Cognitive Deficits, Their Proper Description, and Its Theoretical Relevance

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This note has two parts: First, I examine some general questions that emerge following a proposal such as the one made in the preceding paper (Grodzinsky, 1986). There are some conceptual issues that were either ignored or not fully clarified in that paper, which I think should be spelled out in full. Second, I reply to the criticisms raised in the two preceding papers (Caplan & Hildebrandt, 1986; Sproat, 1986). The replies to some of those come from the general discussion, others are addressed directly.

SOME CONCEPTUAL ISSUES

The paper in question considers the proper description of language deficits. It contends that a precise and coherent description of any cognitive deficit, and linguistic ones in particular, must be done through the use of a theory of the normal function. Looking at data from agrammatic aphasia, one concludes that there is prima facie evidence that the deficit is partial from a structural point of view. That is, the syntactic abilities of agrammatic patients are partially impaired. Hence, the proper descriptive machinery, it is claimed, is to be found among theories of language structure. Indeed, one such theory is selected—Government and Binding theory (GB) as stated in Chomsky (1981). A set of data concerning the comprehension performance of agrammatic aphasic patients is presented, and it is shown that GB provides a formalism from which the data fall out naturally, granting some plausible auxiliary assumptions. Specifically,

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it is shown that there is a striking correlation between the distribution of a formal construct called \textit{trace} and the performance types that agrammatic patients exhibit. A statement is then proposed that modifies the linguistic model to account for agrammatism. This statement is claimed to serve as the proper descriptive generalization concerning the data observed.

These are the main points made in Grodzinsky (1986). Let us examine some conceptual issues that emerge immediately. Off hand, there are two such issues: The ontological status of such descriptive generalizations, and their theoretical relevance.

A descriptive generalization over data patterns is always stated in terms of some theory. This should be quite obvious. The terms used for description can never be "objective." This is a common mistake made by those who blame theoreticians for "apriorism." Any descriptive term is borrowed from some theory, be it as naive as school grammar, or as sophisticated as some generative theory. A description of observed phenomena, however, has no theoretical significance in and of itself. It is precisely what it is called: A descriptive generalization and nothing else. Unless some theoretical claims are made, based on this description, nothing follows from it. It is, in a way, an organized (and abbreviated) list of observations. Given this status, there could be, in principle, more than one such statement. Namely, there could be any number of theories that could account for a given set of data with the same degree of elegance. So, a descriptive statement need not be unique, but could have alternatives.

Another property of such statements, in the context of cognitive psychology, is that they are about data, not about psychological mechanisms. And, as Fodor and Garrett (1966) put it very crisply: "There is no reason at all to suppose that, because the evidence for psychological claims is (often) the occurrence of one or another bit of behaviour produced by some organism, psychology is therefore primarily in the business of arriving at generalizations about behaviour. On the contrary, psychology is primarily concerned with understanding the nature and capacities of the mechanisms that underlie behaviour and which presumably cause it" (p. 136, emphasis mine). In this context, generalizations about behavior may have no theoretical status at all, unless claims are made concerning them. So, when someone says that she/he has "characterized agrammatism," for example, what is intended is that some pattern has been observed in a data set, which is describable in terms of some theory. When this generalization is said to have predictions, this already assumes consistency on the part of the mechanisms that generated the data—not an implausible assumption, to be sure, but a tacit one that has to be stated somewhere, because it is an assumption concerning the mechanisms that underlie behavior, not behavior itself.

The characterization proposed in my paper has exactly the properties
just described. No more and no less. It is not a theory of agrammatism (a combination of terms that makes no sense), nor is it a theory about the language faculty, nor about the brain. Most importantly, it is not a theory at all. So, unless some claim is made on the basis of this characterization, nothing follows. This brings us to the second conceptual question: Is there any potential theoretical use of deficit characterizations, and if there is one, how does it work?

Cognitive theories are about the structure of the mind. The empirical evidence supporting these theories comes from behaviors that organisms exhibit. Take linguistic theories, for example. These are theories about the structure of the language faculty. Evidence that these theories use usually consists of grammatical intuitions of native speakers of language. These data are obtained via psychological experimentation, broadly construed: A speaker is asked for his/her judgments. So, even though facts about grammaticality, synonymy, and ambiguity of strings help reveal crucial aspects of our linguistic abilities, these constitute just a part of the available evidence. Data from the time course of language comprehension and production, or from language deficits might have potential interest for linguists, as it might give them insights into their object of inquiry—the language faculty—which could not have been obtained by their standard data. If the above is true, then there is potential use for deficit characterizations: They make order in data sets that could be used theoretically.

How should these data sets be used? Let us look at an outline of an argument that brings data from language deficits to bear on linguistic theories. Suppose that an insult to some area of the brain results in a selective loss of the ability to understand and produce sentences. The nature of the functional deficit, most commonly induced by stroke, depends on the anatomical site of the damage, which in turn is determined by the distribution of cerebral blood vessels, one or more of which has been ruptured or clogged, causing the brain damage. It is hardly imaginable, to be sure, that these physical variables will play an explanatory role in an abstract theory of language structure. From the point of view of this theory, the anatomical facts are completely arbitrary. Indeed, most language disturbances seem to have very little to do with knowledge of language. Rather, they affect specific modalities for use in ways which are uninteresting from the present perspective.

The situation changes, however, if one encounters a loss that can be shown to be selective from a grammatical point of view. So, if a case can be found, where the brain damaged patient is able to understand sentences of some syntactic types, but unable to correctly interpret others, this has some potential interest for the theory of syntax. What we might have in this case is selective impairment to language mechanisms, where
the selectivity is governed by structural principles. If such a case can indeed be found, it might serve as a powerful test for the biological feasibility of specific theoretical models in the following way: if a part of the language faculty is lost, and if our theory of structure is about this faculty, then the characterization of the structural deficit following this loss must be accommodated in the theory in a natural way. In other words, the theory must be able to state generalizations over the patterns of loss and sparing following brain damage. If it meets this requirement, it is Breakdown-Compatible. So, even though there does not seem to be a necessary match between some arbitrary damage to a cortical site and the theory of syntax, it should now be apparent why there may be language deficits that have direct relevance for linguistics. We can use data from selective brain damage (aphasic syndromes) to assess competing grammatical frameworks. This can be done by checking which of them meets the requirement of Breakdown-Compatibility. Sure enough, the patterns of impairment need not distinguish between theories in principle, as the damage might create impairment forms which are compatible with any known theory. If we do find a relevant case, however, this should be seen as enormous support for one theory, and a serious case against the others. Now a second issue arises: the source of the impairment, and the way it may interact with the claim.

There are two possible causes for the observed functional deficit: it could be either a result of a loss of grammatical knowledge per se, or a disruption to some process that brings this knowledge to use. If it were the former, namely knowledge, then the way grammatical knowledge is impaired is obviously relevant to the theory of syntax: the theory, which is about human grammatical knowledge, must be compatible with the observed deficit. As this deficit reflects the internal organization of grammatical knowledge. This compatibility is ensured just in case the pattern of impairment forms a natural class within the theory. So, were the impairment a result of a knowledge deficit, then the theory would be Breakdown-Compatible if the distinctions that the brain makes, as evidenced by breakdown patterns, were stabile in the theory in a natural, non ad hoc fashion.

On the other hand, the loss could be a result of a processing disruption. In this case, the same argument still holds if one assumes the relation between the grammar and the algorithms putting it to use to be other than arbitrary. That is, the processes involved in recovering grammatical structure on the way toward interpretation must make distinctions which are similar to those made by the grammar. If so, then a disruption to a processor which is responsible for recovering structures dictated by a particular linguistic theory, would still have to meet the Breakdown-Compatibility requirement.

We thus have a general method for the evaluation of linguistic theories
via data from aphasia: find out whether the pattern of loss is structural.
If it is, state a descriptive generalization over it, derive its consequences
and test them, and finally, check whether this description is statable in
your theory.

This is the structure of the argument concerning language deficits and
linguistic theories. If this argument is valid, then it holds universally,
namely, it can be made in any cognitive domain. So, the theoretical use
data from cognitive deficits in general, and linguistic ones in particular,
is in imposing neurologically based constraints on theories. I have made
this argument concerning linguistic theories (Grodzinsky 1984, 1985a),
where I argued that agrammatic data patterns from a natural class in
Government and Binding theory; yet in another grammatical theory—
Lexical Functional Grammar—they are statable in an extremely ad hoc
fashion. I have also made similar claims about a variety of parsing models
(Grodzinsky, 1985b).

In sum, descriptive generalizations, such as the one made in Grodzinsky
(1986) have no theoretical status. However, they can be used for theo-
retically relevant claims in the manner I outline above.

REPLY TO CAPLAN AND HILDEBRANDT

In their paper, Caplan and Hildebrandt (1986) make many claims. They
criticize virtually every aspect of my paper. I take up their criticisms
one by one.

Syntactic Issues

The first criticism is that the characterization I offer is based on a
misconception of GB theory. This, C&H say, is with respect to two
aspects of the theory: The binding of traces and the analysis of passive
by-phrases.

The binding of traces. C&H make the following three-step argument:
(1) My account is aimed at achieving a single generalization over cases
that involve two types of traces (Wh-traces and NP-traces). (2) The
account is based on the assumption that these two trace types are identical.
But (3) in GB theory, these two types are markedly distinct. Hence: My
account does not go through.

This argument is not a valid one. Below I give two versions of a reply:
A non-technical one, for those readers who are not familiar with the
intricacies of the theory, and a technical one (to be skipped by nonlinguist
readers).

One of the core notions in transformational generative grammar is that
of a transformational rule. In earlier versions of the theory (e.g., Chomsky,
1957, 1965, 1973) there was a variety of such rules (one transformation
for passive, one for negation, pronominalization, etc.). The effort in
recent years has been to reduce the number of transformations by imposing
NOTES AND DISCUSSION

constraints on their output. So, instead of stating many such rules, one very general rule (known as Move-alpha) is assumed, that can move constituents more or less freely in a string, and a variety of constraints is assumed to rule out ill-formed output. What a transformation does, then, is move constituents around while dropping a trace (a phonologically null, yet structurally represented element at the syntactic level) at the position from which an element was moved. As a consequence, many structures that do not even look similar are similar in this abstract sense: They all involve a transformational operation. Some examples of that are given in the paper under discussion (Grodzinsky, 1986). So, there is a variety of constructions that may differ in every possible sense, except in that they involve a transformation. From the point of view of the rule Move-alpha, they are similar, but in no other sense. The rule, then, is a generalization over these constructions. The fact that these constructions do not even look alike is of no interest from this point of view. A failure to see this is tantamount to the claim that since the sentences John saw Bill and Mary saw Fred are different, there is no generalization that can be made concerning them—a ridiculous claim, of course.

It is precisely in this sense (the rule Move-alpha) that relative clauses, passives, and cleft constructions are claimed to be similar. This is relevant to the claims made in my paper because the assignment of thematic roles by verbs and verb phrases is to positions, some of which are filled with lexical NPs, some of which contain traces. If one focuses on thematic assignment, then, given that traces (of any type) interact with this assignment in the same way across constructions, then a generalization over these constructions can be maintained. Therefore, while C&H’s observations concerning the variety of trace types is certainly correct, their argument simply does not hold, because all traces are subject to precisely one generalization: The rule of Move-alpha, and it is just this sense that is relevant for the claim they wish to dismiss.

Technically, there is one preliminary point, and some topics for discussion: The preliminary point is that in my paper it is said that traces are deleted from S-structure representation. Nothing is said about a failure of a coindexation mechanism, which is the claim that C&H incorrectly attribute to me. These two statements have different consequences. The former is restricted to transformationally derived sentences, whereas the latter includes antecedent/anaphor relations. My formulation involved traces only (whether the deficit is wider in scope is a yet unresolved empirical question). Now, to the discussion. Consider, first of all, the differences between Wh-traces and NP-traces. C&H seem to agree that the analysis proposed for NP-traces that are contained at S-structure representation of the passive construction would give the desired results. That is, delete the trace in object position, and the theta-role of THEME that the (moved) subject of the passive is supposed to be assigned via
its (now deleted) trace will not be assigned. Consequently, the subject of the passive would not have a theta-role. The disagreement seems to begin when Wh-traces are at issue. Wh-traces are, as C&H correctly point out (and as is ignored in the paper they attack, in order to simplify matters and suppress irrelevant distinctions), bound by an operator in COMP, which stands in predication relation to the head NP in the cases in question. What would the thematic representation of such a construction be once the trace was deleted? C&H claim that under this refined analysis, "the assignment of thematic roles in cleft-object sentences might be intact."

But how could it be? Given that the role of THEME in the cleft case (and in the embedded clause of the relative) is assigned to the trace, there is no way for the theta-structure of these clauses to be intact. So, object clefts and object relatives differ from subject clefts and subject relatives exactly in that the THEME role cannot be assigned in the former, whereas the AGENT role cannot be assigned in the latter. And, coupled with the Default Principle, the data are correctly predicted: Object relatives and clefts would yield chance performance, because their (illicit) thematic representation contains two AGENTS; subject relatives and clefts would contain a (licit, and in fact correct) thematic representation, hence they would yield above-chance performance.

The analysis of passive by-phrases. C&H do not dispute the facts. That is, they acknowledge that agrammatic aphasics seem to be sensitive to the preposition by. What they question is the analysis proposed in my paper, and as a consequence, the generalizability of the claim. Specifically, they point out that the passive by-phrase is not analyzed in GB as a daughter of S, but rather as a daughter of VP. Here are the two analyses, as reflected in these S-structure representations:

(1) a. [John] [\text{vp} was killed $t_i$ \text{ by Bill}]
   b. [John] [\text{vp} was killed $t_i$ \text{ by Bill}]

There are several syntactic arguments for the analysis in (1)b. I will give one. The contrast between (2)a and (2)b and (3)a and (3)b is accounted for by Huang's (1982) Condition on Extraction Domains if the by phrase is not an extraction domain. This condition prohibits extraction from ungoverned domains. So, to account for the contrast between the a and b cases, the PPs in the b cases must be ungoverned, so that extractions from them would be impossible. This can be achieved just in case the PP is not a daughter of VP (i.e., is ungoverned by V). Otherwise, this condition is not met:

(2) a. ?Who did you hear about John's interest in?
   b. *Who did you hear about the city's destruction by?
(3) a. Who did you hear John was interested in?
   b. ?Who did you hear the city was destroyed by?

If this analysis is correct, then C&H's argument is invalid, of course.
NOTES AND DISCUSSION

What is more important, however, is the tentative nature of the claim. Namely, the proposal made in my paper is one way of capturing the distinction between impaired and preserved prepositions in agrammatism, namely, by a configurational difference. It might, however, turn out not to be true. There have been alternative proposals for the grammatical characterization of the agrammatic impairment vis à vis prepositions. Rizzi (1985) has suggested that the correct distinction is thematic. That is, one should state that theta-assigning prepositions are spared, while those which are not are impaired. To be sure, this is pointed out in the paper under attack [see Grodzinsky (1986) footnote 11]. Be that as it may, the passive by is claimed to be spared under both generalizations, and given that this whole issue is secondary in importance, this is sufficient. In sum, the analysis of this phrase may be maintained as proposed; but even if another analysis is chosen, the generalization can still be captured, contrary to C&H’s contention.

Alternative Theoretical Frameworks

C&H observe correctly that GB is not the only available theory of language structure. If so, they ask, why were other frameworks not considered? In particular, they note that in the theory of Lexical Functional Grammar (LFG) (Bresnan, 1982), the generalization over the structures considered cannot be maintained.

These observations are correct, yet what follows from them is exactly the opposite of what C&H would like to argue. First of all, as I said before, a descriptive generalization need not be unique. Given that it is just an organized list of facts, there might be more than one way to do it. Second, the fact that a generalization is not statable in a theory is precisely the reason not to consider that theory as an appropriate descriptive procedure. In fact, in Grodzinsky (1984, 1985a) I have argued that this contrast between these two theories (statability of the agrammatic data) is a reason to prefer GB over LFG: This is exactly how the Breakdown Compatibility Constraint applies. What follows, then, is that GB is appropriate for the description of aphasic deficits, and that LFG is not. In other words, if a description of some data cannot not be accommodated within a theory, then, assuming the correctness of the facts, what should be rejected is the theory and not the description. This conclusion is, again, contrary to C&H’s contention.

The Default Principle

In Grodzinsky (1986), this principle is brought up to force the chance performance where it is observed. Here is how it works: In agrammatism, every transformationally moved NP will not bind a trace, because the trace left by the movement is assumed to be deleted by the trace-deletion hypothesis. Therefore, it will not have a thematic role assigned to it, because thematic roles are assigned to moved NPs via the traces they
bind. The Default Principle ensures that some thematic role will be assigned despite the deficiency. It does so by associating canonical positions in a sentence to specific thematic roles. For example, a subject of a passive (whose trace in object position was deleted) would be assigned an AGENT role, because the subject position is clause-initial and canonically associated with this role in English. This is how chance performance is predicted in this case: There will be two AGENTS in the agrammatic thematic representation: One AGENT assigned via the byphrase (the oblique object) and one AGENT assigned by the Default Principle. This representation forces the patient to guess, and the result is (the observed) chance performance.

C&H have several complaints: First, they contend that the principle is unclear, in that it does not state which theta-role is assigned by default. A look at the original claim would reveal the contrary, however. It is explicitly stated that "the clause-initial position in a language like English would on this account have the role of AGENT as its default value."

Second, they contend that "the Default Principle would not apply to the head NP of a relative clause at all, because this NP has already been assigned a thematic role by the matrix verb or VP." This criticism would be correct were this NP to receive the Default theta-role. This is how the situation is described in Grodzinsky (1986), where matters are considerably simplified for readers who are not familiar with the GB theory. The precise details are slightly different. As the criticism is raised, let me state things in full. It will be shown that my original assumptions are correct.

In the object relative clauses in question, the theta-role of THEME is transmitted from the trace to the operator in COMP that binds it. This operator, being an NP, is effectively the clause-initial NP. Given that in agrammatism the trace was deleted, and that therefore the operator has no theta-role assigned to it, it now falls under the scope of the Default Principle. So, the head NP (which stands in predication relation to the relative), and the theta-roles assigned to it do not interact with the Default Principle at all. What is determined by this principle is the thematic identity of the operator in COMP. This is how we get the difference between subject and object relatives: In the former, the operator receives by default the same theta-role that it would have gotten had the trace not been deleted, namely AGENT. In object relatives, it is AGENT again, as opposed to the THEME role it would have gotten normally. The agrammatic performance is correctly predicted. This account is, for all practical purposes, identical to the one presented in the paper under discussion. The criticism C&H raise is thus justified in that the description given in that paper is incomplete, for simplicity reasons. Here I have given the details that demonstrate the correctness of my assumptions.

Finally, the Default Principle is claimed to have an ad hoc status. If so, C&H ask, why is it different from the assumptions made by Caplan
and Futter (1986), criticized in Grodzinsky (1986) for this reason? The difference is that unlike Caplan and Futter, I provide independent motivation for the Default Principle: It is one of the most widely assumed cognitively based strategies invoked for the explanation of the linguistic behavior of children and adults. See Bever (1970), Slobin and Bever (1982), Hakuta (1981) and many others.

**Conflicting Findings**

This is the most puzzling of C&H's criticisms. What they say is that the trace-deletion hypothesis does not account for all the data at hand. Specifically, they claim that S.P., the patient reported in Caplan and Futter (1986) performed above chance on object-object relatives, contrary to the prediction the hypothesis makes.

First, this putative finding (whose correctness is addressed below) conflicts with the finding reported in Grodzinsky (1984). There, four agrammatic aphasics performed at chance level on object-object relatives. There was, however, one difference between the two studies: My study consisted of sentences with one main verb and two NPs, compared with the two-verb and three NP constructions that S.P. was presented with in the Caplan and Futter study. S.P. presumably showed better performance (although see the discussion below). What could account for this difference? It is quite unlikely that the addition of a verb and a noun phrase made it easier for S.P. to interpret these sentences. Therefore, given that four patients were tested in the Grodzinsky (1984) study, compared to a single patient in the Caplan and Futter report, this could hardly count as a "counter example" to the description. But much more importantly, let us examine the Caplan and Futter finding itself. Their patient was presented with sentences like (4):

(4) The frog chased the monkey that the bear bumped

The patient was supposed to assign thematic roles to NPs, as evidenced by an acting out task she performed with toy animals. She gave the correct interpretation in six of nine trials. In a binary choice design, this is obviously at chance level. However, given the particular task, and since there are three NPs and two verbs in this construction, there are arguably more than two possible ways to err. So, an analysis that would take all the possible errors into account might yield an above-chance result. However, even if this result can be obtained, such a statistical analysis is inappropriate in the case of the object-object relatives. This construction has two distinct parts: The matrix clause *(the frog chased the monkey)* and a relativized NP *(the monkey that the bear bumped)*. According to the trace-deletion hypothesis, only the relative clause should be problematic, because its analysis involves a transformational operation, hence a trace. The matrix clause should cause no problem at all to the patient (and indeed, it did not for S.P.). Statistical analyses are forever
meaningless unless they are theoretically motivated. Let us, then, examine the result from the point of view of the trace-deletion hypothesis, since the finding in question is claimed by C&H to be a "refutation" of this hypothesis. From this point of view, errors on the matrix clause should never occur, since no transformation is involved. Indeed, no such errors were observed. Given this theoretical prediction, matrix-clause errors should not even be considered as possible responses for statistical purposes. The only optional responses for the patient are those that involve the NPs in the relative (i.e., *the monkey* and *the bear*). That is, any possible assignment of these two is among the predicted responses. Consequently, the error space is reduced to a binary choice, that considers misassignments of thematic roles only to these two NPs (which is indeed the only error type that the patient made). If this statistical analysis is carried out, then the patient's performance is clearly at chance level. To conclude, the statistical analysis that Caplan and Futter presumably performed is misleading. It leads to the false conclusion that "S.P. interpreted object–object relatives correctly." Obviously, she did not. It is exactly for these reasons that sentences involving more than one verb and two NPs were explicitly excluded from my discussion of this patient's performance (see footnote 12 in Grodzinsky, 1986).

In conclusion, there is nothing in C&H's claims that would lead to either rejection or modification of the account proposed in Grodzinsky (1986).

**REPLY TO SPROAT**

Sprat (1986) makes an interesting point. He observes that the account proposed in Grodzinsky (1986) is agnostic with respect to the source of impairment in agrammatism. That is, even though some possibilities are considered, no real claim is made regarding the antecedents of the representational deficit. Rather, both a processing deficit and a knowledge deficit are taken to be the possible sources of the observed impairment as captured by the characterization. Assuming the characterization as proposed, Sprat then considers both possibilities, and concludes that the deficit could not be a knowledge deficit, but could only stem from a disrupted processor. His reasoning (the details of which I leave aside now) is that if the deficit involved a loss of grammatical principles, then it would be much wider than what is actually observed. Hence it must be a processing deficit. He then proceeds to identify the deficit with a component of the Marcus (1980) parser.

I tend to agree with Sprat's claims, yet with several reservations. First, when the details of his argument are examined closely, it turns out to be much less conclusive than he would like it to be. So, even though the overall structure of the argument is quite appealing, its conclusiveness is hampered by the particulars. These are discussed below.
The second point is conceptual: It is not clear that the identification of the source of impairment in some language deficit is inherently interesting. Namely, it is not clear that such identifications might help us gain insights into the language faculty. Let me elaborate.

Consider Sproat’s discussion of the Projection Principle and its violation in agrammatic aphasia. He points out, correctly, that the deletion of traces from S-structure representation would imply a violation of this principle because it requires subcategorization properties to be observed at all syntactic levels. If the deficient representations violate the Projection Principle, Sproat argues, and if the deficit were a knowledge deficit that involves the “disappearance” of this principle from core grammar, then we would expect to encounter other violations of it by agrammatic aphasic patients. But in fact, such violations are not observed. On the contrary—violations of it were promptly detected in the Linebarger, Schwartz, and Saffran (1983) grammaticality judgment experiment. Hence, the deficit could not possibly have a knowledge source.

As I said, I think that this form of argument is correct (in fact, I have made a similar point in Grodzinsky, 1985b). But there are problems. Specifically, the argument Sproat provides might exclude a deficit involving the Projection Principle, but not a knowledge deficit that stems from another source. Consider, for example, a deficit that stems from a loss of knowledge of Binding Theory (or some part of it). This would result in the observed violations, but would also predict an inability to detect violations of antecedent-anaphor relations of the type Linebarger et al. actually tested, where the patients’ performance was poor. Thus, there could be a knowledge deficit that involves aspects of core grammar other than the Projection Principle. In this case, a deficiency with respect to Binding Theory (see Borer & Wexler, in press, for a similar claim regarding children’s comprehension of the passive construction). So, the argument that Sproat makes against a knowledge deficit is inconclusive.

Now to the conceptual point. Cognitive science may be interested in cognitive deficits only to the extent that they can provide insights into the properties of mental structure. The method by which this should be done, in my view, has been outlined above. An identification such as Sproat proposes (of the source of the deficit as a component of the parser) is merely a translation of the descriptive generalization proposed in Grodzinsky (1986) into parsing terms. Constraints on the class of biologically feasible theories of language structure come directly from my original statement, and need no such translations. Arguably, however, there are properties of parsers that are independent of the grammar they recover, and thus, for every grammar there is a class of parsing models. Since these could use neurologically based constraints just like their grammars, statements such as the one Sproat makes are necessary. Yet unfortunately, there are very few parsing models around. In fact, for
every grammatical theory there is at most one such parsing model, to
the best of my knowledge. As a consequence, the value of Sproat’s
claims is rather limited.

A CONCLUDING REMARK

Linguistic descriptions of language deficits are important, as they may
lead to neurologically based constraints on normal models of language
structure and processing. In Grodzinsky (1986), the paper under consider-
ation, such a description is proposed, that accounts for a subset of the
known agrammatic disabilities. In this note I have argued with criticisms
raised against the characterization I proposed, but also described how
such characterizations can be used for the imposition of constraints on
linguistic theory and the theory of human parsing. The characterization
I propose need not, in principle, be the ultimate description of the agram-
matic limitation, nor does it have to be its center. There are still lots of
things to be discovered, described, and debated. Yet there is nothing in
the arguments presented by the critics here that would lead to any changes.

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