Linguistic Approaches to Agrammatism

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Abstract

This article discusses linguistic approaches to agrammatism. First, we consider the possible contributions that the study of agrammatism can make to an understanding of the ways a brain computes language. To understand how a brain computes language, we minimally need to know something about language and something about language processing so that we can pose coherent questions to a brain. We consider in some detail how one well-known model of agrammatism accommodates what we know about language and language processing and find that the basic challenges have not been met. We compare this to a developing range of inquiry into agrammatism that is potentially more useful in moving the larger project forward.

Introduction

Agrammatism is a somewhat general term that is used to refer to an aphasic production deficit that results in slow, effortful speech in which functional categories, such as tense markers, while they might make an occasional appearance, are often missing or wrongly substituted. This characterization is traditionally associated with Broca's aphasia, as the following exchange with a patient suggests:

Patient: Farmer, uh, Ohio, farm, soy beans and corn, uh, 400 acres, yeah, miles and miles and miles, uh 32 years ago, me, supervisor
Q: Do you still farm?
Patient: No, well, uh, lawnmower (laughs).
Q: Your children, what do they do?
Patient: Uh, uh, Harry, uh, Minnesota, uh, Minneapolis, and uh, Indians, dead, oh, many, 200, 300, 400, 500, dead.
Q: What does Harry do with Indians?
Patient: Grounds, uh, Indian grounds.
Q: A reservation?
Patient: No, uh, die, uh, goddammit.
Q: Oh, burial grounds?
Patient: Yeah! Yeah!
Q: Harry works in burial grounds. What job does he do?
Patient: Uh, uh, oh boy, arrow, uh, Indian pieces.
Q: He looks for these things?
Patient: Yeah! Yeah!
Q: So he's an archaeologist?
Patient: Yeah! Yeah!

This extract also supports another traditional observation, namely, that Broca's aphasic patients have preserved comprehension. As Broca himself noted a century and a half ago, they operate with full intelligence; they hear and understand everything that is addressed to them perfectly (1861).

Inquiry into unimpaired sentence production lags behind inquiry into unimpaired sentence comprehension; so agrammatic comprehension if it were not in fact perfectly intact, would have the greater potential to make a useful contribution to an understanding of normal parsing. As it turned out, a landmark study in the 1970s (Caramazza and Zurif, 1976) revealed that Broca's aphasics could not understand certain types of sentences once their 'full intelligence,' including world knowledge, conversational context, and so forth, was rendered irrelevant and they were forced to rely on their knowledge of sentence structure to determine who was doing what to whom. On a sentence-picture matching task (in which they point to one of two or three pictures that best matches a sentence they have just heard), they could understand a passive sentence, such as *the ball was kicked by the boy*, because they know that boys can kick balls but not the reverse, but if the sentence was reversible, such as *the girl was chased by the boy*, they could not tell who was doing the chasing. In semantically reversible sentences, they could understand actives, but not passives; and they could understand subject relative clauses but not object relative clauses.

Even if this asymmetric agrammatic comprehension had been recognized in Broca's time, he would not have been able to say why such a pattern should have occurred. The problem could not be addressed in Broca's time because there was no theory available that could make sense of the observations. In fact, Broca was acutely aware of this: "the biggest obstacle hindering progress in this part of physiology [cerebral localization] results from the insufficiency and uncertainty of the functional analysis, which must precede the search for identifying organs for each function. Science is so behind in this respect, that it has not even found its foundation yet …" (1861/2006, p. 294). Broca did not envisage that this need for theory applied to language; he considered a notion such as 'spoken language' to be a neatly circumscribed, self-evident function not requiring any explanation. It was other faculties of mind that were in need of theory. However, if we take up Broca's realization that we cannot relate brain states to some function unless we know something about that function, and if we assume – surely uncontroversially – that the only relevant knowledge is theoretical, then we might expect to benefit from attention to a theory of language in seeking to understand the nature of Broca's aphasia. We might also expect to benefit from an appreciation of how language conspires with other faculties involved in comprehension, such as memory, that is, a theory of sentence processing.

Fortunately, we have an increasingly good appreciation of the factors that affect sentence processing and we have an increasingly good understanding of the nature of the human language faculty. In light of this, how might studies agrammatic comprehension contribute to what is known about how a brain processes the sentences it is confronted with? What has been accomplished to date? What challenges lie ahead? Let us address each of these questions in turn.

 http://dx.doi.org/10.1016/B978-0-08-097086-8.54021-5

What Contribution Can Inquiry into Agrammatic Comprehension Make?

To answer this question, it is useful to step back and view the issue from a broader perspective and ask what the study of brain and language, that is, neurolinguistics could contribute. If we had no theory of language, then we would be reduced to posing pretheoretical, and therefore incoherent, questions to the brain. But at least since Chomsky (1981), understanding and evidence have reached a point where we can refer to a theory of language. Similarly, the interplay of hypothesis and experiment in neurobiology, at least since the work of Hodgkin and Huxley (1952a, b, c, d, e), has attained a level of explanation that can reasonably be referred to as a theory of brain. The problem is that our best theory of language and our best theory of brain do not have a single property in common. It is what Chomsky (1995, 2000) has called a theory unification problem, a standard kind of problem throughout the history of science, but that is not to minimize the challenge. Unifying our theories of language and brain currently seems hopelessly remote.

We know how we could find out more. We could proceed invasively, as is routinely done in inquiries into brain function with nonhuman animals. Single cells in precisely identified regions of normally functioning intact brains are recorded to see if they responded to specific stimuli. To complement such findings, a whole range of techniques for inactivating those regions are deployed. For example, in avian song research, tissue from a target region might simply be ablated; or toxins introduced to block sodium channels, thus preventing cells from firing. Or, an antagonist to a particular neurotransmitter might be injected into the region, which has the effect of blockading receptors for that neurotransmitter, but leaving the receptors for other neurotransmitters unaffected. Or, brain regions might be systematically cooled, which has the effect of slowing function, allowing localization of behaviors with structures on many timescales, such as the motif, syllabic, and subsyllabic structures of zebra finch song (Long and Fee, 2008). The range, precision, and discriminability of the techniques available to research on intact and impaired brains in research on birds have yielded a great deal of knowledge in a very short period of time.

But for obvious reasons, they are not available for research on humans; there can be no direct electrical stimulation, no ablation of tissue, no severing of fibers, no single-cell recording, no pharmacological interventions to temporarily disable either pre- or postsynaptic neurons, and no cooling to analyze complex temporal dynamics. There can be no research at the cellular or molecular levels at all (except adventitiously, and in a very limited way, when patients have electrodes implanted for clinical reasons prior to surgery, for example, for intractable epilepsy).

So, the problem that confronts us is twofold: first, current theories of language and brain share not a single property; and second, language function, unlike, say, vision, in core respects is not available in other species, so we cannot avail ourselves of the invasive techniques that would allow inquiry at cellular and molecular levels.

With regard to the second problem, we have had to be more subtle. The tools at our disposal are the various noninvasive imaging technologies with which we attempt to identify regions involved in language function, or the timing of those functions, in living brains; and opportunistic use is made of naturally occurring brain damage that affects language function in some way, that is, in aphasia.

Addressing the first problem, the remoteness of language – brain theory-unification, it is a little less obvious how to proceed. One approach would be to let linguists and neurobiologists get on with the job or developing better and better theories and return in 50 years to see if some means of unification presents itself. And for a satisfying resolution on a par with Pauling's unification of chemistry and physics, we may have to wait for one theory or the other to undergo a parallel to the quantum revolution. But resolution might be arrived at differently. One possible approach is to target intermediate levels of unification, and the most obvious is for neurolinguists to deploy not only linguistic theory but also whatever understanding has been gained in psycholinguistics. Seeking to integrate what we know about the nature of language and what we know about language processing is major challenge but one that is at least realistic in the medium term; indeed many researchers have been actively investigating the relation. Precisely how to envisage the relation varies widely among individuals, and it is beyond the scope of this article to discuss the issues involved, but see, for example, Phillips (2012) and Phillips and Lewis (2013) for relevant discussion. For current purposes, it is sufficient to note that the relation between linguistics and psycholinguistics is firmly on the agenda, as it is in this respect that syntactic approaches to agrammatism may have more to offer in the coming years.

What Has Been Accomplished to Date?

In what follows, I will examine just one model of agrammatic comprehension. It is worth examining one model in some depth rather than cursorily describing several (and there are many) because much can be learned from the ways in which it has failed conceptually and empirically and where progress may be made. We will see that it runs into serious empirical problems, but more interestingly, we will see that it avoids the constraints of linguistic theory and uses theory-free strategies to accommodate anomalies. In addition, we will see that it does not set itself up in relation to normal, unimpaired processing. Seeing these limitations clearly ought to make it easier to see an alternative approach to agrammatic comprehension data.

The Trace-Deletion Hypothesis

In discussing many current models of agrammatic comprehension, we must first confront an epiphenomenon. Consider the following three facts:

1. In the world's languages, subjects typically precede objects.
2. Subjects are typically assigned thematic roles that are higher in thematic hierarchies than the thematic roles assigned to objects.

3. Agents (the do*ers* of an action) typically occur before Themes (the do*ees* of an action).

The canonical order of thematic roles is a statistical fact. It does not follow from any principle in linguistic theory. It is, that is to say, merely epiphenomenal, as far as linguistic theory is concerned. However, many models of agrammatic comprehension have been based squarely on the epiphenomenon of linearity, among them, Avrutin (2006), Caplan and Futter (1986), Linebarger (1995), and Piñango (2000). The guiding notion is that Broca's aphasics resort to a strategy in which thematic roles are assigned in a linear order. The Agent role, the highest role in all thematic hierarchies, is assigned to the first relevant noun phrase (NP) in a sentence. A role that is lower in a thematic hierarchy is assigned to a second relevant NP, and so on.

This reliance on linearity is true also of the trace-deletion hypothesis, henceforth TDH (Grodzinsky 1986, 2000).

The TDH invoked a concept from linguistic theory, the concept of syntactic movement and trace. In the 1980s, when the TDH was first formulated, very few researchers working with aphasic data were inclined to regard linguistic theory as relevant; so, viewed against that atheoretical background, the TDH, in opting to construct a hypothesis based on something theoretical, amounted to a trail-blazing proposal.

An early formulation of the TDH (Grodzinsky, 1986) proposed that in an otherwise normal representation, traces in theta positions (i.e., positions to which thematic roles such as Agent or Theme are assigned) are deleted. Since the trace is lost, thematic role information is also lost in NPs that have undergone movement. Broca's aphasics are apparently unable to infer the missing role by consulting their knowledge of the roles that the relevant verb requires. Instead, and this is where the reliance on linearity asserts itself, they resort to a nonlinguistic default strategy which assigns a role to the moved NP according to its serial position in the surface string (e.g., in a sentence with two NPs, if the verb is *bite*, and if the first NP in the surface string is the one that has moved, the strategy assigns it the role of Agent; if the second NP in the surface string is the one that has moved, the strategy assigns it the role of Theme; and so forth).

How does this play out in a passive sentence such as (1) which many Broca's aphasics tend to guess at, when the task is sentence-picture matching? What happens in the unimpaired representation is that the NP argument of the *by*-phrase (*the baker*) receives the Agent theta role, but not via movement. The subject (*the zebra*), by contrast, is derived by movement, and so is linked to a trace. In (1), the normal role assignments are given:

(1) [The zebra]$_i$ was bitten t_i by [the baker]
THEME AGENT

Because no movement is involved in the assignment of the Agent role to *the baker*, it is not linked to a trace, and the prediction is that Broca's aphasics will have no difficulty with that assignment; they get it for free via the syntax, which is by hypothesis unimpaired except with respect to movement. The NP (*the zebra*) is derived by movement, but since the trace it is linked to is deleted in a Broca's representation, thematic role assignment cannot be accomplished syntactically. The parser outputs an incomplete representation and its job is done.

Enter the default strategy. It assigns the role of Agent to the moved NP. The result is that the Broca's aphasic has a representation of the passive that contains two Agents, as in (2), where '*' indicates loss of trace:

(2) [The zebra] was bitten* by [the baker]
AGENT (via default) AGENT (via syntax)

When asked to complete a sentence-picture matching task, the representation in (2) compels Broca's aphasics to guess which NP to interpret as Agent. Thus, their performance is random. Consequently, in the passive, we see that random performance is accomplished by means of trace deletion, the application of a linear default heuristic, and the resulting competition for Agenthood.

How is the frequently observed above-chance performance on active sentences by Broca's aphasics explained? If, as in earlier versions of syntactic theory, it is assumed that there is no movement in sentences such as (3), then the representation of a Broca's aphasic is complete and thus there is no reason for the linear default strategy to be triggered.

(3) [The zebra] bit [the baker]
AGENT (via syntax) THEME (via syntax)

Thus far, we have seen how the TDH explains the core findings that Broca's aphasics perform above chance on actives and randomly on passives. Now, we turn to the other core findings, above-chance performance on subject relatives in sentence-picture matching tasks, but random on object relatives. Let us see how the TDH accounts for these. With respect to object relatives, earlier versions of syntactic theory assumed only one movement, as in (4):

(4) Point to [the zebra]$_i$ Op$_i$ that [the baker] bit t_i
THEME AGENT

In (4), the movement of *the zebra* is from the object position, now occupied by the trace. In an aphasic representation, lacking trace, the default strategy assigns the Agent role to the first NP (*the zebra*), and normal assignment obtains for the subject, *the baker*, as in (5):

(5) Point to [the zebra] that [the baker] bit*
AGENT (via default) AGENT (via syntax)

As in the passive, there are two Agents. The aphasic guesses and random performance follows.

In the subject relative, the normal representation is as follows:

(6) Point to [the zebra]$_i$ Op$_i$ that t_i bit [the baker]
AGENT THEME

In a Broca's representation, in which trace is lost, the Agent role is assigned to the NP (*the zebra*) by the default strategy, which happens to be the correct assignment, compensating for the loss of trace, and yielding a normal interpretation:

(7) Point to [the zebra] that* bit [the baker]
AGENT THEME
(via default) (via syntax)

The above examples show how the TDH succeeded in partitioning the core data. At this point, note only that in the three examples in which a trace is involved, the application of the atheoretical default strategy is necessary for the data to be partitioned in the desired way. No default strategy, no division. The strategy is already doing much of the heavy lifting, but now we will see that it works overtime, changing freely, to keep pace with selected empirical anomalies and theoretical developments.

Here is an example of an empirical anomaly which prompted a change in the default strategy. A study by Hickok and Avrutin (1995, 1996) on *wh*-questions reported that Broca's aphasics performed above chance on *which-N* questions involving subject extraction, such as (8), but at chance on *which-N* questions involving object extraction, such as (9).

(8) [Which horse]$_i$ t$_i$ kicked [the giraffe]
AGENT THEME

(9) [Which horse]$_i$ did [the giraffe] kick t$_i$
THEME AGENT

In (10), application of the default assigns the Agent role to *which horse*, and thus aphasics perform in a manner similar to normal subjects, above chance. In (11), the default assigns the Agent role to *which horse*, but now both NPs in the sentence have the Agent role. Forced to guess, random behavior is assured in Broca's aphasics.

(10) [Which horse]* kicked [the giraffe]
AGENT THEME
(via default) (via syntax)

(11) [Which horse] did [the giraffe] kick*
AGENT AGENT
(via default) (via syntax)

So far, so good. However, Hickok and Avrutin also reported above-chance performance for *both* subject- *and* object-extracted *who* questions, (12) and (13).

(12) [Who]$_i$ t$_i$ chased [the horse]
AGENT THEME

(13) [Who]$_i$ did [the horse] chase t$_i$
THEME AGENT

The problem is with (13). According to the TDH, the default should assign the Agent role to the moved wh-phrase, but this would mean that both NPs are Agents, and therefore subjects should behave randomly, contrary to fact:

(14) [Who] did [the horse] chase*
AGENT AGENT
(via default) (via syntax)

Hickok and Avrutin (1995, 1996) account for the differences in *who* questions and *which-N* questions by appealing to Cinque's (1990) distinction. He proposed that *which-N* phrases are referential and yield one kind of chain, a binding chain, whereas *who* phrases are nonreferential and yield a different kind of chain, a government chain. So, there is a theoretical distinction between the two kinds of questions, consistent with Hickok and Avrutin's data; but inconsistent with the TDH.

To address the problem, Grodzinsky (1995, p. 46) elected to revise the strategy (which, being atheoretical, can change without constraint) by asserting that the default strategy applies only to referential NPs. Since it could be argued that *who* is nonreferential, the TDH does not apply.

Another theoretical development was about to test the flexibility of the default strategy like never before: the VP-internal subject hypothesis, henceforth VPI.

A consensus had formed that subjects are base generated within VP and undergo movement. An immediate consequence is that traces occur in virtually every sentence, including simple actives. Apply this to the TDH and traces are deleted in virtually every sentence Broca's aphasics hear. And yet, Broca's aphasics do not have problems understanding virtually every sentence they hear, so trace-deletion per se makes no predictions at all. More and more obviously, what plays the crucial role in distinguishing sentences Broca's aphasics understand from those they do not is the theory-free default strategy. In fact, only by virtue of the strategy does the TDH have any consequences.

The VPI not only exacerbated the conceptual difficulties faced by the TDH, but it multiplied its empirical problems exponentially, as we will now see. In a single-clause sentence with a transitive verb such as *chase* and two referential NPs {Agent; Theme}, the three logical possibilities for linear role assignment are as follows:

Option A	NP1 moved Agent assigned by default	NP2 not moved Correct role assigned normally
Option B	NP1 not moved Correct role assigned syntactically	NP2 moved Theme assigned by default
Option C	NP1 moved Agent assigned by default	NP2 moved Theme assigned by default

Instances of Option A are easy to find. One such example is the passive sentence in (2) above, repeated for convenience as (15):

NP1 moved NP2 not moved
(15) [The zebra] was chased* by [the baker]
AGENT AGENT
(via default) (via syntax)

An example of Option B is not so easy to find, but for Option C, there are examples in abundance, especially from languages in which word order is freer than it is in English. Even with the English sentences above, the TDH unravels. Consider the object relative clause in (4) and (5). There was only one movement prior to VPI, but now there are two, as shown in (16):

(16) Point to [the zebra]$_i$ Op$_i$ that [the baker] t$_j$ chased t$_i$
THEME AGENT

The core finding is that Broca's aphasics perform randomly on such sentences. When there was only one movement, the TDH could account for the data, as shown in (4) and (5). Now that there are considered to be two movements that result in two NPs that do not receive theta roles syntactically, but must acquire them both via the default strategy, what does the TDH predict? Application of the default strategy in a linear fashion to both NPs must give the following aphasic representation, as indicated in Option C above:

NP1 moved NP2 moved
(17) Point to [the zebra] that [the baker]* chased*
AGENT THEME
(via default) (via default)

In (17), since the TDH default strategy assigns roles by linear positions, the Agent role is assigned to the first NP (*the zebra*) and the Theme role to the second NP (*the baker*). Thus, subjects would be predicted to perform reliably *below chance*, as their role assignment is exactly the opposite of the unimpaired subject's role assignment (16). Since Broca's aphasics in fact perform at chance, this prediction is wrong.

The TDH now also makes the wrong prediction with the *which-N* question in (9), *Which zebra did the baker chase*. Prior to ISH, the TDH could arrive at the chance (random) prediction that is consistent with fact, but now that the subject NP (*the giraffe*) moves in addition to the wh-phrase, it would yield a below-chance prediction, contrary to fact, as shown in (18):

(18) [Which zebra] did [the baker]* chase*
AGENT THEME
(via default) (via default)

In (18), once again, since the TDH default strategy assigns roles by linear positions, the Agent role is assigned to the first NP (*which zebra*) and the Theme role to the second NP (*the baker*). Thus, subjects would be predicted to perform reliably *below chance* (their role assignment is exactly the opposite of the unimpaired subject's role assignment).

Another problem, for the end is not yet: The combination of VPI and the default strategy predicts anomalous results with any sentence type which does not assign an Agent role and which only involves a single dependency. In this respect, adjectival passives are of interest. Adjectival passives are derived in the lexicon, unlike verbal passives, which involve syntactic movement from object position (Levin and Rappaport, 1986). An example of the structure of an adjectival passive is given in (19). The negative prefix *un-*, which only attaches to adjectives, is often used as a test of an adjectival passive.

(19) The man$_i$ [was [$_{AP}$ t$_i$ unhurt] in the accident]

Because it is lexically derived, the adjectival passive has a structure involving a predicate-internal trace, as can be seen in (19). What is interesting here, however, is that adjectival passives assign a Theme role to their predicate-internal position.

With that in mind, consider the finding by Grodzinsky et al. (1991) that Broca's aphasics performed above chance on adjectival passives involving negative *un*-prefixation. In this study, the VPI was not assumed, and above-chance performance was predicted because no trace was thought to be involved. However, as we see in (19), adjectival passives with negative un-prefixation *do* involve movement and contain a trace. Application of the default strategy would assign the role of Agent, not Theme, to the moved subject of an adjectival un-passive, and therefore comprehension should be seriously impaired, which it is not.

The problems posed by the VPI with English sentences is challenging enough, but once we look at Broca's aphasia in languages with less restrictive word order than English, the problems mount up exponentially for the TDH and its linear strategy. We will consider only two examples here, but see Beretta (2001) for other examples.

Japanese scrambled actives (in which the object NP moves to the front), such as (20) yielded chance performance by Broca's aphasics in a study by Hagiwara and Caplan (1990).

(20) Hanako$_i$-o Taro$_k$-ga t$_k$ t$_i$ nagutta
Hanako-Acc Taro-Nom hit
'Taro hit Hanoko'

According the TDH, however, the prediction is for below-chance performance:

(21) [Hanoko]-o[Taro]-ga**nagutta
AGENT THEME
(via default)(via default)

In Korean actives (22), in which the object NP is dislocated to a sentence-initial position, Broca's aphasics perform at chance (Beretta et al., 2001).

(22) saja-lul$_i$ key-ka$_j$ t$_j$ t$_i$ mul-eyo
lion-Acc dog-Nom bite-COMP
'The dog bit the lion'

As with the Japanese case, the TDH wrongly predicts below-chance performance, as in (23):

(23) [saja]-lul [key]-ka** mul-eyo
AGENT THEME
(via default) (via default)

There are many other crosslinguistic data one could discuss here, but the moral of the story is by now abundantly clear. With the VPI and frequently *two* dependencies for the default strategy to deal with, data from languages with relatively free word order were always going to raise difficulties for the TDH and its commitment to a linear heuristic to do the work of partitioning data, and so it proved.

The TDH solution to the two-dependency problem posed by the VPI was to simply add another strategy. This new strategy, called *theta-bridging* (Grodzinsky, 2006), in effect absolves Broca's aphasics of any constraints imposed by the linguistic theory, in this case, the VPI. By stipulation, they do not have to attend to the dependency involved in subject movement out of VP.

But by now, we are very far indeed from any commitment to theoretical relevance.

What Challenges Lie Ahead?

If we can agree that the ultimate goal of neurolinguistic inquiry is theory unification, then muzzling linguistic theory by shackling it to a theory-free strategy is inimical to that goal. The linear models do not entirely dismiss linguistic theory, but they have set out their stall in such a way that they fail to make a single prediction without the effective agency of a linear strategy. The strategy, in all of the linear models, is designed to compete with syntactic processes in the sentences Broca's aphasics do not understand, but not to compete in those they do understand. However, nothing constraints strategies, and linearity of role assignment does not follow from anything in linguistic theory. And, absent a linear *deus ex machina*, there is simply no way that the linear accounts can divide the Broca's comprehension data in the required manner.

Linearity, we must conclude, is not a useful concept to invoke in inquiry into aphasic comprehension deficits, either empirically or conceptually. But independently of the arguments and evidence above that lead to this conclusion, there is another line of reasoning that lends support to it: relating language disorders to processing in the normal, unimpaired population.

Many commentators have observed that Broca's aphasics retain much, conceivably all, of their syntactic knowledge. After all, it has been reported that they can understand not just simple actives, subject relatives, but also clitic-doubling actives, subject clefts, raising constructions, and adjectival passives, among many other constructions (e.g., Avrutin, 2006; Beretta et al., 1999, 2001; Grodzinsky et al., 1991). If structural knowledge is more or less intact in these cases, then surely structure-building processes are also largely preserved in Broca's comprehension. After all, it would be entirely implausible that Broca's aphasics could understand all of the structures that they do if they did not possess extensive resources for structural analysis. Assuming that they do in fact have such resources, we are confronted with the question as to where these resources come from. Parsimony suggests that they were always there, but now perhaps restricted following neural insult. If we wish to press this view into service, then we have no choice but to commit ourselves to grounding our approach to Broca's comprehension in relation to a model of normal comprehension.

Linear models are not sympathetic to this view. As Friederici and Gorrell have pointed out, the TDH "lacks any connection to theories of sentence comprehension in normals" (1998, p. 260), a point also made with some force by Mauner (1995, p. 364). There seems to be little motivation to propose and defend extralinguistic models of linear heuristics in the absence of any evidence that Broca's aphasics resort to processes that are unlike those used by the normal, unimpaired population.

In this connection, Lukatela et al. (1991) have proposed that we would do better to relate aphasic deficits to the normal case. They reported that both Broca's aphasics and Wernicke's aphasics exhibit an identical pattern of comprehension errors in relative clauses in Serbo-Croatian, and furthermore, they are both different from controls only in the quantity of errors they make, but not in the kind of errors they make. In a similar vein, considering experimental reports in language development, dyslexia, aphasia, and normal adults sentence processing, Crain et al. (2001, p. 294) conclude that, "the results of experimental investigations across several subject populations reveal parallel patterns of linguistic behavior on [relative clauses]". Crain et al. believe that a processing account that encompasses both disordered and normal sentence processing would do justice to the facts. Caplan and Waters (1999), considering the role of memory in sentence processing, observe that in "many experimental paradigms, the processing of syntactically more complex sentences is not disproportionately affected by a concurrent verbal memory load, either in normal subjects or in subjects with extremely reduced working memory capacity or in aphasic patients" (p. 92).

If we are to meet the challenge of relating disordered language processing to normal language processing, it will entail a more concerted effort to carry out online rather than offline studies. In an excellent review of real-time comprehension in agrammatic aphasia, Schumacher (2009) presents a compelling case for using online techniques. There has been a steady tradition of this approach in studies of agrammatism (e.g., Blumstein et al., 1998; Burkhardt et al., 2008; Haarman and Kolk, 1994; Love et al., 2008; Prather et al., 1997), though these are more the exception than the rule.

To conclude, in order to rescue many current models from a parochial dependence on atheoretical concepts, the major challenge that lies ahead is to view agrammatic comprehension as complementary to normal processing, and to pursue online inquiry rather than offline. There are already efforts in that direction and it would be surprising if this were not to become the norm.

See also: Aphasia; Control, Syntactic; Functional Brain Imaging of Language processes; Language Processing, Functional Magnetic Resonance Imaging of; Neurolinguistic Processing of Psychological Verbs; Neurological Approaches to Agrammatism; Speech Errors, Psychology of; Speech Processing, The Cortical Organization of; Speech Production, Psychology of; Syntax (General).

Bibliography

Avrutin, S., 2006. Weak syntax. In: Grodzinsky, Y., Amunts, K. (Eds.), Broca's Region. Oxford University Press, Oxford, pp. 49–62.

Beretta, A., 2001. Linear and structural accounts of theta-role assignment in agrammatic aphasia. Aphasiology 15, 515–531.

Beretta, A., Piñango, M., Patterson, K., Harford, C., 1999. Recruiting comparative crosslinguistic evidence to address competing accounts of agrammatic aphasia. Brain and Language 67, 149–168.

Beretta, A., Schmitt, C., Halliwell, J., Munn, A., Cuetos, F., Kim, S., 2001. The effects of scrambling on Spanish and Korean agrammatic interpretation: why linear models fail and structural models survive. Brain and Language 79, 407–425.

Blumstein, S.E., Byma, G., Kurowski, K., Hourihan, J., Brown, T., Hutchinson, A., 1998. On-line processing of filler-gap constructions in aphasia. Brain and Language 61, 149–168.

Broca, P., 1861. Remarques sur le siège de la faculté du langage articulé suivie d'une observation d'aphémie (perte de la parole). Bulletin de la Société d'Anatomie (Paris), 2e serie 36, 330–357.
Burkhardt, P., Avrutin, S., Piñango, M.M., Rulgendijk, E., 2008. Slower-than-normal syntactic processing in agrammatic Broca's aphasia: evidence from Dutch. Journal of Neurolinguistics 21, 120–137.
Caplan, D., Futter, C., 1986. Assignment of thematic roles by an agrammatic aphasic patient. Brain and Language 27, 117–135.
Caplan, D., Waters, G.S., 1999. Verbal working memory and sentence comprehension. Behavioral and Brain Sciences 22, 77–126.
Caramazza, A., Zurif, E.B., 1976. Dissociation of algorithmic and heuristic processes in language comprehension: evidence from aphasia. Brain and Language 3, 572–582.
Chomsky, N., 1981. Lectures on Government and Binding. Foris, Dordrecht.
Chomsky, N., 1995. Language and nature. Mind 104, 1–61.
Chomsky, N., 2000. Linguistics and brain science. In: Marantz, A., Miyashita, Y., O'Neill, W. (Eds.), Image, Language, Brain. MIT Press, Cambridge, MA, pp. 13–28.
Cinque, G., 1990. Types of A' dependencies. MIT Press, Cambridge, MA.
Crain, S., Ni, W., Shankweiler, D., 2001. Grammatism. Brain and Language 77, 294–304.
Friederici, A.D., Gorrell, P., 1998. Structural prominence and agrammatic theta-role assignment: a reconsideration of linear strategies. Brain and Language 65, 252–275.
Grodzinsky, Y., 1986. Cognitive deficits, their proper description, and its theoretical relevance. Brain and Language 27, 178–191.
Grodzinsky, Y., 1995. A restrictive theory of agrammatic comprehension. Brain and Language 50, 27–51.
Grodzinsky, Y., 2000. The neurology of syntax: language use without Broca's area. Behavioral and Brain Sciences 23, 1–71.
Grodzinsky, Y., 2006. A blueprint for a brain map of syntax. In: Grodzinsky, Y., Amunts, K. (Eds.), Broca's Region. Oxford University Press, Oxford, pp. 83–107.
Grodzinsky, Y., Pierce, A., Marakovitz, A., 1991. Neuropsychological reasons for a transformational analysis of verbal passive. Natural Language and Linguistic Theory 9, 431–453.
Haarman, H.J., Kolk, H.H.J., 1994. On-line sensitivity to subject-verb agreement violations in Broca's aphasics: the role of syntactic complexity and time. Brain and Language 46, 493–516.
Hagiwara, H., Caplan, D., 1990. Syntactic comprehension in Japanese aphasics: effects of category and thematic role order. Brain and Language 38, 159–170.
Hickok, G., Avrutin, S., 1995. Representation, referentiality, and processing in agrammatic comprehension: two case studies. Brain and Language 50, 10–26.
Hickok, G., Avrutin, S., 1996. Comprehension of *wh*-questions in two Broca's aphasics. Brain and Language 52, 314–327.
Hodgkin, A.L., Huxley, A.F., 1952a. Measurement of current-voltage relations in the membrane of the giant axon of Loligo. Journal of Physiology 116, 424–448.
Hodgkin, A.L., Huxley, A.F., 1952b. Currents carried by sodium and potassium ions through the membrane of the giant axon of Loligo. Journal of Physiology 116, 449–472.
Hodgkin, A.L., Huxley, A.F., 1952c. The components of membrane conductance in the giant axon of Loligo. Journal of Physiology 116, 473–496.
Hodgkin, A.L., Huxley, A.F., 1952d. The dual effect of membrane potential on sodium conductance in the giant axon of Loligo. Journal of Physiology 116, 497–506.
Hodgkin, A.L., Huxley, A.F., 1952e. A quantitative description of membrane current and its application to conduction and excitation in nerve. Journal of Physiology 117, 500–544.
Levin, B., Rappaport, M., 1986. The formation of adjectival passives. Linguistic Inquiry 17, 623–671.
Linebarger, M., 1995. Agrammatism as evidence about grammar. Brain and Language 50, 52–91.
Long, M.A., Fee, M.S., 2008. Using temperature to analyze temporal dynamics in the songbird motor pathway. Nature 456, 189–194.
Love, T., Swinney, D., Walenski, M., Zurif, E., 2008. How left inferior frontal cortex participates in syntactic processing: evidence from aphasia. Brain and Language 107, 203–219.
Lukatela, K., Shankweiler, D., Crain, S., 1991. Syntactic processing in agrammatic aphasia by speakers of a Slavic language. Brain and Language 49, 50–76.
Mauner, G., 1995. Examining he empirical and linguistic bases of current theories of agrammatism. Brain and Language 50, 339–368.
Phillips, C., 2012. Parser-grammar relations: we don't understand everything twice. In: Sanz, M., Laka, I., Tanenhaus, M. (Eds.), Language Down the Garden Path: The Cognitive and Biological Basis for Linguistic Structure. Oxford University Press, New York (Papers in honor of Thomas G. Bever).
Phillips, C., Lewis, S., 2013. Derivational order in syntax: evidence and architectural consequences. Studies in Linguistics 6, 11–47.
Piñango, M., 2000. Canonicity in Broca's sentence comprehension: the case of psychological verbs. In: Grodzinsky, Y., Shapiro, L., Swinney, D. (Eds.), Language and the Brain. Academic Press, New York.
Prather, P.A., Zurif, E., Love, T., Brownell, H., 1997. Speed of lexical activation in nonfluent Broca's aphasia and fluent Wernicke's aphasia. Brain and Language 59, 391–411.
Schumacher, P., 2009. Real-time comprehension in agrammatic aphasia. In: Ibanescu, G., Pescariu, S. (Eds.), Aphasia: Symptoms, Diagnosis and Treatment, Ch.4. Nova Science Publishers, Inc.