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When Is a Grammatical Disorder a Disorder of Grammar?*

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Let me start concretely by introducing you to the attempts of two Broca's aphasics to tell the story of Little Red Riding Hood. These patients are typical of the syndrome in the nature of their errors; one is fairly mild, one fairly severe, as these things go. It's important to remember that these transcripts, even with their stage directions (see examples) don't do justice to these patients as intelligent people who are fighting to deploy their limited language production resources to tell this story which they both know quite well.

Example 1: mild Broca's aphasic "Mr. Franklin", Cross-Language Aphasia Study.

> Little Red Riding Hood . go . to . see...grand.ma . house. (pause) The wolf was . watching. Uh ... The Wolf . says . where're you . going. An...Little Red Riding Hood . says . I'm going . to see...granma house. An...the wolf . said: "Oh." (chuckles). So . he a - the wolf - hurrying - running. (pause) [To examiner: Hold it (gestures that tape should be turned off. Examiner complies. After a further pause of half a minute or so, Patient indicates that taping may resume).]
>
> The uh - the wolf . is running to . Little Red . Riding Hood . granma. So. He . comes in. An'...he ties up . uh . granma. [soft aside to E: Vaguely. (makes extended-hand 'mezza-mezza' gesture of equivocation). Chuckles slightly.]
>
> An' then - he - uh -took . gran-granma - /han...hanət . han - hanət/-that - bonnet. And hopped in.to bed. An'...then the uh . knock . on the door - uh - in - (pause) So - uh (sigh) Little Red Riding Hood . knocks . on the . door...out-outside. An'the . wolf said: Come in. The uh . /rejd / uh - Red - Riding . Hood comes in. /rejd / - Red - Riding Hood says: My, uh - you . ears is . larger? Something like that? (The questioning tone and question accompany a glance at E., apparently to see if this version of the well-known phrase is good enough. E. nods.)
>
> The wolf ah - er - says: Better hearing. Then . uh . nuh - the uh Little Red Riding Hood . says: My, your . nose is . larger. And uh . wolf says . uh . better uh. smell. An' . (sigh) Little Red Riding Hood says: Your teeth . are . larger. And the . uh - better . eat you . all up. And - I just - I forgot really . ending.

Just to enumerate the output characteristics of Broca's aphasia that jump out of the examples: Mr. Franklin omits the 3rd singular /z/ on 'goes', the possessive /z/ on 'grandma's house' (assuming that's the target), and on 'grandma's hat/bonnet'. There is heavy use of the -ing form of verbs (I say it that way because there is considerable debate over which of the several linguistic categories that this form represents is the one it should be assigned to), and some omission of the copula, as in "The wolf hurrying - running'; self-corrected, in this case.

Less obvious, perhaps, is the reliance on agent-action-object/complement sentence structure even when it is not optimal for the narrative: notice the aborted sentence that begins "An' then the knock on the door...", which is then reformulated, with a sigh, as "Little Red Riding Hood knocks on the door outside." We also have the repeated semantic violation in 'go to see grandma house', where it is, of course, grandma and not her house which is to be seen. This is possibly a matter of conflating several familiar acceptable sequences (go to see grandma, go to visit grandma's house, go to visit grandma) - as frequently happens, we can't really reconstruct the target with certainty or even be sure that there was a well-defined syntactically-encoded target.

Example 2: Moderate/severe Broca's aphasic, "Ms. G", CSU Northridge Speech Clinic. (Patient has just heard E. tell the story.)

> Ridin' hood. Grandma made it and this (gestures to her clothing) - and - happy, very happy and um okay goodies, goodies - okay, okay...walking...and... wolf. And um...eat'em up and a walk...very...walk. Knock knock and um... grandma...What ears you have. Okay and Grandma...eyes we have. Beat 'em up.

The poverty of the resources of the second patient, Ms. G. (see example 2), is really overwhelming. We get a few words, a few phrases, a few minimal 3-word sentences, and the repeated 'okay, okay' to hold the floor and reassure the listener, when she's stopped to find some way to express something about an episode.

Other general characteristics that we find in most (but not all) of the languages in which this syndrome has been studied include: reduction in use of verbs, use of wrong person, number, and other agreement markers, absence of markers of subordination, and reliance on direct discourse--this list being given in a form which attempts that impossibility, a theory-neutral description.

I've presented you with aspects of the syndrome of Broca's aphasia; if you've been following recent debates, you might wonder why, when I've been part of the attack on the unity of this syndrome. But the classical syndromes, with their hodge-podge of names, do exist, do represent recurring correspondences between left-hemisphere lesion sites and complexes of language disturbance. They have to be explained, even if only about 40% of aphasic patients can be classified using them (Goodglass' estimate). And Broca's aphasia has been the focus of a great deal of psycholinguistic/neurolinguistic work, for some very good reasons--the most theoretically important one being the continued failure of attempts to blame this disorder on failure of articulation, or of any general-purpose cognitive mechanism such as memory or attention. The patients give clear evidence of trying to say something about a defined topic, of being dissatisfied with their attempts, of attempting to self-correct--this is, in short, very clearly a *language* disorder. And a very lively debate, which I will review in this paper, has been taking place over the last 10 years or so concerning the nature of this disorder. The review will be necessarily only a swift skimming of some major papers; my focus will be on the kinds of hypotheses that have been advanced, and especially on whether authors have considered this language disorder to be a disorder of performance or of competence.

The place to begin looking at psycholinguistic research in aphasia is the series of papers written by Zurif and Caramazza and their associates, beginning in 1972 and summarized in Zurif & Caramazza, 1976. They upset a great deal of accepted thinking, especially the neat categorization, enshrined in some of the syndrome labels that we don't use where I come from (i.e., the labels of 'expressive aphasia' v.s. 'receptive aphasia'), that Broca's aphasia was purely a disorder of production and involved no impairment of comprehension. Sophisticated clinicians--especially those who were German-trained or who were acquainted with the German literature--certainly already knew that at least some Broca's aphasics had comprehension problems, although the nature of these was not very clearly described. But the general doctrine of "no comprehension impairment" in Broca's aphasia was widely-enough accepted that it fostered Weigl and Bierwisch's 1970 suggestion that brain damage in general disrupts only performance and leaves competence intact--this given the generally-shared assumption that competence is unitary and so therefore neutral among modalities such as auditory input, oral output, various metalinguistic tasks, etc.

The strongest version of this view was generally attacked as being unverifiable, and therefore philosophically unacceptable--this being the version in which language competence is essentially ineradicable in the living brian. For example, there are global aphasics who show

neither comprehension nor production of language; to say that their competence is intact but that all their performance modalities destroyed is to make an untestable claim.

But a modified form of the question persists: in a given syndrome, should we speak only of impaired performance, or of the impairment of both competence and performance? Philosophically, this formulation is still suspect at the level of verifiability; since we can always claim that there are multiple performance defects, we never need to invoke competence defects, as my colleague Akio Kamio, among others, has argued (ms.; pers. comm). But heuristically, which is where it really counts, the notion of 'competence deficit' seems defensible by a parsimony argument, to the extent that there are, in some patients, similar deficits that cut across all testable language modalities. So, a large part of the research action has been to find out whether such deficits really exist, in order to argue for the existence of competence disorders and thence for the neuropsychological reality of competence. Even if across-the-board deficits exist, however, one has the option of speaking, as Zurif does now, of 'central processing disorders'--in other words, the option of using performance terms, a move which it will become clear that I endorse.

The Zurif, Caramazza, *et al.* series of papers, then, considered a variety of performances. One was the von Stockert judgment task: patients are asked to sort sets of three cards, each bearing a written word from a sentence which has just been read aloud, by grouping together those two of the three words that 'go best together', and this is repeated for all triads of words from each test sentence. What Zurif and Caramazza found was that function words were more-or-less ignored, and ignored relatively more if the semantic load they carried was lighter. For example, articles and infinitive-marking 'to' were repeatedly set aside in the sorting, while pre-nominal 'to' and 'by' as markers of recipient and agent were grouped with their following nouns.

The point of this, in the words of the authors, was to see whether agrammatic/Broca's patients, freed from the real-time pressures of speaking and understanding, had (p. 263) "more information about permissible structures than would be expected from their speech". They concluded that "the Broca's aphasic retains a knowledge of the basic semantic roles noun-phrase constituents can assume even though...[they are] no longer fully able to process the determiners that mark such constituents."

Further experimental work--e.g., the series of papers by Diane Bradley, Merrill Garrett, and their associates at MIT, continued to implicate function word problems in Broca's aphasia; and clinically, papers by Goodglass and many others linked--and continued to link--these words with at least the inflectional morphemes as being impaired in output and sometimes in other tasks. (Evidence about derivational morphemes is still hard to come by.)

The next major thread in the story is Mary-Louise Kean's attempt to tie these two kinds of grammatical formative deficits together with Chomskyan theory of the middle 1970's, for in this theory they certainly did not form an obvious natural class. With a little readjustment, she was able to identify word-boundary-attached morphemes and function words at the phonological level, using a fairly complex statement involving bracketing by word-boundary markers; under the theoretical constraints that she set herself, it was the best available solution, though it necessarily left a lot of loose ends, like the semantic-load effect described above.

Kean's line of thought has continued to evolve; the most recent paper available to me is Kean & Garrett 1980) in which the phonological level is identified with Garrett's speech production model's 'positional level'--a late output level at which lexical words are represented in their eventual output order and are phonologically realized, but in which grammatical formatives of all sorts are still not phonologically spelled out.

This paper is also remarkable for postulating the strongest model of the relation between linguistic formulation and psycholinguistic production models that can conceivably be maintained

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anymore: linguistic levels are held to be in one-to-one correspondence with stages of processing. It is held, in other words, that there are no intermediate unmatched linguistics levels and no intermediate unmatched levels of processing; however, Kean and Garrett state that there is no resemblance between the linguistic rules that lead from one level to another and the psychological processes that lead from one stage to another--that possibility, of course, having been discounted a long time ago.

Kean's phonological formulation of the link between function words and--at any rate--the affixes we usually call inflectional is not necessary if one accepts a morphological linguistic level and puts both of these types of grammatical morphemes there, as Steve LaPointe does. But regardless of that, what we find is that in the Kean series of papers, morphological damage is sometimes spoken of in terms of 'knowledge', sometimes of 'processing', and repeatedly in terms of 'representation', which in the one-level-equals-one-stage formulation appears to be neatly ambiguous between the linguistic and the psychological terminologies.

Now, an entirely different research direction, appearing to have nothing to do with discussing Broca's aphasia in terms of affixes and function words, was taken about 1980 by Eleanor Saffran, Myrna Schwarz, and Oscar Marin in a series of articles. They found a number of Broca's aphasics who had trouble comprehending or using word order information even in simple active declarative sentences, and in locative and instrumental phrases (not trouble with the prepositions in these phrases, but with e.g. whether, in a sentence like "the suitcase is behind the chair", the corresponding picture should be one of a suitcase behind a chair or of a chair behind a suitcase). The patients tested by these authors tried to rely on an animacy strategy and similar heuristics for sentence interpretation and production: for example, an animate-first strategy seemed to block production of "the ball hit the boy" in describing a picture of such an event, and inability to construct the passive correctly, for whatever reason, left "the boy was hit by the ball" (the typical response of a normal English speaker to such a picture) unattainable and often reduced to the incorrect active "the boy hit the ball", or further to "boy hit ball".

Saffran and Schwartz therefore suggested that in addition to their function word problems, Broca's aphasics had problems with mapping semantic relations on to word order, and so had to rely on a battery of less-than-adequate semantic heuristics of order and mention.

David Caplan, as well as Schwarz and Saffran and their colleagues, has attempted to synthesize these findings with the function-word-based analysis of the deficit in Broca's aphasia, employing some sophisticated reanalysis which I will not attempt to review here, focusing on the key jobs done by function words in signalling many syntactic structures. Caplan also suggests that Broca's aphasics can manage only linear and not hierarchical node structures, and that perhaps they can only tell one node from another when the nodes bear distinct major category labels, e.g., N, A, V. He bases this on an experiment of his own in which 5 Broca's aphasics were able to distinguish, in an act-out task, among the following three sequences:

Can you show the woman washing the children?

Can you show the woman the washing children?

Can you show the woman the washing of the children?

Note that processing the instances of 'the' modifying 'washing' and 'children' is critical to distinguishing among these. This result he contrasts with one in the literature, Heilman and Scholes' 1976 result showing failure to sort out the contrasting possessive-object-patient relations also contrastively signalled by the position of the definite article in the pair.

Can you show her the baby pictures?

Can you show her baby the pictures?

Most recently, the whole-field--and especially the 'impaired competence' and 'impaired central processor' formulations [of Broca's aphasia] were given a real jolt when Marcia Linebarger joined forces with Schwartz and Saffran, devising a set of accept/reject grammaticality judgment tasks crucially dependent on the processing of function words, e.g., sets like

*the boy put the book

*the boy put the book that was in the kitchen

vs. the boy put the book in the kitchen

and *I hope you to go to the store now

*I want you will go to the store now

vs. I hope you will go to the store now

I want you to go to the store now

--and so on and so on, a very impressive list of sentences probing eleven types of grammatical violations. They gave this test to four agrammatic patients whose comprehension of function words, by the usual picture-pointing tests, was seriously impaired--and they found incredibly high accuracy of grammatical judgments in 8 or 9 of the 11 categories tested; reflexive and tag question gender/number agreements got the low marks, for reasons that are not yet fully elucidated.

So it's clear that many Broca's aphasics can "compute the syntactic consequences of the closed-class vocabulary" (Zurif & Caramazza formulation--or some of those consequences, in spite of deficits in comprehending sentences in which those words signal meaning contrasts. Linebarger et al., and also Caplan, suggest several possible resolutions, involving separating syntactic parsing (considered to be relatively spared) from semantic interpretation of the results of the parse (considered to be relatively impaired). There are various ways of hypothesizing processing models to do this, but there are also serious problems--defining the syntax/semantics boundary involved, for starters.

But those problems aside, consider what this means for 'competence'. I can't do better than to quote Caplan (1985):

> A strong possibility is that many processing mechanisms have as their sole function access and conversion of particular linguistic representations, and that the disturbance in agrammatism represents a disorder of mechanism specifically concerned with particular linguistic representations.... In this case, the ontological status of 'competence' would have to be seen as a more abstract level of unconscious knowledge, related to task-specific representations as well as task-specific processes.

Competence, in other words, is no longer cutting across modalities nor unified within a modality, and the motivation for assigning 'it' the status of a separate neurologically-represented component 'prior to' or 'utilized by' processing is correspondingly weakened. So: we have to resolve a dilemma. Neither linguists nor psychologuists can do without grammars. All of us

absolutely must deal with a notion of knowledge of language that is modality-independent; for that, we can't be encumbered with some putative operational definition dependent on a labfull of psycholinguistic tests that we barely know how to interpret and processing mechanisms whose specification is still a recipe for pie in the sky. We must have some notion like what we're used to denoting by 'competence' because we can't describe languages without it. But how shall we arrive at a philosophically defensible notion, given the way neurolinguistics keeps making it more and more elusive?

I want to argue for the move that is becoming more and more explicit in work by Sheila Blumstein and by Harold Goodglass. One usually says that competence is speakers' tacit knowledge of language used in speaking, hearing, etc. Competence is said to be 'abstracted' or 'idealized' from performance in the sense that all errors due to fatigue, distraction, slips of the tongue, memory limitations, i.e., all ephemeral impediments and general cognitive impairments to comprehension or production, are eliminated as artifacts, just as the physicist must eliminate friction in devising an elementary theory of motion.

We also have noted the following usual assumption: Competence, as stored knowledge of the language, is 'called upon' in processing a sentence; it is directly represented in the brain prior to and independent of performance.

I argue that this usual assumption, this reification of competence, is a conceptual error. We cannot do without the notion of competence as abstracted from performance--but we have been using an unexamined and inappropriate notion of the verb 'to abstract'. Plato aside, we can abstract without reifying--which is of course what physicists and mathematicians do. Mathematical triangles, ideal pulleys, etc. do not exist in the real world 'prior to' real triangles or real block & tackle sets. We can't do mathematics and physics without these ideals and we can't do linguistics without ideal "internalized grammars"--but these are not the same kind of objects as real ones.

Closer to home: one could abstract mathematical postulates from the operations of a calculator or a grammar from the operations of parser, but you can't generally open up such a machine and find Peano postulates or rules stored in an independent representation--instead, they are implemented directly in the construction of the device. There would be no way to damage the grammar without damaging the parser, any more than you could damage the time-telling of a watch without damaging the watch. (And could you damage the parser without damaging the grammar? Well, the grammar is an abstraction of some aspects of the output patterns of the parser; it would depend on the nature of the damage to the parser. Perhaps the damage might let it work properly under certain conditions, say if the damage only concerned the speed and the reliability under competing processing loads.)

To those who have argued in roughly this sort of way already and those who are used to thinking computationally about linguistics, this is all very obvious as at least a possible move to make; but it may not be plain--or even reasonable--to many others, judging by what I read. Let me elaborate on how it works, taking as a starting point a question raised by Susan Fischer (in conversation). Suppose you have a person with no brain damage who doesn't know a certain language very well, for example a second-language learner or a semispeaker (in the sense of Dorian 1978: one who comprehends almost everything, but who can say almost nothing beyond formulae). If such a person doesn't know the language very well, then surely their competence in that language is less than the competence of the normal native speaker. They don't know all the rules, all the words, etc. Now if one wants to conceptualize the speaker's competence as neither more nor less than an abstraction of the speaker's patterns of language performance, does that make sense for these speakers, who have mature undamaged brains? Do you want to say that since they surely have competence (knowledge) deficits, they therefore have performance deficits? Doesn't this proposed revision of the notion of competence, in other words, imply a counter-intuitive companion revision of the notion of performance?

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But what exactly is wrong with claiming that normals who don't know a language as well as native speakers have a performance problem? Obviously, they have different patterns of performance; and if not using the same patterns as native speakers is a problem--and it certainly is, for low-level and mid-level L2 speakers--then why don't we generally feel comfortable with calling it a performance problem? For two reasons, I think. One is that we already have a notion of what performance problems are supposed to be like: stuttering, word-finding difficulty, forgetting what one intended to say, slips of the tongue. L2 speakers aren't like that: their word-finding problems due to not knowing the right words in the first place, their disfluencies due to being unable to come up with a grammatical construction adequate to their meaning (my acquaintance with data from basilang and mesolang L2 speakers is courtesy of John Schumann). In short, and this is the fact the second of the two reasons for disliking the changed conceptualization, their problem is lack of knowledge. Surely it is totally misleading to call that a performance problem? Well, it is startling, I grant you. But these speakers are not performing like native speakers.

This forces us to think through again the claim being made here: that what speakers have in their heads in a group of language storing/processing mechanisms--that knowledge of grammar is a kind of 'knowing how', as some of Chomsky's old critics claimed (and see also Ryle 1949). What we memorize in language classes or by conscious metalinguistic reflection is a kind of knowledge 'that'--and we all know subjectively the different between memorizing a rule and internalizing it so that we can use it on-line. In the first case we have metalinguistic knowledge, in the second we have competence with the rule--equivalently, as I see it, in the first case we know about it and in the second case we know how to use it.

Linguistics has been in an epistemological rut that no other modern science has gotten into: we've been looking for abstractions as if they were real objects, looking for frictionless pulleys in the real world. I think we should stop.

**This paper was prepared while Lise Menn was working at the Aphasia Research Center of the Boston University School of Medicine and given at the 1984 LSA/AAAL Meeting in a symposium on clinical linguistics organized by Jacqueline Schachter.*

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Wichita Text Structure*

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In this paper I want to examine the structural and performance characteristics of two Wichita texts (appended). They are both narrated by the same person, Mrs. Bertha Provost, late of Anadarko, Oklahoma; they are both what would have to be called "traditional narratives," and they are both about the same length, but they represent two different genres. I want to look at the meanings of elements in the texts which seem to signal event boundaries and special types of language, and to describe some rather striking differences between the two examples here, differences which must be due solely to the genre, which in turn colors the speaker's attitude toward the subject matter.

The first text represents what the Wichitas call a "true story." The characters are human beings who act very much the way ordinary Wichitas act; and from the point of view of the Wichitas, the events are normal happenings. Stories of this sort can be told any time, and they seem to have little reason for existence beyond the intrinsic interest in the story itself. The second text, in contrast, is a Coyote story--and a very common one all over the Plains. Mrs. Provost called these "fairy tales" (*ksi:rʔo:kha:rʔa*). The characters are usually talking animals, and the ordinary laws of physics and biology are often suspended in these tales. They cannot be told during the summer months, and they always have some explicitly stated reason for existence--in this case, as the explanation for why coyotes cry.

Perhaps it is not surprising, then, that the first narrative is told in a very unemotional, matter-of-fact way, almost as a broadcast journalist might present it, while the second seems designed to entertain, and includes a lot of variation in its phonology. To illustrate, I have prepared some excerpts from each text for you to listen to.

Before we start, you need a few words of caution about the recordings vs. the transcriptions. The transcription you have before you is of slow speech forms, and it is also the result of sometimes rather drastic editing by the speaker as we went over and over the original tape. On the tape, however, you have fast speech forms, hesitations, repetitions, errors like stammering or slips of the tongue, and repairs--in short, a normal oral performance. That means that you will not see what you hear part of the time, particularly as regards grammatical (rather than intonational) pitch, word boundaries, reduction of VwV sequences to [o:], and the allophony of [r] and [n]. I will try to help you associate the right parts of the transcription with the tape as we go along.

The first class of phenomena we will examine is phonological, specifically the use of pitch level, speed changes, and pauses. First we will hear lines 1-13 of the true story text. Note that except for the grammatically conditioned pitch changes and for the kind of pitch drop that apparently universally precedes a pause, the passage is quite intonationless. Note, too, how few pauses there are. In particular, in line 7, note that there is almost no pause at all before the word meaning 'suddenly'. This is surprising, for we will find in the next text that that word regularly marks boundaries between sections of the story.

The text goes on to describe how everyone left, but the woman did not rejoin the group. A year later they have returned to the same place, but still not found her. Then toward sunset someone takes his horses out of the camp to graze; he spots the woman they had left behind, but sees that there is something wrong. Now we will hear lines 24-26.