients classified as agrammatic and paragrammatic that "omissions and substitutions of grammatical morphemes were more frequent in the agrammatic group, but showed considerable overlap
between subject groups" (Goodglass, 1976, p. 247); and he notes that the criterion advocated in that study, "a tendency ... to fall back on the most habitual simple sentence types and ignore those structures which represent more complex syntactic relations" (ibid.), is also unsatisfactory. Instead, Goodglass advocates a criterion elaborated in Goodglass, Quadfasel and Timberlake (1964), which is based on utterance length. He writes:

The short-phrase dominant patients ... included all the patients in the sample who were clinically classified as Broca's aphasics with agrammatism. Only one paragrammatic patient fell into the short-phrase group. One of the practical benefits of this finding was to provide a simple and relatively objective criterion for classifying aphasics for further linguistic analysis, without the circularity entailed in the Goodglass-Mayer study (Goodglass, 1976, pp. 248-9; emphasis added).

However, this measure of 'fluency' is also unsatisfactory as a means for objectifying our notion of agrammatism given reported cases in which the patients produced phrases of considerable length despite the fact that their utterances were judged agrammatic (Kolk and VanGrunsven, 1981; Miceli, Mazzucchi, Menn and Goodglass, 1983). Thus, utterance length can be neither a necessary nor sufficient criterion for diagnosing agrammatism. In this regard, we have failed to objectively determine the extension of this clinical category.

Of course, one could be tempted to believe (though we think it is a mistake) that this methodological shortfall need only be temporary. If we recognize the clinical category as an approximation only, might it not still be possible to focus in on a coherent nexus of performance features that will allow us eventually to fix the exact parameters of this category? To have any confidence in such a proposal, there are two issues that we must settle. One is the status of variation in performance which is taken to indicate agrammatism: We must be able to distinguish within-category variation from across-category variation if there is to be any possibility of recognizing the actual extension of agrammatism so conceived. The other is how the selection of

6 Goodglass (1976) characterizes 'paragrammatism' as involving "not so much the reduction of grammatical organization as the juxtaposition of unacceptable sequences: confusions of verb tense, errors in pronoun case and gender, and incorrect choice of prepositions" (p. 238).

It is worth noting that the preceding grammatical description of paragrammatism bears a striking similarity to the description of agrammatic production which appears in Grodzinsky et al. (1983). Here, the so-called agrammatic performance deficit is described not as a loss of the closed class item vocabulary per se, but rather as a loss of the ability to select properly among inflected forms. On this view, the [agrammatic] patient retains all the morphological rules ... But what is not available to the patient is a means of selecting the properly inflected form. As a consequence, he is forced into a guessing situation concerning what form to use.
criteria for identifying instances of the syndrome is to proceed, and whether it is possible to make such selections objectively. In the section which follows, we examine the sort of variation one encounters in the speech of so-called agrammatics, and we discuss the significance of this variation with respect to the problem of establishing the consequences of category membership. In section 3 of this paper, we proceed to the issue of how and whether we can select criteria to pick out members of a category that is significant with respect to the mechanisms of cognitive processing.

2. Variation in agrammatism

In order to appreciate the significance of our inability to objectify the clinical impression on which the clinical grouping is based, it would help to consider some examples from the literature on agrammmatism. For instance, Saffran, Schwartz and Marin (1980) provide excerpts from transcripts of patients' attempts to produce descriptions of pictures. In one such picture, a girl is presenting flowers to a woman in a classroom setting. The following utterances represent the efforts of (presumably) different patients they classified as agrammatics.

(4) a. The young...the girl...the little girl is...the flower.
b. The girl is flower the woman.
c. The girl is...going to flowers.
d. The girl is giving...giving the teacher...giving it teacher.

Saffran et al. (1980, p. 225) recognize "the omission of functors and inflectional affixes" as "the criterial feature of agrammatic speech"; but it is clear that the examples in (4) are not adequately characterized solely in terms of this criterial feature. (4a), for example, lacks a main verb (or inappropriately employs the main verb be); and there is only one candidate inflectional omission which would not be accounted for by the absence of the verb—a plural marker on flower. Similarly, (4c) is at least semantically ill-formed, but ungrammaticality in this case cannot be attributed to the omission of functors or inflectional morphemes since no such obligatory items can be said to be missing. In still another response that Saffran et al. report, a main verb is replaced by a nominal stem with verbal inflection:

(5) The girl is ...is roses. The girl is rosin'.

Apart from this categorial error, though, the final sentence exhibits an entirely adequate deployment of inflections and function words. The response in (4b) is more ambiguous in light of examples like (5) since we can interpret
it as relating to any of the syntactic targets in (6). (Target items which could have been omitted are bracketed.)

(6) a. The girl is [giving] [the] flower[s] [to] the woman.
   b. The girl is [giving] flower[s] [to] the woman.
   c. The girl is flower[ing] the woman.

If we assume the more conservative analyses of (4b)—i.e., that the target was something closer to (6a,b) than (6c)—then the omission of a main verb is consistent with the performance of the subject who produced (4a), but not with that exemplified in (4c,d).

The point, of course, is not to generalize across utterances, but to see whether we can do so across subjects. The omission of main verbs is, as Saffran et al. point out, a common feature in speech characterized as agrammatic. However, it happens that the omission of grammatical markers in a patient's speech does not entail the omission of main verbs. Miceli et al. (1983) report an Italian patient who is a case in point. This individual, T.F., omits determiners (72%), auxiliaries (73%), preposition + determiners (49%), clitic pronouns (100%), and prepositions (26%), but does not omit main verbs. However, main verbs in T.F.'s speech are almost always inflected inappropriately, despite the fact his verb-stem finding abilities were unimpaired. This pattern of performance clearly fits the omission-of-function-word criterion for classification as agrammatic, so we are led to ask what is the status of a deficit which subsumes both T.F.'s speech and that exemplified in (4a,b,c).

The picture of agrammatism is further complicated by the fact that, in at least one reported case which satisfies the omission-of-function-word criterion, the use of main verbs is generally preserved (though the verbs employed are often semantically inappropriate), but the verb forms agree syntactically with their subjects or governing auxiliaries. (Saffran et al. (1980) provide a somewhat extensive transcript for this patient on pp. 234-5.) His output contains omissions of determiners and auxiliaries (though not a great many) and it exhibits selectional difficulties with verbs and prepositions (including occasional substitutions of nouns for verb stems). For example, the following utterances were among his responses to a picture description task.

(7) ...The painter washed the paint...(picture of a man painting a house)
(8) ...The cat leans the sofa up...(picture of a cat peeping out from behind an armchair)
(9) ...The boy put the valentine into this girl. (picture of a boy giving a valentine to a girl)
(10) The man kodaks...and the girl...kodaks the girl. (picture of a man taking a photograph of a girl)
However, only one apparent inflectional error occurs in this corpus, and that is the intrusion of an inappropriate plural marker ("the lady puts the washes..."). This form may in fact be a lexical blend of wash and clothes, which is an error type involving lexical form selection under the influence of semantically related items competing for the same phrasal position (Garrett, 1980, 1982), and so it does not constitute a clear example of an inflectional error. The items which occur as main verbs are all properly inflected (e.g., "the lady kissed...", "the lady is putting the tray on...", "the man drives...", and the verbs in (7)–(10)).

The contrast between T.F.'s speech and that of Saffran et al.'s patient is striking. If we were to attempt to apply the same category label to these two individuals, it is obvious that we would have to ignore many of the details of each individual's output. Would it be legitimate to make such generalizations? To the extent that both cases are members of the same category, it ought to be possible. But if the different varieties of omissions and misselections that occur in the speech of so-called agrammatics can overlap, then what we take to be significant variation among members of this category should be determined by and measured in terms of the parameters countenanced by our theory of what agrammatism is.⁷

Consider the following examples of spontaneous speech from Goodglass (1976, pp. 238–9). (11) is a patient's attempt to explain why he has returned to the hospital, and (12) is another patient's autobiographical sketch.

(11) ah...Monday...ah, Dad and P.H. (the patient's name) and Dad...hospital. Two...ah, doctors..., and ah...thirty minutes...and yes...ah...hospital. And, er Wednesday...nine o'clock. And er Thursday, ten o'clock...doctors. Two doctors...and ah...teeth. Yeah, ..., fine.

(12) My uh mother died...uh...me...uh fi'tecn. Uh, oh, I guess six month...my mother pass away. An' uh...an'en...uh...ah...seventeen...seventeen...go uh High School. An' uh Christmas...well, uh, I uh...Pitt'burgh

Goodglass describes the second patient as exhibiting a "milder level of the same type of disturbance", a distinction he bases on the observation that in (12) there are instances of Subject–Predicate constructions (My uh mother died, my mother pass away), whereas in (11) one finds only nominal phrases. However, this characterization ignores another distinction which is grammat-

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⁷ Clearly, if we posit a psychological category of deficit which allows broad variation, then the set of coherent psychological characterizations of this category will be limited to those which can encompass this variation. We discuss such consequences of variation in more detail in section three.
ically based. The output in (11) contains several occurrences of properly inflected plural noun phrases (*two doctors, thirty minutes*), while the speech in (12) contains no plural inflections, even when they are required (*six month*). In describing these two cases, we may very well want to maintain that the variation which Goodglass focuses on and the variation regarding nominal inflection differ in terms of their significance, but it should be clear that such differences in significance cannot be determined a priori. In fact, we want to maintain that the extent to which the distinction can be made will depend to a great extent on the meaningfulness of the claim that any two aphasics have "the same type of disturbance" when there is any variation at all in their performance. For such a claim to have meaning, we must have a theory in terms of which this 'sameness' is defined.

The problems we face in making such distinctions arise out of the pre-theoretic status of agrammatism. Since we lack necessary and sufficient criteria for identifying members of this category, we depend on having a theory of agrammatism in order to justify acceptance of this category as a psychological entity. Such a theory must enable us to make specific predictions regarding the linguistic performance of agrammatics and, in so doing, limit and explain variability within this category. If our theories fail this second requirement, then they cannot succeed in satisfying the first. As we argued above, for example, it cannot be an arbitrary matter as to what will count as significant variation and what is simply noise without the consequence of rendering the selection of evidence an arbitrary matter as well. The fact of the matter is, though, that we do not have any candidate theories which will justify treating one or another of the differences in the examples of aphasic speech just cited as one kind of variation or the other. What distinguishes the variation in the extent and contexts of omissions of function words in these examples from the same sort of variation in the deployment of content words (such as main verbs)? Could we say, having employed the function-word-omission criterion to identify an agrammatic, that the omission of auxiliary verbs (which are function words) is any more a consequence of the agrammatic deficit than the omission of main verbs (which are not)? Suppose a patient, like T.F., omits some function words but not main verbs. Is this enough to conclude that main verb omission in other cases is not the product of deficit to the same functional system which, as the result of brain damage, gives rise to function word omission? If it were, then the same logic of dissociation would enable us to argue that a patient who omits some prepositions and determiners but not auxiliaries provides sufficient evidence that the psychological mechanism whose disruption underlies the omission of prepositions and determiners in this case cannot be invoked to explain the omission of auxiliaries in the speech of some other aphasic. This sort of reasoning
would quickly lead us to posit a separate processing mechanism for every item that is retained in aphasic speech, but this would just amount to another arbitrary treatment of variation.

It is more reasonable to maintain that there is no simple discovery procedure (like following the logic of dissociation) that would alone enable us to affirm or deny such hypotheses: To make these sorts of decisions regarding the significance of instances of a syndrome, a principled means for distinguishing types of variation is required. These means, we argue, will only be available if we have at our disposal a theory which provides detailed accounts of normal speech processing and in terms of which the specific patterns of performance of individual aphasics can be explained. It's not difficult to see that if we did have a computationally adequate model of language processing which could be modified (or damaged) in such a way as to explain the various patterns of performance of individual aphasics, then the distinction between within- and across-category variation would have a solid theoretical basis. (This assumes, of course, that the category at issue can be defined by this model.) What may appear less obvious to some, but which is no less true, is that no equally cogent principles for drawing these lines have yet been proposed (much less applied). Thus, lacking such a computationally adequate model to guide us in the interpretation of putative instances of agrammatism, we are left to rely on intuition for deciding which features of performance are entailed by this categorization and which are the result of independent psychological deficits.

To summarize up to this point, we have argued that it is quite unacceptable to base scientific inquiry on intrinsically subjective methods. However, such methods would appear to be unavoidable when categories that are so poorly delimited by the criteria employed to identify their members (or by any candidate for the sort of theory that could explain these categories) are taken as a starting point for neurolinguistic research. We can find no basis for assuming the non-arbitrariness of the category of agrammatism as it is defined by the grouping based on clinical impression, nor can we find merit in preserving that assumption. Rather, we think that it should be clear that, at least with respect to the issues we have examined so far, this assumption has raised more problems for research than it has solved.

3. The notion of syndrome and classificational criteria

From our foregoing discussion, it should be clear that we believe that an explanation of specific patterns of aphasic performance has to be based on explicit hypotheses concerning the computational system underlying normal performance. That is, given some pattern of impaired performance, its expla-
nation will consist of a model of normal processing along with postulated modifications to this system (specified in terms of disruptions to a unique subset of the components) in virtue of which that pattern may be derived (see Caramazza, in press, for detailed discussion). Clearly, the power of any such account is directly proportional to the degree of explicitness of the normal model. Where it is possible to elaborate such a computationally adequate model of linguistic processing, we may test this model by requiring that, when damaged appropriately, it will produce the observed patterns of impaired cognitive performance. It is under this requirement that the data from aphasia are relevant to the goal of understanding normal linguistic processing. More important to our present purpose, though, we consider this the only route to an explanatory account of impaired performance.

In this section we discuss the category of agrammatism when considered as a psychological syndrome—i.e., as a syndrome that is defined or definable in psychological terms. Specifically, we examine the limits of the mode of explanation we have been proposing with respect to agrammatism conceived of as a syndrome. We also consider the role of assumptions about its status as a syndrome (i.e., the sort of syndrome it might be) in studies that take agrammatism as a starting point. Finally, we will discuss how (and whether) one could collect evidence for treating agrammatism as a psychologically definable syndrome. In order to meet these goals, though, it is important that two different senses of the term 'syndrome' be distinguished, and it is to this task that we apply ourselves first.

In the case of an individual aphasic, the notion of explanation is rather straightforward. Since the deficits are considered to be the result of impairments to a specific set of processing components, an understanding of this individual's performance on linguistic tasks must derive from a characterization of the system which remains intact, of the residual processing capabilities of disrupted components, and of how the components in that system interact to produce that particular pattern of performance, just as an understanding of normal linguistic processing derives from a characterization of the normal processing components and their modes of interaction (Caramazza, 1984). This, as Kean (1979) writes, is a matter of logical necessity. To see how this mode of explanation carries over to the explanation of aphasic syndromes, however, we must first distinguish two different senses of the notion syndrome.

There is a weak sense of syndrome which is based on the statistical reliability of the co-occurrence of symptoms, and a syndrome in this sense need not have a coherent interpretation in terms of the functional structure of the system underlying language processing. That is, the pattern of co-occurring symptoms may only correspond to a family of functional deficits, where no
particular functional deficit (i.e., no single set of disturbed components) is implicated in every instance of the syndrome. For example, such a pattern of co-ocurrence could result from accidents of proximity or some other variety of organization which holds of the neuronal substrate, but which do not respect the functional organization of the cognitive mechanisms (see Caramazza, 1984).

The strong sense of the notion syndrome corresponds to groupings of individuals according to categories delineated by the set of normal processing components. Under such a conception, the essence of a syndrome is that it is defined in terms of deficits to a specific set of processing components. Unlike a syndrome-in-the-weak-sense, a syndrome-in-the-strong-sense is not identified directly with the various patterns of performance of individual members of this group or category, but with those aspects of performance which result from the particular deficits to the normal system in terms of which the syndrome is defined. Under the strictest interpretation of this notion of syndrome, no two individuals present with the same syndrome unless the disrupted and intact systems (as defined by the normal system) are absolutely identical. However, there is a less stringent interpretation of syndrome-in-the-strong-sense which still permits us to speak meaningfully of a syndrome as a psychological entity: so long as all and only the patients categorized by syndrome X are impaired by means of damage to a specific, fixed set of processing components, we can still speak of X as a syndrome which corresponds to those aspects of performance that can be attributed to the deficits in those particular components. That is, we will still identify the syndrome in terms of a fixed set of affected components, but an individual who presents with syndrome X could simultaneously exhibit additional aphasic symptoms (defined in terms of deficits to another set of processing components). We believe that this less restrictive version of strong syndrome comport with common usage, but we wish to observe in passing that if we cannot justify treating an aphasic category as a syndrome of this less restrictive variety, then our arguments most assuredly would not suffice to establish it as a syndrome under the strictest interpretation.

In the case of strict syndromes-in-the-strong-sense, the mode of explanation we described for individual aphasics carries over without significant amendment. The pattern of performance that is identified with a syndrome

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8 A number of researchers have in fact argued that the clinical categories can only be considered kinds at the anatomical level of analysis (Marshall, 1982; Poeck, 1983), but these arguments must be construed as support for our own position that it is a gross error to employ these categories as starting points for neurolinguistic or cognitive neuropsychological research.

9 Thus, if there were autonomous components in the normal processing system, there would be possible syndromes of this sort.
must be explained in terms of the component deficits that underlie the syndrome and the attendant account of how that pattern is derived from any such impaired normal system. Of course, when we discuss syndromes in the less restrictive version of the strong sense, we must consider those symptoms that result from the interaction of the effects of the disrupted components which comprise syndrome X, with those implicated in any other accompanying deficits. This is not logically different from the kinds of problems we face in explaining a syndrome in the strictest sense, though. The point we wish to stress is that it is the structure of the modified system which we identify as determining the essence of the syndrome-in-the-strong-sense, and not the patterns of linguistic performance—the 'symptoms'—which result from the deficits. Inasmuch as a syndrome is a projection of a natural partition of the modular system underlying linguistic performance, it may be considered a natural kind of deficit and accorded the status of a psychological entity. However, if X is intrinsically a syndrome-in-the-weak-sense, then it is neither a natural kind (a psychological entity) at the cognitive neuropsychological level of analysis, nor amenable to the mode of explanation of the sort we have been discussing. It is not a natural kind because it is heterogeneous with respect to the component deficits which give rise to the family of symptoms in terms of which X is defined; it eludes explanation because of this same heterogeneity. If we were able to characterize every individual who was judged to exhibit a syndrome that turns out to be intrinsically of the weak variety, then though these accounts would constitute explanations of each individual's performance, the set of characterizations would only amount to a description of an arbitrary set of component deficits. It would be arbitrary because there is no theoretical reason for including any one such description in the set and not another.

With these distinctions in mind, we must consider what are the consequences, for our research methods, of the sort of variation we encounter in speech characterized as agrammatic, and of our inability to provide necessary and sufficient criteria for inclusion in this category. Can we proceed from an approximation of the category to the discovery of objective criteria for membership in a “true” category? How could such a program be developed? A substantial number of neurolinguistic studies are devoted to an explication of agrammatism, where by explication is meant either (1) the presentation of certain hypotheses concerning the nature of processing systems which could produce agrammatic output (i.e., functional explanations), or (2) elaborations on what is entailed by membership in this category (e.g., studies which attempt to determine the range of linguistic abilities of agrammatic aphasics). Either sort of study begins with agrammatism as a given, and from this starting point attempts to develop an understanding of the processing mechanisms
underlying normal linguistic performance in terms of which agrammatic patterns of performance may be derived, or (in the second type of study) an understanding of the entity itself. More relevant to the questions currently under consideration, both types of research share some rather crucial assumptions. To begin with, it is assumed that agrammatism is a natural category and not an arbitrary grouping of subjects. This assumption is most evident in research whose goal it is to explain agrammatic patterns in terms of underlying processing systems. As we have explained, for such an approach, the essence of agrammatism is that it is characterizable in terms of impairments to a unique subset of the linguistic processing components. Individual agrammatics may vary to the extent that additional components of the system are implicated in each case, but it is the postulated fixed core of impaired components that determine the boundaries of the category agrammatism. That is, this mode of explanation entails a strong notion of aphasic syndrome. On the other hand, if there is no such fixed core of impaired components, if the clinical category can only be characterized in terms of a family of overlapping deficits (with no fixed set of processing components implicated in every case), then it wouldn’t be possible to use agrammatism, as a whole, to test our theories of normal processing. Furthermore, if there is no fixed set of disrupted components, then providing a list of all the (potentially non-overlapping) sets of disrupted components corresponding to an agrammatic deficit will only amount to an extensional description of agrammatism (an enumeration of cases). We would have failed to provide an explanation of agrammatism unless we could specify, in functional terms, what it would take for inclusion on this list. That is, if there is no theoretically motivated boundary to this family of deficits (at the functional level of description), then one new family member can always be added which is derived by impairments to components not implicated in all family members. If this is the case with agrammatism, then agrammatism is an intrinsically subjective category (since it must remain the burden of clinical intuition to determine the extension of the category). It follows that, even if one’s goal is simply to explore the limits of the linguistic abilities of agrammatics, the assumption that agrammatism is a natural category is one leg on which any defense against charges of triviality and circularity must stand. If agrammatism is merely a subjective category in the sense just described, then claims regarding the linguistic abilities of agrammatics are intrinsically arbitrary. Likewise, we would have to overcome this objection for our criterion of membership.

The other major assumption to be considered here is that one will be able to recognize the attendant features of agrammatism. Just as it is important for this kind of neurolinguistic research that the category of agrammatism be a psychologically natural category, it is essential that there be objective means
for determining the properties of agrammatism. If one were free to select—or were limited to selecting—arbitrarily from the performance of some aphasic group for the purpose of testing hypotheses about that group, the theoretical significance of one's findings would be severely undermined. One must be guided by principles in the selection of data if it is to be construed as evidence for a particular hypothesis. It would be no more satisfactory if it were an arbitrary matter as to what we were responsible for explaining among the patterns of performance within a category than it would if it were an arbitrary matter as to who could be admitted into or excluded from that category.

In line with these issues, the first observation we must make is that the delimitation of the category 'agrammatism' is, for all intents, arbitrary in the sense that it is not achieved on the basis of criteria that were selected by considerations of theory. Of course, this does not mean that these criteria are without theoretical consequence—that would not be possible. Our characterization of the situation is as follows: We do not have a computationally adequate model of language processing on the basis of which we can project the range of linguistic behavior that is considered to constitute agrammatic speech, nor are we equipped with necessary and sufficient operational criteria for identifying instances of this clinical type. Instead, a class of phenomena, the omission (and/or substitution) of function words and inflections, is employed to approximate the intuitive grouping of subjects. But since this category of phenomena both admits wide and unpredictable variation among so-called agrammatics, and it occurs in speech characterized as non-agrammatic (paragrammatic speech according to Goodglass, 1976), it is altogether reasonable to ask why this feature is used and not some other. Why not Goodglass's short-phrasing criterion, the presence of phonemic paraphasias, or the omission and/or misselection of main verbs (where 'misselection' would include any variety of inappropriateness, semantic, subcategorizational or inflectional)? We have no theoretical basis for choosing one of these criteria over another, and none of them alone or in conjunction with another pick out just that group we are supposedly studying.

Even if we were satisfied with the limited utility of the "criterial feature" of agrammatism, we must question the motivation for this criterion on other grounds. The partition of lexical items in terms of which agrammatism is defined can be captured by means of several grammatical contrasts (which vary in the degree of their formal motivation)—function words vs. content words, phonological clitics vs. phonological words, minor lexical category vs. major lexical category, etc.—as well as by certain computational distinctions. Each of these distinctions will effect roughly the same partition. How-

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10 See, for example, Kimball (1973) for a discussion of the role of minor category words in surface structure parsing.
ever, the relevant boundary lines for this bifurcation of the lexicon cannot be
determined simply by cataloguing which items are omitted in speech charac-
terized as agrammatic. To the extent that the omission of items which com-
prise a particular set of words (the closed-class vocabulary) imperfectly picks
out instances of agrammatic speech, we must confront the fact that this
characterization of the omitted items has not been chosen on the basis of a
theory which could explain why these items are omitted. Given that in actual
transcripts of so-called agrammatic speech there are almost always some
closed-class items retained in some contexts, and given that we have no
explanatory account based on this characterization as to why certain of these
items are retained and when, it would appear that we could just as accurately
predict which words will be omitted if we identified agrammatism in terms
of the omission of words which are both very frequent and very abstract.
(Eighty-five of the one hundred most frequent items in Carroll, Davies and
Richman’s (1971) word count are function words, and generally speaking, the
remaining function words cluster around the high end of that frequency rank-
ing. Open-class items in the overlapping range tend to be more concrete than
their closed-class neighbors.) Failure to account for the variation with regard
to which of these frequent and abstract words will be retained (and when
they are retained) is no more (and no less) of a problem for the frequency-
abstractness analysis than it is for the analysis which attributes the omission
of these items to their status as function words. Present accounts all seem alike
in that they treat this kind of variation as insignificant. The fact that we have
no theoretical motivation for choosing one of these characterizations over the
other should clearly demonstrate the arbitrariness of the criterion.

Inasmuch as the grouping of patients into the agrammatic category is arbi-
trary on at least these two important counts, we have no reason to believe
that this grouping coincides with a psychological natural kind. Of course, it
is and should be an empirical issue as to whether there is a syndrome (in the
psychologically strong sense) which subsumes only and all, or only and some
specific subset of, those cases that have been characterized as agrammatic. It
is our contention, though, that no empirical demonstration of either case has
been effected, nor is it a reasonable expectation that taking agrammatism as
a given will somehow lead to a relevant demonstration. To the contrary, the
assumption that there is a psychological entity ‘agrammatism’ is more likely
to direct us away from seeking valid evidence for one of these positions.

We have been at pains to point out that the burden of theory is particularly
great in any attempt to support the nonarbitrariness of this category. Would
this situation improve if, by adding criteria, we could better approximate the
clinical grouping? (Again, we should note that to even ask this question is to
grant that this clinical group should be allowed a special status, though we
have provided no reason to believe that this group is not arbitrary.)

The first observation we should make in response to this question is that, regardless of whether a new criterion is employed to delimit the category of agrammatism differently, or simply to subdivide the group that is picked out by the omission-of-function-word criterion, the new criterion will have to overcome the objection that it imposes an arbitrary grouping. The choice of a criterion of inclusion must be based on a theoretical characterization of the category that it will be used to establish membership in; otherwise we can have no confidence that the group delimited by the conjunction of new and old criteria will correspond to any theoretically significant group. Any number of taxonomies could be imposed on aphasics, but the utility of an arbitrary taxonomy cannot be antecedently specified. If agrammatism is construed to be capable, by virtue of its putative natural kind status, of revealing some important details concerning the structure of the mechanism underlying language, then the privileged status of agrammatism in this regard admits of the possibility that a category which does not have this natural kind status will not be able to reveal such details. But, since we have no strong reason to believe that agrammatism is one of these favored categories, the ability of some new criterion to bridge the distance between the clinical grouping and the grouping based on our present criterion would not in and of itself be evidence that matters had changed. Only if the choice of criteria for membership in some category were based on an account of the nature of that category would this be different. Thus, the further we are from having a computationally adequate account of language processing, the further we are from being able to produce an informed set of criteria for grouping patients. Similarly, having a new criterion that is able to group subjects in a particular way will not, by virtue of this ability, solve the problem of how to interpret variation. As we have argued, the resolution of this problem too can only be achieved by way of a processing model that is capable of accounting for the details of the performance of individual aphasics.

Another point that should be made in response to the question concerning the utility of adding new criteria for agrammatic classification is that the selection of new criteria for membership, even if not consciously made on the basis of a particular theory of processing, is not theory neutral. Since it is a common trap to seek a characterization of agrammatism in terms of the characterization of speech under these criteria—even when the criteria for membership are not employed for theoretically sound reasons—a corollary to the theory-laden status of criteria selection is that we should not expect such means of selection to be methodologically productive (or even benign). Consider the following candidate for criterial feature of agrammatism. Patients classified as agrammatics typically have right hemiparesis (RtH). Sup-
pose, for the moment, that every patient that 'presented with agrammatism' up until today has RtH. Should RtH then be included as a criterial feature of agrammatism? Few would answer this question affirmatively because, according to any theory of language processing either explicitly stated or covertly held today, the mechanisms the disruption of which will result in RtH are not considered to be intrinsically related to the mechanisms which underlie language processing (and which are implicated in what are considered to be 'linguistic' deficits). But wouldn't we be compelled to revise our position on these matters in light of the pervasiveness of this correlation? No, not if we can develop computationally adequate models of language processing which do not draw on the kinds of mechanisms that, when disrupted in a particular way, will underlie RtH. Of course, if we couldn't construct an adequate model of processing that respected this partition, then RtH would have to be considered a criterial feature of agrammatism (assuming that agrammatism exists as an entity in terms of these unlikely models). The point of all this is that the decision of whether RtH should be a criterial feature of agrammatism will usually be made on the basis of our theories, most often covert theories, of what kinds of deficits could explain agrammatism. It's only because the intuitively plausible candidate theories do not link the two sets of mechanisms in any essential way that we can feel confident of such decisions. It is not a theory-independent decision even when the theories we draw from are rather minimal in predictive power. When such distinctions are not so clear (due to the poverty of our theories of language processing), then judgments regarding other candidate criteria, ones which are more linguistic in character, will be correspondingly intuitive. As we argued before, this is undesirable.

As to the consequences of adding new criteria, it should be clear that these new criteria may have deleterious effects. The more one attributes to an idealized entity, the more one limits the class of possible accounts of this entity, and these restrictions will not always be felicitous—nor should we expect them to be so. An instructive example of the pitfalls relating to this aspect of criterion selection can be seen in Berndt and Caramazza's (1980) attempt to redefine Broca's aphasia. In that study, agrammatism, asyntactic auditory comprehension and disturbed metalinguistic abilities are considered—on the basis of their intuitive relatedness as well as a statistically reliable correlation—to be features of one functional deficit. Given the disparate nature of these categories of deficits (if only considering the separate modalities of comprehension and production), the candidate analyses for such a unifying account are of necessity abstract. In the case of Berndt and Caramazza's solution, a central syntactic parser is posited as the locus of the general deficit. According to their account, the parser functions in normal
processing both to recognize the hierarchic and categorial organization of input sentences and to impart syntactic structure on sentence outputs during utterance planning and execution. Presumably, this component is also invoked for the performance of metalinguistic tasks. The problem with this theory, as we see it, is that apart from the desire to provide a theoretically unified account of phenomena that have been grouped for (at least partially) intuitive reasons, there is no motivation for proposing such a processing component. Simply asserting what the parser is supposed to accomplish in the performance of several different tasks does not suffice as a demonstration that it is a theoretically meaningful component at any level of description. Minimally, one would want to know how the proposed mechanisms work (both in terms of the component’s internal mechanisms and its input–output relations with other components of linguistic processing). Without some indication of how the hypothesized parsing component can be made sense of computationally, the proposal goes no further than positing an underspecified linguistic faculty for the sake of collecting several intuitively related phenomena under a single generalization. As it stands, though, the account which invokes this abstract parser fails to predict any of the details of so-called agrammatic language production or asyntactic comprehension, or poor performance on metalinguistic tasks. None of the disparate phenomena that have been lumped together are explained by Berndt and Caramazza’s analysis.

Having made our case against agrammatism based, among other things, on the arbitrariness of the delimitation of this category, we should stress that we have not claimed that there do not exist any combinations of criteria which can pick out theoretically relevant (i.e., natural) groupings of aphasics. However, we have argued that the selection of such criteria is not independent of the problem of selecting the correct theory of normal processing. Thus, we should not invest our confidence in categories that are not based on computationally adequate theories of linguistic performance and that are not supported by the success those theories have in accounting for individual cases of aphasic deficit. The lesson for neurolinguistic research is that the method of inquiry should not be to ask questions about some subjective phenomenon like agrammatism, since research which starts from this point is unequipped to provide any theoretically motivated criteria that can objectively pick out the intuitive grouping (or even a close approximation of it). It is simply bad method to build on such unsound foundations.
4. Summary and conclusions

Is there any utility in taxonomizing patients prior to the development of a theory in terms of which the patterns of patients' performance can be accounted for individually? In terms of our examination of the category agrammatism, we have answered this question relative to specific research goals:

(1) Not if our goal is to use data from the aphasic category to test models of normal processing, since for group characterizations of aphasics to be relevant to this task it must first be the case that the models in question are up to the task of explaining the individual instances of the category;

(2) Not if our goal is to explain the aphasic category in terms of normal processing mechanisms and deficits to these mechanisms, as this too is theoretically posterior to providing an account of the patterns of performance of individual members of the category; and

(3) Not if our goal is to identify the consequences of agrammatism, nor if it is simply to provide statements as to when agrammatism occurs (e.g., 'locating' the anatomical correlates to agrammatism), because, in the absence of any justification for treating agrammatism as a non-arbitrary entity, any hypotheses which assume its non-arbitrariness will be empty. Such justification, we have argued, can only be derived from theories which (here by hypothesis) we lack.

Categorizing patients as an end in itself may count as an unassailable goal on theoretical grounds, but it is similarly indefensible as well. We find no other utility in this activity.

The conclusions we have reached about agrammatism could have been established just as easily for any other of the aphasic, dysgraphic or dyslexic syndromes which populate current neurolinguistic discussions. In every case, the methodological problems these categories pose far outweigh their explanatory benefits (which we believe are largely nonexistent). We have raised our particular objections not because of a specific disaffection with agrammatism, but because of the disturbing theoretical and methodological consequences of taking any such such syndrome as a basis for neurolinguistic research. At the same time, we do not believe that, by advocating the expulsion of categories like agrammatism from our research, we have left no route for serious exploration into the territory of aphasia. Nor have we called into question the applicability of neurolinguistic research to the program of developing models of normal linguistic processing. To the contrary, we believe that by demonstrating the methodological imperative for dispensing with these categories, we have taken the first step toward identifying those research methods which hold any promise.
In the sort of research we have been criticizing, one pattern recurs persistently. By taking as a starting point a category such as agrammatism, a requirement is placed on our theories that they explain an unspecified (and perhaps unspecifiable) 'sameness' in the members of this category. As we have argued, though, problems arise because the category cannot be delimited objectively and, since we lack a model of linguistic processing of the sort that could demonstrate the objective nature of this category, there is commonly no way even to determine which features of an individual's performance can be construed as evidence for or against a particular hypothesis concerning this category. Doing away with such categories will, of course, obviate any attempt to provide evidence for their natural kind status. What is more important, though, is that it will impose certain limitations on the kinds of questions we ask. Since grouping of patients will have no theoretical preeminence, there is no more motivation for asking how two instances of aphasic performance are similar than for asking how they differ. The sorts of questions which are relevant to the task of characterizing individual aphasics concern the underlying mechanisms of linguistic performance and how particular patterns of performance can be accounted for in terms of disruptions to these mechanisms. Similarly, the statistical distribution of co-occurring patterns of aphasic performance (within a group) has no bearing on the question of how the normal system is organized functionally. Hence, it doesn't matter that the dissociation of symptoms X and Y is either common or rare: what is important is whether, in those instances where X occurs without Y, the underlying deficit responsible for X is responsible for it when X does occur with Y. The requisite means for answering such questions provides no basis for grouping patients: Symptoms X and Y must be understood in terms of performance on various linguistic (or other cognitive) tasks. By providing a model of such tasks that is explicit to the degree necessary to predict how it would operate under various deficit conditions, we can hope to understand how X and Y are related. This involves testing patients who exhibit these symptoms (separately or together) on those tasks we have modeled, and interpreting their performance relative to that model. The means for determining the answers to such puzzles may require great effort, but they at least do not elude us for methodological reasons.

In a single-case study, the basic units of analysis are the patterns of performance a subject exhibits on particular tasks. To understand such patterns, it is necessary that we begin with a conception of each task in information processing terms so that, to as great an extent as possible, the task can be understood computationally. That is, a preliminary goal is to characterize each task in terms of the mechanisms which subserve it so that we might distinguish what is essential to its performance. The overriding requirement
that we must place on this characterization is that the information processing model of each task be explicit enough in computational terms so that we can predict how the system would perform if specific components of the model are disrupted.\textsuperscript{11} When we apply such characterizations to the task of understanding the linguistic abilities of a single subject, we will, of course, be interested in an overall pattern of performance on a set of tasks which are, under these characterizations, convergent in terms of the mechanisms that we have hypothesized. Insofar as these models stand up to the test of characterizing elicited patterns of performance on these tasks, we will have garnered some support for these models. Where our models prove inadequate for the characteriza-
tion of a specific pattern of performance on a task, this inadequacy reflects either an incompleteness of detail in our model of this task, or an error on some point or points. By extending our models to the limit of linguistic processing abilities, the assessment of the aphasics’ patterns of performance on various tasks will be able to address the development of models of normal processing. The obvious added restriction is that, regardless of the number of patients we observe, and no matter how varied their performance on different tasks, our interpretations of their performance must converge on the same model of normal processing. Clearly, when our models are both sufficiently explicit and correct, the accounts they provide of individual aphasics’ performance will constitute an explanation of their deficits.

It is not our goal to detail the ins and outs of single-case study methods. For more discussion on these issues, the reader is referred to Caramazza (1984; in press), Caramazza and Martin (1983), and Shallice (1979). However, its is important to point out that the method of research that we have proposed has the added advantage that it avoids the second major drawback of the method of inquiry which takes aphasic syndromes like agrammatism as starting points for analysis. When we test a model of performance of a linguistic task by requiring that it account for attested patterns of performance on that task, there is no question as to what we may take as evidence for our

\textsuperscript{11}Lapointe (1985) is an example of a study which takes seriously many of the requirements we have detailed for an explanatory account of particular patterns of aphasic performance. Lapointe’s study develops a detailed characterization of the computational properties of a verbal-phrase component comprising part of the normal language production system, and it is shown that specific deficits to this component can account for the patterns of verb form use found in three otherwise dissimilar case studies. Lapointe also distinguishes a number of other patterns of poor performance that would be expected under different deficit conditions on this component. The true test of these hypotheses, however, is not whether it succeeds in accounting for numerous characteristics of the clinical category of agrammatism, or even whether it can be invoked to explain the performance of all so-called agrammatics. Lapointe’s theory will stand or fall on the basis of its ability to account both for normal verb form production and for particular patterns of disrupted performance on tasks which, under a reasonably explicit characterization, invoke the performance of this hypothesized component.
hypotheses concerning these mechanisms. Any pattern of performance on a
task must be considered as evidence for or against the adequacy of the model.
Thus, by taking it as our primary research goal to discover which models of
language processing are up to the job of explaining individual aphasic deficits,
and not to group patients, the questions concerning what is to count as evi-
dence for the legitimacy of patient groupings do not arise. In this framework,
it is and ought to be the performance models for the sorts of tasks which are
compromised by the subject's aphasia that generate predictions about the
various patterns of deficit that will result when different sorts of conditions
are imposed on an implicated processing component. So construed, neurolin-
guistic research is in effect guided solely by one's theory in the selection of
evidence for or against particular hypotheses concerning the mechanisms
which subserve normal processing or the nature of the preserved and dis-
rupted systems that are responsible for particular aphasic deficits.

Given, then, that the assumption of agrammatism (and similar categories)
raises essential methodological problems that undermine any findings that
are based on this assumption, there is reason enough for wanting to dispense
with such categories. Since there also exist methods of research which can
address both the question of how individual aphasic patterns can be explained
as well as the goal of constructing models of normal linguistic processing
without assuming that such categories are psychologically definable entities,
we have no need for maintaining categories like agrammatism either. Thus,
we believe that it would be entirely to the benefit of neurolinguistic and
cognitive neuropsychological research if the use of these categories were
eliminated.

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The case against agrammatism


Résumé

L’utilisation massive des catégories cliniques de l’aphasie dans les recherches en neurolinguistique et en neuropsychologie cognitive laisse présumer que les classements des patients reflètent les atteintes du système de traitement du langage selon des cadres théoriquement pertinents. On examine ce présupposé en se rappor- tant plus particulièrement à l’agrammatisme. De nombreuses raisons amènent à s’interroger sur la cohérence de l’agrammatisme comme entité psychologique. Pour répondre aux objections, il est nécessaire de remplacer les intuitions cliniques qui fondent cette catégorie d’aphasie par des critères objectifs permettant un regroupement théoriquement pertinent des patients. Pour cela on doit établir une distinction théoriquement motivée entre la variation inter et intra catégorie. Dans le cas de l’agrammatisme il semble que des obstacles méthodologiques sérieux rendent ce propos impossible. Les théories ne doivent donc pas prendre les catégories telles que l’agrammatisme comme des données psychologiques, particulièrement lorsque le but de la recherche est de comprendre les mécanismes de traitement du langage ou les déficits aphasiques eux-mêmes dans l’étude de l’aphasie. La méthodologie recherchée permet de faire ce que les études parmi des catégories cliniques.