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The neurology of syntax: Language use without Broca's area

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Abstract: A new view of the functional role of the left anterior cortex in language use is proposed. The experimental record indicates that most human linguistic abilities are not localized in this region. In particular, most of syntax (long thought to be there) is not located in Broca's area and its vicinity (operculum, insula, and subjacent white matter). This cerebral region, implicated in Broca's aphasia, does have a role in syntactic processing, but a highly specific one: It is the neural home to receptive mechanisms involved in the computation of the relation between transformationally moved phrasal constituents and their extraction sites (in line with the Trace-Deletion Hypothesis). It is also involved in the construction of higher parts of the syntactic tree in speech production. By contrast, basic combinatorial capacities necessary for language processing – for example, structure-building operations, lexical insertion – are not supported by the neural tissue of this cerebral region, nor is lexical or combinatorial semantics.

The dense body of empirical evidence supporting this restrictive view comes mainly from several angles on lesion studies of syntax in agrammatic Broca's aphasia. Five empirical arguments are presented: experiments in sentence comprehension, cross-linguistic considerations (where aphasia findings from several language types are pooled and scrutinized comparatively), grammaticality and plausibility judgments, real-time processing of complex sentences, and rehabilitation. Also discussed are recent results from functional neuroimaging and from structured observations on speech production of Broca's aphasics.

Syntactic abilities are nonetheless distinct from other cognitive skills and are represented entirely and exclusively in the left cerebral hemisphere. Although more widespread in the left hemisphere than previously thought, they are clearly distinct from other human combinatorial and intellectual abilities. The neurological record (based on functional imaging, split-brain and right-hemisphere-damaged patients, as well as patients suffering from a breakdown of mathematical skills) indicates that language is a distinct, modularly organized neurological entity. Combinatorial aspects of the language faculty reside in the human left cerebral hemisphere, but only the transformational component (or algorithms that implement it in use) is located in and around Broca's area.

Keywords: agrammatism; aphasia; Broca's area; cerebral localization; dyscalculia; functional neuroanatomy; grammatical transformation; modularity; neuroimaging; syntax; trace deletion

0. Getting more precise

Advances in neuroimaging technology have increased our knowledge of the neuroanatomy of higher functions of the central nervous system: It is now possible to get a glimpse of the brain while it is in action. However, this progress would not have been possible without improved understanding of the knowledge base and operations that underlie complex behavior. Parallel to the greater precision of the technology, some progress has been made in our understanding of the cognitive architecture that underlies certain behavioral domains. In the study of brain-language relations, theoretical developments in linguistics have gone hand in hand with imaging, making a joint contribution to behavioral neurology. After several decades of the study of language and the brain from a linguistic angle, there is now a relatively dense body of facts that can be seriously evaluated. This target article will review central results and use them to motivate some novel conclusions about the representation of language in the human cerebral cortex. The discussion will revolve around the choice of unit of behavioral analysis and its theoretical import. An outlook on language derived from current linguistic theory can lead to a new and more precise picture of language and the brain.

The old Connectionist school – led by Broca, Wernicke, and Lichtheim (see Lichtheim 1885, for a comprehensive exposition) and revived in our time by the late Norman Geschwind (e.g., 1970; 1979) – fortified belief in the existence of cerebral language centers. As clinicians, these authors mostly emphasized the patients' communicative skills, viewing language as a collection of activities, practiced in the service of communication: speaking, listening, reading, writ-

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ing, naming, repetition, and so on. Their characterization of the language centers derived from this intuitive theory, and for each activity they posited a cerebral center. The resulting theory of localization uses these activities as building blocks and takes them as the essence of human linguistic capacity. This view, based mostly on aphasia studies, is still held, especially in clinical circles, as illustrated by current clinical manuals and textbooks (e.g., Bradley et al. 1996, p. 37). The leading neurology textbook in the United States explicitly espouses the teachings of the great neurologists of the nineteenth century, depicting the perisylvian region as the location of language and providing an internal division as follows (analytic categories of behavior are in italic):

> The conventional teaching is that there are three main language areas, situated, in most persons, in the left cerebral hemisphere. Two are receptive and one is executive. The two receptive areas are closely related and embrace what may be referred to as the central language zone. One, subserving *the perception of spoken language*, occupies the posterior-superior temporal area (the posterior portion of area 22) and Heschl's gyrus (areas 41 and 42); Wernicke's area comprises the posterior part of area 22 and the parietotemporal junction. A second area, subserving *the perception of written language*, occupies the angular convolution (area 39) in the inferior parietal lobule, anterior to the visual receptive areas. The supramarginal gyrus, which lies between these auditory and visual language "centers," and the inferior temporal region (area 37), just anterior to the visual association cortex, are probably part of the central language zone as well. Here are located the *integrative centers for cross-modal visual and auditory functions*. The third area situated at the posterior end of the inferior frontal convolution is referred to as Broca's area or Brodmann's area 44 and is concerned with *the motor aspects of speech*. The entire language zone is perisylvian, i.e., it borders the sylvian fissure. (Adams & Victor 1993, pp. 412–13)

Against this background, psycholinguists have, since the 1960s, attempted to devise a new perspective. As a first step, they challenged the old outlook regarding the centers, on the basis of theoretical and experimental tools borrowed from linguistics and psycholinguistics (e.g., Blumstein 1972; Caramazza & Zurif 1976; Gardner & Zurif 1975; Goodglass 1968; Goodglass & Berko 1960; Heilman & Scholes 1976; Zurif & Caramazza 1976; Zurif et al. 1972; 1974). Focusing on the distinction between linguistic levels of representation, these authors did not altogether deny the validity of the old approach. Rather, they took new issues to be central and used linguistic concepts, as well as new experimental techniques, to explore them: For them, language was no longer just a set of activities but a structure-dependent body of knowledge divided into levels of analysis, mainly phonological, syntactic, and semantic. A variety of experiments in the 1970s showed that this approach was worthwhile, providing surprising results and demonstrating that the brain made linguistic distinctions that could not be couched in the standard view. Consequently, an attempt was made to "redefine" the centers (Zurif 1980); the anatomical, center-based conception was retained, but each center was now said to contain devices used for the analysis and synthesis of language, rather than for activities. Roughly speaking, the anterior language area was taken to house syntax (harnessed in the service of both comprehension and production), and semantics was believed to reside posteriorly, in Wernicke's area. Neuroanatomy also witnessed parallel advances: It was becoming increasingly clear that the anterior language area is larger than previously supposed; by then, large samples of patients had become available, making comprehensive surveys possible. On this basis, the area subtending mechanisms implicated in Broca's aphasia was now taken to "encompass most of the operculum, insula, and subjacent white matter, exceeding Broca's area" (Mohr 1976, p. 202). The schema remained localizationist, and, although its units of analysis changed, the overall view of cerebral loci supporting linguistic behavior remained the same.

From the early 1980s onward, new series of studies began to emerge, drawing on still more finely grained functional distinctions and using new materials and methods to explore the language areas from a more detailed linguistic perspective (see, e.g., Bradley et al. 1980; Caplan & Futter 1986; Grodzinsky 1984a; 1984b; 1986; 1989; 1990; Kean 1980; Linebarger et al. 1983; Schwartz et al. 1980; Swinney et al. 1989). With neuroanatomical considerations remaining constant, the most natural move was to look more deeply into the details of the syntactic disruption in Broca's aphasia; syntax was a natural candidate, constituting a central combinatorial aspect of language, and Broca's aphasia was the pathology of choice because most researchers believed the area damaged in this syndrome to be the locus of syntax. Thus, although the view of the syndrome was changing, the traditional diagnostic schema was not abandoned because its usefulness was proved, time and again (but see sect. 2.6 for challenges to this view).

I will be using evidence obtained in these experiments, enhanced by more recent findings, to reexamine the "redefined centers" view. I will show that a new, highly abstract and precise approach is necessary, not only to accommodate the fine patterns of performance that emerge, but also to describe the cross-linguistic variation within the syndrome. This will draw a new picture of the cerebral representation of the language faculty; after three decades of this line of research, it appears that the ability to create and analyze meaningful expressions through rule-based combination is sharply distinguished from other seemingly related mental capacities (such as arithmetic or general intelligence). Mental modularity, moreover, is also a property of syntax itself; the neurology indicates that syntax is not supported by one piece of neural tissue. Within this picture, syntax is entirely represented in the left cerebral hemisphere, but for the most part it is not located in Broca's area. The evidence suggests that this cerebral region has a crucial, highly specific role: It is the neural home to mechanisms involved in the computation of transformational relations between moved phrasal constituents and their extraction sites.

This is a radical conclusion (at least when pitted against prevailing neurological traditions and beliefs). It is therefore important to emphasize that the evidence for it is entirely empirical, coming from a thorough survey of the aphasia literature, the literature on other cognitive impairments, and recent work in functional neuroimaging. Broca (together with a few generations of great neurologists) appears to have had the right intuitions, yet he was wrong in certain important respects that could not be understood in his day. The better understanding of the nature of language now available, coupled with improved experimental techniques, allows us to reexamine old claims. This target article thus begins with a tour through results obtained in neurolinguistic research that have brought about the change.

1. Cerebral loci for syntax

1.1. The neuroanatomy of syntax: Lesion- and imaging-based approaches

The movement to redefine the language centers has had its effect in certain circles; the neurolinguistic localizing schema of language perception might not have permeated the clinical literature, yet it is currently accepted in cognitive neuroscience. In this model, syntax is represented in the part of the left anterior cortex that receives its blood supply from the superior distribution of the left middle cerebral artery (i.e., Broca's area and its vicinity, as indicated above), whereas semantics and the lexicon are posterior, located temporoparietally around the Sylvian fissure (Alexander et al. 1990; Damasio & Damasio 1989; Zurif 1995). Based on an ever growing experimental record, it is now widely accepted that the speech production problem in Broca's aphasia implicates syntactic mechanisms of some sort (Damasio 1992; Goodglass 1976; 1993; Grodzinsky 1984a; Marshall 1986; Zurif 1995); it is also believed that, in comprehension, there is a deficit in receptive mechanisms of grammatical analysis (Damasio 1992; Goodglass 1993; Grodzinsky 1990; Zurif 1995). Wernicke's aphasia, by contrast (following a posterior lesion in and around Wernicke's area), is believed to involve the lexical and interpretive components of the language faculty (Damasio & Damasio 1992; Goodglass & Kaplan 1983; Zurif 1995). Crucially, although other pathological signs are found in the overall description of many, if not most, language disorders, they are not part of the model for brain-language relations, either because they come from nonlinguistic domains (e.g., anosognosia, nonfluency, dysarthria) or because they are not pathognomonic of any particular syndrome.

This neurolinguistic model was formed on the basis of studies of pathology, which related neuroanatomy to linguistic function by correlating impaired behavior with morphological lesion data (PM, CT, or MRI). With the advent of functional imaging, evidence for the same distinctions has been sought from normal language processing. Initial findings seem to corroborate the basic approach; functional imaging studies (PET and fMRI) have detected involvement of similar regions in syntactic processing (Bavelier et al. 1997; Bookheimer et al. 1993; Just et al. 1996; Mazoyer et al. 1993; Stromswold et al. 1996); electrophysiological studies (ERP) that chart electrical activity during the performance of syntactic tasks have likewise been consistent with this picture (Friederici 1995; Kluender & Kutas 1993; Münte et al. 1993; Neville et al. 1991). The movement to revise the picture from one of activity-based cerebral centers to centers representing different levels of linguistic analysis, then, has appeared to be on the right track and has thus become the prevailing view in neuroscience.

1.2. Contradictory results from aphasia

A careful examination of the experimental evidence, which includes the more recent results, leads to conclusions that are much less definite. The data even appear contradictory at times: Wernicke's aphasics have some disturbances in syntactic comprehension (Grodzinsky & Finkel 1998; Schwartz et al. 1987; Shapiro et al. 1993; Zurif & Caramazza 1976), whereas Broca's aphasia patients, though failing certain tasks that probe receptive syntactic abilities (Caramazza & Zurif 1976; Goodglass 1968), have shown success in others (Linebarger et al. 1983). Taken at face value, these findings cast serious doubts on the model, in which Broca's area (but not Wernicke's area) supports receptive syntactic mechanisms. However, with certain provisos, the neurological model can still be maintained. Wernicke's patients' failures in syntactic comprehension are rather inconsistent and varied (Shapiro et al. 1993; Zurif 1995a; Zurif & Caramazza 1976) and are by and large ignored. In the case of Broca's aphasia, attempts have been made to reconcile empirical contradictions: Syntactic abilities have been broken down into tasks, and it has been claimed that "syntactic comprehension is compromised" (Martin et al. 1989) and that "grammaticality judgment is intact" (Linebarger et al. 1983). Common to such analyses is a rather "holistic" approach to the functional deficit, details of cerebral localization being of more concern than linguistic questions. Gross distinctions between form and meaning seem sufficient, and hence less attention is paid to more detailed structural properties of linguistic stimuli. As a result, the neurological model of language has continued to prevail.

It is quite possible, however, that the apparent inconsistencies in the results discussed above occur because many analyses lump together complex grammatical systems without distinguishing between syntactic types that are used in experiments. Seeming experimental discrepancies may thus be reconciled after the structural properties of experimental stimuli are examined. If true, this possibility could lead to a new view of the functional role of the language areas. In particular, Broca's (and perhaps Wernicke's) aphasia may come to be seen as *selective* deficit in receptive grammatical (i.e., syntactic) mechanisms, affecting only subsystems of the syntax (in addition to overt problems in speech production). In this view, inconsistencies in experimental results are only apparent; they disappear once the right linguistic distinctions are made.

It is hard to overestimate the implications of these conclusions (if they are valid) for the neurological study of language; data on a partial syntactic deficit do more than lead to a refinement of our view of brain-language relations, they also call for an experimental linguistic approach, involving extensive and systematic use of large varieties of sentence types as test materials. Aphasia studies, which allow tests of the most fine-grained aspects of language and their neural representation, should play a central role in this kind of research program. A review of the current experimental record, and the conclusions it leads to, is thus in order. I will argue that the move from activity-based descriptions of brain-language relations to task- and linguistic-level-oriented ones is insufficient. The evidence suggests that the main language areas of the brain follow the particulars of linguistic theory. A precise account of the functional neuroanatomy of these regions must therefore be more specific linguistically.

2. A syntactic approach to Broca's area

2.1. Language comprehension without Broca's area: The centrality of syntactic movement

Broca's aphasia is best known for the nonfluent, "telegraphic" speech with which it is associated. The comprehension problem in this syndrome is less noticeable and is harder to detect. This is probably why Broca's aphasia was initially thought to be only a speech production problem (with obvious consequences for the diagnostic schema); not until the 1970s, when controlled experiments on comprehension began, did a deficit in this modality become apparent. Indeed, the standard diagnostic batteries, such as Goodglass and Kaplan's (1983), have not even acknowledged a receptive disorder in Broca's aphasia. These authors merely noted that, at the early stages, Broca's aphasics "may be confused by more complex spoken messages" (p. 55), yet, when experiments began, they revealed – much to the surprise of investigators – a disruption in syntactic comprehension. An experiment that is taken as a landmark (Caramazza & Zurif 1976) compared "semantically irreversible" and "reversible" object relative clauses, such as *the ball that the boy is kicking is red* and *the cat that the dog is chasing is black.* Whereas Broca's aphasics were successful in comprehending the former, using semantic cues to get around their deficit, they failed with the latter, indicating that they were unable to use the relevant syntax to get at the correct interpretation. This result clearly did not fit the old Connectionist model that distinguished production and comprehension anatomically, locating the former anteriorly in Broca's area and the latter in and around Wernicke's area.

This surprising finding, and others that ensued, triggered a theoretical attempt to put the newly discovered receptive problem on a par with its expressive counterpart: Because the speech production deficit was already thought to disrupt syntax, it was initially claimed that Broca's aphasics also suffered "asyntactic comprehension" (Caramazza & Zurif 1976; Zurif & Caramazza 1976), and that their deficit was parallel in both domains, "overarching" both production and comprehension (see Bradley et al. 1980 and Zurif 1980, for elaboration).

This claim shaped the debate in the early 1980s. The need to make more precise statements, and to make distinctions between linguistic types, was becoming apparent, and initial attempts were made to capture all aspects of the grammatical aberration in one descriptive statement that would, moreover, span all modalities. The belief in Broca's area and its surrounding tissue as the sole locus of syntax led to a hope that patterns of impairment and sparing in speaking, listening, reading, and writing would all fall under the same generalization. Thus, labor was invested in obtaining such a generalization, a unified deficit analysis of comprehension and production in agrammatism (Grodzinsky 1984a; 1984b; Zurif 1980; Zurif & Grodzinsky 1983).

It quickly turned out that this view was overly optimistic, however. The pattern of selective impairment was more intricate than this account could allow for, and comprehension had to be set apart from production. New experimental results were coming in, creating a rather dense body of data that was drawing a new picture in which the comprehension deficit seemed more restricted than previously supposed. On testing, Broca's aphasics showed near-normal abilities in comprehension and grammaticality judgment on many syntactic constructions, and thus did not appear to have "asyntactic comprehension." There was a disruption, but it was restricted to certain aspects of syntax. It was becoming clear, then, that a distinction between different levels of linguistic analysis would not suffice, and that distinctions *within* syntax were needed to account for the comprehension deficit, just as they were for speech production. Much of syntax, then, was intact in comprehension, as became evident through experiments that mostly required interpretation (and, as will be shown below, the production deficit also turned out to be more selective, though in a very different manner). The one clear exception, which had actually stood out since the beginning of the experimental investigations in the late 1960s, was transformational movement in the syntax, as indicated by marked comprehension deficiencies with structures derived by such operations (see Ansell & Flowers 1982, for early results; also see Caramazza & Zurif 1976; Goodglass 1968; and Schwartz et al. 1980). These basic findings have since been fortified by massive evidence, coming from different laboratories using diverse experimental techniques. For almost all these patients, anatomical and pathologic data are available; common to all is a positive diagnosis of Broca's aphasia on standardized test batteries (i.e., BDAE and WAB for English, AAT for German and Dutch) and a focal lesion to the left cerebral hemisphere, caused in the majority of cases by occlusion of the left middle cerebral artery. One repeated finding is clear: Patients who are diagnosed as Broca's aphasics do suffer a receptive disorder of syntax, but a highly restricted one. There is clear evidence for near-normal performance in most other domains of syntax, coupled with sharp failures with structures containing transformational operations.

We will now examine the experimental record in detail; it underscores the centrality of grammatical transformations in the comprehension deficit of Broca's aphasia and, as a consequence, delineates the functional role of Broca's area in language rather precisely. First, in comprehension, Broca's aphasics can construct basic syntactic trees (phrase structure) for simple sentences that do not contain intrasentential dependency relations, such as actives (see, e.g., Grodzinsky et al. 1999 for a review); they are also near normal in detecting violations of phrase structure rules (Grodzinsky & Finkel 1998; Linebarger et al. 1983). Second, the patients seem to have no impairment in their lexicon in comprehension; the part of the lexicon that interfaces with sentence grammar is intact. This is demonstrated by their ability to detect violations of subcategorization (Linebarger et al. 1983) and argument structure (Grodzinsky & Finkel 1998) and by the normal time course of their lexical processing when argument structure is at issue (Shapiro & Levine 1990; Shapiro et al. 1993). Third, when required to carry out tasks that involve thematic (ϑ-)assignment, they are successful. The ϑ-part of the grammar is concerned with thematic roles that a predicate assigns its arguments and the manner by which they are linked to positions in the sentence where these arguments are realized. Simply put, each position (i.e., Subject, Object) is associated in the lexical entry of a predicate with a ϑ-role (Agent, Theme, Goal, Source, and Experiencer), from which the semantics of this sentential position can be recovered (see Haegeman, 1991, for a review). In this domain, we know that Broca's aphasics have intact abilities. They know the ϑ-roles of predicates and are able to assign them directly to positions. This is evident from their normal performance in comprehension tasks that involve direct ϑ-role assignment in simple structures such as active sentences (see Grodzinsky, 1990, for an exposition). They also never violate constraints on the thematic structure of sentences (i.e., the ϑ-criterion) when they construct syntactic representations (Lapointe 1985). Fourth, these patients can even compute some (but not all) intrasentential dependencies and are able to detect violations of rules that govern them. One piece of evidence in this respect is their ability to detect case (Nominative, Accusative, Dative, and so on) on noun phrases (NPs). Mechanisms for case assignment are conditions that license the presence of NPs in their base positions. In certain languages, these conditions have overt consequences, through case markers that are realized phonetically. Broca's aphasics have virtually intact abilities in this domain, as can be seen from their performance in case marking languages, for example, Serbo-Croat (Crain et al. 1989; Lukatela et al. 1988). They can also represent other complex intrasentential dependencies that are defined over tree structures. Specifically, they can handle the formal aspects of binding relations, which are the conditions that define the anaphoric relations between pronouns (and reflexives) and their antecedents (Grodzinsky et al. 1993; see also Crain & Shankweiler 1985). Some relations between pronouns and reflexives and their antecedents are impaired, yet these have to do with discourse-related aspects of pronominal reference, not with syntax (see Grodzinsky & Reinhart 1993).

In sharp contrast with the above-mentioned evidence for intact receptive abilities, we see a rather severe deficit in the comprehension of constructions derived by transformational movement, as indicated by the following familiar pattern of results, taken from dozens of experiments that investigated aphasics' interpretive abilities. In such experiments, the patient typically listens to a ("semantically reversible") sentence that contains two argument NPs and is required either to select one picture among several (including a critical foil that depicts a reversal of the actors) or to make a truth-value judgment regarding the match between the sentence and a single picture (in which the roles are either correct or reversed; see Grodzinsky, 1990, for a review). This task is believed to probe the subjects' ability to assign ϑ-roles to positions where the manipulated variable is syntactic structure. Several tokens (10–20) are usually presented for each sentence type, allowing evaluation of the patients' performance relative to chance, or guessing:

Construction type	*Performance level*
(1) a. The girl pushed the boy	***above chance***
b. The girl who pushed the boy was tall	***above chance***
c. Show me the girl who pushed the boy	***above chance***
d. It is the girl who pushed the boy	***above chance***
e. *The boy was interested in the girl*	***above chance***
f. *The woman was uninspired by the man*	***above chance***
(2) a. *The boy was pushed by the girl*	***chance***
b. The boy who the girl pushed was tall	***chance***
c. Show me the boy who the girl pushed	***chance***
d. It is the boy who the girl pushed	***chance***
e. *The woman was unmasked by the man*	***chance***

This list is compiled from a large number of studies. For the basic active/passive contrast (1a)/(2a) there are results taken from 17 different studies, with at least 42 patients, all diagnosed positively as Broca's aphasics on standard tests; for the subject/object relatives (1b,c)/(2b,c) there are at least 4 studies of 16 patients (see Grodzinsky et al., 1999, for a detailed review); and 3 studies on 7 patients have documented the contrast between subject and object cleft sentences (1d)/(2d) (Ansell & Flowers 1982; Caplan & Futter 1986; Hickok & Avrutin 1996). The contrast between (1) and (2) does not manifest only in performance levels – it is also structural. As we shall see, the sentences in (2) are derived by a transformation in a way that hampers the patient's comprehension, whereas those in (1) are not. Note also that passive constructions feature in both (1) and (2), as emphasized by italics. We return to this issue later. At any rate, the contrast (1e)/(2e) was obtained in one study of five Broca's aphasic patients (Grodzinsky et al. 1991; see the end of sect. 2.3.1 below). Critically, the contrast between (1) and (2) cannot be explained by appeal to familiarity, or frequency; this type of account would have to show that (1e), for example, is less frequent than (2a) or that (1c) is more frequent than (2c). Such demonstrations are unimaginable. It thus appears that most aspects of syntax, whether pertaining to basic relations or to the more intricate dependencies, are intact in the comprehension of Broca's aphasics, with one salient exception: syntactic movement – grammatical transformations.

2.2. The trace-deletion hypothesis

A grammatical transformation is a complex operation over structural representations of sentences in natural language. Roughly speaking, it involves the copying of a constituent to another position in a sentence and the substitution of the material in the original position by a *trace* – a phonetically silent yet syntactically active category that plays several important functions, two of which are relevant here. First, it is through the link between the trace and its antecedent that ϑ-roles (which are always assigned to canonical positions) are transmitted indirectly. Uniformity considerations force verbs to assign their associated ϑ-roles in the same direction, regardless of the ordering of constituents around them. Hence, when a constituent is moved, it is through the link between it and the trace that its ϑ-role is transmitted. Thus, in (3a) the role of Theme (recipient of action) is assigned by the verb *like* to its right-hand constituent (=***t***), as it would be in an active sentence; the mutual index shared by the trace and its antecedent ***which man*** is the vehicle for ϑ-transmission. As a result, the latter becomes Theme:

(3) a. **[which man]**$_i$ did Mary like $\boldsymbol{t_i}$
b. *** [which man]**$_i$ did Mary spread the rumor that she liked $\boldsymbol{t_i}$

The mediating function of traces has major ramifications for the comprehension of structures with movement, which will be discussed and will become especially salient in cross-linguistic contexts. Second, constraints on movement are sometimes formulated as permissible relations between traces and their antecedents; the ungrammaticality of (3b) follows from an upper bound set on the (structural) distance between a trace and its antecedent, which was exceeded in this case. We shall see how this involvement of traces in the determination of grammaticality also impinges in important ways on the patients' metalinguistic skills. Note that the description of this syntactic relation is deliberately generic, in a manner just sufficient for present purposes. Linguistic theory evolves, at times even rapidly, and terms tend to change with theoretical perspective. Certain generalizations remain stable, however, because they deal with basic syntactic phenomena. The relationship between traces and their antecedents in movement operations plays a central role in a large class of theories. In the main, then, the presentation here is compatible with most current theoretical frameworks, including the Minimalist Program (Chomsky 1995).

Thus, movement operations are implicated in the comprehension deficit in Broca's aphasia. Their impairment is also manifest in the on-line processing of structures with movement: We will see that these are compromised, whereas other complex processes are intact. This disruption may suggest some preliminary clues to the underlying cause of the comprehension deficit in Broca's aphasia. Finally, we shall see how therapeutic methods whose theoretical underpinnings are like the above may be surprisingly successful.

One attempt to capture agrammatic comprehension used this general theoretical background to accommodate the main findings. The basic observation was that structures derived by movement – those in (2) – were poorly comprehended by the patients, whereas those without movement, (1), yielded normal comprehension performance on tasks requiring thematic (ϑ-)assignment (put simply, the determination of "who did what to whom"). An initial attempt was made to partition the two structural types (Grodzinsky 1984b; 1986). It was assumed that *in agrammatism in Broca's aphasia, all traces of movement are deleted from syntactic representations.* This claim has far-reaching empirical consequences for sentence interpretation, grammaticality judgment, and on-line processing in agrammatism. As we shall see, considerable empirical evidence gathered from all these tasks supports this claim, suggesting that the patients suffer a disruption to only part of their syntax.

What performances follow from trace deletion? What linguistic behaviors would a traceless language user exhibit? In interpretation, this would imply problems in movement-derived constructions and the preservation of all else; in judgment, it would predict that violations of grammaticality would go undetected if traces are crucially involved in the determination of grammaticality; and, in processing, it would predict real-time problems in linking antecedents to the positions they vacated. All three predictions are borne out. What remains is to examine the distribution of traces and inquire whether trace deletion cuts the experimental pie correctly. The demonstration will also tie this deficit to the anterior language areas, by linking this functional impairment to Broca's aphasia.

A first hint comes from an examination of the patients' performance on passive constructions. In (1) and (2), predicates with passive morphology feature (italicized) in both cells; they generate both guessing behavior (2a,e), and near-normal behavior (1e). This finding correlates perfectly with trace deletion; passive morphology does not necessarily imply a syntactic movement transformation. Certain passives – known as "lexical" (1e) – are base-generated, whereas others, "verbal" (2a,e), are derived by a transformation; hence, their representations contain a trace (see Grodzinsky et al. 1991). Trace deletion partitions these data correctly (movement⇔comprehension problem), yet this discussion glosses over important particulars. As we will see, there are still constructions that do contain movement but where the aphasics perform normally; there are also distinctions among types of erroneous performance that trace deletion cannot handle. In short, a more detailed analysis of both the syntax and the experimental tasks is in order.

Consider, first, interpretive tasks that require ϑ-role assignment, the basic data set in (1) and (2). If traces mediate the transmission of ϑ-roles (Agent, Patient, Experiencer, etc.) to moved constituents, then the result of trace deletion would be that moved NPs would lack a ϑ-role. This may provide a formal means of partitioning the data: Impaired structures contain traces, whereas the rest do not. However, mere partitioning is insufficient; although it points to the line dividing those structures that give the patients trouble from those that do not, the actual performance rates do not follow from just trace deletion; trace deletion may explain why a moved constituent does not have a ϑ-role, yet it does not imply chance performance on the passives or object relative clauses. On the above assumptions, the rest of the grammar is intact. This should give patients enough information to carry out a thematic assignment task and get around the deficit; they should be able to infer the missing ϑ-role from the rest of the available information (i.e., the ϑ-structure of the predicate, the fact that another NP is assigned a ϑ-role directly, and all other grammatical principles; see Grodzinsky 1990, Ch. 5; 1995a).

Another consideration that points to the inadequacy of trace deletion is internal to the linguistic framework. Current approaches to syntax assume traces in virtually every structural representation. Even in simple active-declarative sentences, there is movement of the subject from a (deep) position inside the verb phrase (VP) into the overt position of subject (see 4a, where the VP is inside boldface brackets). This is known as the VP–internal-subject hypothesis (Kitagawa 1986; Koopman & Sportiche 1988; Kuroda 1986): Subjects are base generated inside the VP and are forced to move up the tree. If true, this hypothesis may have the consequence that even actives can receive a ϑ-role indirectly. If traces are deleted, then subjects of active sentences will have no ϑ-role, because the trace, under this hypothesis, may be crucial for ϑ-transmission. Such sentences pose no comprehension problems to the aphasics, however. Trace deletion, then, is an insufficient account of the data. Something must be done to remedy this.

Moved constituents lack a ϑ-role for aphasics because of the deletion of the trace, yet every NP must have some role in interpretation. It was proposed that moved NPs are assigned a role by a nonlinguistic, linear default strategy, which in the cases discussed assigns the Agent role to traceless clause-initial NPs (this is somewhat reminiscent of Bever's 1970 influential proposal, and similar to Jaeggli's 1986 proposal for the objects of *by* phrases in derived nominals). The interaction of this strategy with the grammar results in compensation for certain structures but confusion for the cases that are performed at chance. For example, consider a schematic representation of the active taken from (1a) and the verbal passive (2a):

Normal assignment
Agent *Theme*
(4) a. [**The boy**]$_i$ [$_{VP}$ $\boldsymbol{t}_i$ pushed [the girl]] ***above chance***
Agent *Theme*
Agrammatic assignment

Normal assignment
Theme *Agent*
b. [**The boy**]$_i$ was [$_{VP}$ $\boldsymbol{t'}_i$ pushed $\boldsymbol{t}_i$] by [the girl] ***chance***
Agent *Agent*
Agrammatic assignment

In (4a), the subject normally receives the Agent role. It moves from the VP internal position, where it leaves a trace. The correct role is transmitted through the trace to the subject, which moves leftward. In Broca's aphasia, this transmission does not occur (trace deletion precludes ϑ-transmission), yet the default strategy assigns the subject the Agent role, which happens to be correct, and normal performance follows. In the case of passive (4b), the oblique object (the NP argument of the *by* phrase, *the girl*) gets the

Agent role. Crucially, no syntactic movement is involved in this part of the representation, so the agrammatic patient is able to assign it properly. By contrast, the subject of the passive, *the boy,* is movement derived in two steps, hence two traces, both linked to the subject: First, it moves out of its base object position (***t***) and lands in the VP-internal subject position; second, it raises from the latter position [Spec, VP] to its final place [Spec, IP] (see Burton & Grimshaw, 1992, for discussion). The link between these positions is the channel for ϑ-role transmission. In agrammatic Broca's aphasia, both traces are deleted, so no such channel is available, which means that the subject of a passive sentence receives no ϑ-role grammatically. At this stage, the strategy kicks in, assigning Agent to this NP. Thus we have a thematic representation with one Agent in the *by* phrase (assigned grammatically) and one in the subject (assigned strategically). This situation does not allow for a unique determination of the Agent of the action, and guessing follows, resulting in chance-level performance.

Next, compare the account for subject (5a) versus object (5b) relatives:

Normal assignment
Agent *Theme*
(5) a. [**The boy**]$_i$ who [t'_i [$_{VP}$ t_i pushed the girl]] was tall
Agent *Theme* ***above chance***
Agrammatic assignment

Normal assignment
Theme *Agent*
b. [**The boy**]$_i$ who [the girl [$_{VP}$ t'_i pushed t_i] was tall
Agent *Agent* ***chance***
Agrammatic assignment

In (5a), the VP-internal trace (t_i)is linked to a trace in subject position (t'_i), which is in turn linked to the head of the relative clause (*the boy*). The traces are deleted in Broca's aphasia, and the relative head lacks a ϑ-role. However, the strategy assigns it Agent, which is precisely what it would have received under normal circumstances. The cognitive strategy thus compensates fully for the deficit here – and in other cases in (1) – and the observed above-chance performance follows. In (5b), by contrast, the traces (t_i) and (t'_i) are also deleted and the strategy applies, yet here it gives an undesirable result: There are two Agents in the representation, and the result is chance performance.

In sum, the performance rates of Broca's aphasics on the comprehension of all the above constructions is deduced by assuming trace deletion and a strategy. This combined claim has become known as the Trace-Deletion Hypothesis (TDH) whose current formulation, to be precise, is restricted to traces of constituent movement, see Grodzinsky 1995a. The performance of the patients is deduced through either thematic *competition* or *compensation:* The strategy always assigns an Agent label to clause-initial NPs. Thus, if a moved constituent is linked to a different ϑ-role normally (as in passive, object-gap relatives, object clefts, and so on), this constituent now becomes Agent; and, because there is another, grammatically assigned Agent in the thematic representation, the two Agents compete, thereby inducing chance performance by agrammatics. In cases where the moved NP was supposed to be Agent (such as subject-gap relatives, subject clefts, or actives under the VP-internal subjects hypothesis), this role is not assigned normally through the trace owing to trace deletion, yet the strategy correctly compensates by assigning that NP the Agent role by default.

Direct evidence for the validity of this strategy comes from an experiment with "psychological" verbs (Grodzinsky 1995b). When required to assign ϑ-roles to sentences containing such verbs, four Broca's aphasics (all suffering focal lesions as a result of left middle cerebral artery infarction) performed *below* chance (i.e., they reversed ϑ-roles systematically, pointing to the *wrong* picture most of the time) on passives of psychological predicates such as in (6) (even though they performed normally on their active counterparts):

Normal assignment
Theme *Experiencer*
(6) [**The girl**]$_i$ was t'_i admired t_i by [the boy] ***below chance***
Agent *Experiencer*
Agrammatic assignment

This contrasts sharply with the chance-level performance of patients on movement-derived structures with agentive predicates. What is different about these verbs (*admire, adore, love, hate,* etc.) is their thematic structure – their subject is not Agent, but Experiencer (Belletti & Rizzi 1988; Pesetsky 1995). This means that the object of the *by* phrase in the passive is Experiencer. The interaction of the default strategy with the rest of the representation is different from the other cases of passive, because the nature of the competition in the representation that is created – seen in (6) – differs sharply from the previous passive cases; it is not a competition among equals anymore (i.e., Agent vs. Agent) but, rather, between ϑ-roles with different semantic properties. The surprising contrast between agentive and psychological passive thus follows, strengthening the theoretical account.

This deficit analysis may or may not turn out to be correct in its particulars, in the long run, but it underscores two observations that hold true for a large population of Broca's aphasic patients with damage to the left anterior language areas:

1. Syntax is mostly spared in their comprehension.
2. Syntactic movement, and precisely that, is disrupted along the lines of the TDH:

A. Traces are deleted from Broca's aphasics' syntactic representations.

B. Phrasal constituents with no ϑ-role are assigned one by default, by linear considerations (NP_1 = Agent).

These TDH-based observations force a new view of the role of these cerebral areas: Broca's area and its vicinity (operculum, insula, and subjacent white matter) support receptive language mechanisms that implement some, but not all, aspects of syntax, namely, those pertaining to syntactic movement rules in comprehension (as well as limited aspects of tree building in speech production). Crucially, the basic combinatorial capacities necessary for sentence processing – structure-building operations, lexical insertion, and so on – are not supported by the neural tissue of these cerebral regions. This means that there is very little language in the anterior "language" area. In the next few sections, this conclusion will be fortified with evidence from several experimental angles.

2.3. Comprehension across languages

The TDH handles the comprehension of English-speaking Broca's aphasics, yet questions immediately arise concerning the manifestation of this disease in other languages. At birth, the brain is presumably the same across future speakers of different languages. However, are universal grammatical principles, once cashed in as particular grammars of Arabic, Navajo, or Thai, represented in adult neural tissue in the same fashion? Does the deletion of traces have identical consequences across languages? Could specific (perhaps parametrically defined) properties of a particular language correlate with the way the deficit is manifested, as they do in speech production? Is the default strategy universal? All these questions call for a comparative approach to agrammatic comprehension, for which we have some early results. The idea, then, is to probe the syntactic abilities of patients who suffer lesions to the same cerebral loci, and have a diagnosis of Broca's aphasia, yet who speak languages whose structural properties differ in ways that are relevant to the deficit described by the TDH.

Comparative aphasiology must begin with a choice of dimension along which one compares. Languages, after all, vary along many dimensions (as described, for example, by parametric theories of grammatical variation). A reasonable place to start is in basic word order. This is a property with several manifestations; languages may manifest SVO (subject-verb-object) as their basic word order, SOV, OSV, and so on. Moreover, languages may have secondary orders (for example, an SOV language, in which an active sentence would have this order, might also have an OSV order for active sentences). The mapping from basic to secondary word order is done, in many cases, through a grammatical transformation. This cross-linguistic variation is thus very pertinent to the description of Broca's aphasia and to our understanding of the neurology of syntax; the application of the TDH to different structures may depend on basic word order. A comparison between languages with different basic orders (English, Japanese, and Chinese) might thus provide clues regarding the cerebral organization of syntax.

2.3.1. Comprehension in Japanese Broca's aphasia. Hagiwara (1993) has conducted a series of experiments on the comprehension of Japanese-speaking Broca's aphasics. Her impressive findings provide an unusual angle on the deficit and underscore its restrictive nature. They show how movement structure, not the active/passive distinction, determines agrammatic comprehension performance.

The linguistic description of basic Japanese word order has been a subject of debate. One initial puzzle comes from the two kinds of active sentences Japanese uses, featuring overt SOV and OSV orders (7a,b). Japanese was initially analyzed as a "nonconfigurational" language (Hale 1983), but it is now widely accepted that the SOV order is the basic, "nonscrambled" one (7a), whereas OSV (7b) is secondary, derived by a transformation that moves the object across the subject (Saito 1985; Saito & Hoji 1983; see also Fukui 1993; Miyagawa 1997). The evidence for this assumption comes from a variety of directions but is based mostly on the behavior of anaphoric expressions when they are in the different positions, which leads to the conclusion that *Hanako,* the object in (7b), must c-command the VP, so it must have moved to adjoin to a higher projection than that of *Taro,* the subject:

(7) *Active:*
a. *Nonscrambled (basic):*
Taro- ga Hanako-o nagutta ***above chance***
-NOM -ACC hit
Taro hit Hanako
b. *Scrambled (secondary):*
Hanako$_i$-o Taro-ga t_i nagutta ***chance***

The scrambled case is derived by a movement transformation, so its representation contains a trace. What would be the prediction of the TDH regarding agrammatic performance in Japanese? The configurations of the active sentences in (7) are given in (7′), together with the linked ϑ-roles for agrammatics:

(7′) a. S O V ***above chance***
Agent *Theme*
b. **O$_i$** S t_i V ***chance***
Agent *Agent*

If we assume that the trace is deleted, and that the operation of the strategy is like that in English, then it follows that in the scrambled case the object, moved from its base position, would not have a ϑ-role owing to trace deletion. The strategy would thus link Agent to it, by virtue of its sentence-initial position. The resulting thematic representation will be as in (7′b), and chance performance will follow. This is precisely what Hagiwara and Caplan (1990) obtained in a sentence-to-picture – matching test.

The split performance on actives in Japanese shows that chance performance can be obtained for agrammatics on "simple" active declarative sentences without passive morphology or relativization, but with movement. Next, we move to the opposite case in this language, sentences with passive morphology [-(*r*)*are*], with and without movement. These are the "direct" and "indirect" passives, tested by Hagiwara (1993) in 10 Broca's aphasic patients. Again, movement is the sole determinant of performance.

(8) *Passive:*
a. *Direct (derived):*
Taro$_i$-ga Hanako-ni t_i nagu-*rare*-ta ***chance***
-NOM -by hit-PASS-PAST
Taro was hit by Hanako
b. *Indirect (not derived):*
Okaasan-ga musuko-ni kaze-o hik-*are*-ta ***above chance***
mother-NOM a son-by a cold-ACC catch-PASS-PAST
Mother had (her) son catch a cold on her

According to Hagiwara, the sentence in (8a) is a standard, "direct" passive, derived by a movement transformation. The representation is annotated and contains a trace of movement, which is coindexed with its antecedent, the moved NP *Taro* (with its nominative case -*ga*). The *by* phrase is *Hanako-ni.* Trace deletion should result in the subject *Taro* not being able to receive its ϑ-role through the chain of movement. It is thus subject to the Default Strategy that assigns it the Agent ϑ-role; the *by* phrase *Hanako-ni* is intact, just as in the English case; *Hanako* receives the ϑ-role of Agent. The result is a thematic representation with two Agents, so chance performance is predicted. Broca's aphasics indeed perform at chance on this structure, as do their English counterparts. By contrast, the "indirect" passive in (8b) is not derived by movement (as Hagiwara argues, following Kubo, 1990, and others). Although it has passive morphology on the verb (-*are*), this construction has several properties that lead to the conclusion that it is base-generated rather than transformationally derived. For example, the by -phrase in (8b) is not optional, whereas in a regular passive it is. Following the general prediction of the TDH (movement⇔comprehension problem), Broca's aphasics perform at above chance on this structure, although it contains passive morphology, is more complex (having a tree with more nodes), is more loaded semanti-

(9) **Japanese actives and passives in aphasia**

MOVEMENT	*PASSIVE MORPHOLOGY* –	*PASSIVE MORPHOLOGY* +	*PERFORMANCE LEVEL*
–	basic (unscrambled) active	indirect passive	*above-chance*
+	**derived (scrambled) active**	**direct passive**	***chance***

cally (having two predicates), and has more words than constructions that lead to chance performance, such as the "scrambled" active or the "direct" passive.

The results from the comprehension abilities in Japanese Broca's aphasics provide a comparative perspective that demonstrates the stability of this syndrome across languages, as well as the generality of the proposed account. Moreover, it underscores the claim that the deficit in Broca's aphasia is not related to passive morphology. The data cluster in a way that cuts across this factor: Broca's aphasics score at above chance on basic actives and indirect passives (with passive morphology) and at chance on derived (scrambled) actives and direct passives, as shown above (9).

Insofar as Broca's aphasics fail only on the bottom horizontal line, it follows that the sole determinant of the performance of Japanese Broca's aphasics is whether or not the structures contain transformational movement. This result has already been obtained for English. Specifically, it has been shown that Broca's aphasics easily comprehend English sentences with passive morphology that are not derived by transformations (Grodzinsky et al. 1991). These are sentences such as (1e) above (*the man is interested in the woman*), for which a transformational analysis does not apply for various reasons. This conclusion rules out simplistic approaches, according to which comprehension and its failure are "cue"-driven, namely, that sentence comprehension, which normally depends on overt "cues" such as passive morphology (*-en*) or the preposition *by,* is impaired in Broca's aphasics because they are insensitive to these overt markers. Finally, these results fortify the syntactic accounts of Japanese word order, which assume that its basic word order is SOV (see Kayne, 1994, and Miyagawa, 1997, for recent discussions).

2.3.2. Agrammatic comprehension of relative clauses in English versus Chinese. Su (1994) has investigated the comprehension of relative clauses in Chinese in two Chinese speaking agrammatic Broca's aphasics. Of interest is the peculiar structure of Chinese relative clauses; although the overt basic word order is SVO – as seen in the bracketed relatives in (10) – heads of relative clauses, as well as complementizers – boldfaced in (10) – follow the relative, contrary to the case in English (11); this clear contrast in phrasal geometry correlates, in a rather interesting way, with the performance of Broca's aphasics on relative clauses in the two languages. Whereas object relatives (11b) are impaired in English, and subject relatives are preserved (11a), Chinese relative clauses show the exact opposite pattern (10):

(10) *Chinese relative clauses*

a. *Subject:* [t_i zhuei gou] **de$_i$ mau$_i$** hen da ***chance***
chase dog **COMP cat** very big
the cat that chased a dog was very big

b. *Object* [mau zhuei t_i] **de$_i$ gou$_i$** hen xiao ***above chance***
cat chase **COMP dog** very small
the dog that the cat chased was very small

(11) *English relative clauses*

a. *Subject:* **[The man]$_i$ who$_i$** [t_i pushed the woman] was tall ***above chance***

b. *Object:* **[The man]$_i$ who$_i$** [the woman pushed t_i] was tall ***chance***

The performance of Broca's aphasics is predicted only by the location of the trace and its interaction with the strategy: In English, the (clause initial) head of the relative is assigned the Agent role, whereas in Chinese, the head (being clause-final) is Theme. Most importantly, the performance of aphasics cannot be specified through reference to construction type (i.e., subject- vs. object-relative clause), as the contrast between English and Chinese shows. This conclusion holds, despite debates regarding word order in Chinese (see, e.g., Huang 1982; Travis 1984). This is reminiscent of the discussion of Japanese, where the data could not be accounted for through a specification of constructions such as active and passive or a morphological distinction between them. The comparative examination has shown, then, that the determinants of the comprehension deficit in Broca's aphasia are phrasal geometry, the location of the trace, and the interaction of trace deletion with the strategy. Finally, if the comprehension deficit is part of the diagnosis, parametric considerations (in this case, word order parameters) must be taken into account (see also Beretta et al. 1996, for comprehension data on Spanish-speaking Broca's aphasics that support this view). An "extensional" diagnosis of Broca's aphasia, that is, one based on an enumeration of construction types with which the patients succeed and fail, is ruled out.

2.4. Combining perspectives: Judgment and real time

2.4.1. Results from grammaticality judgment. The results that have just been reviewed provide factual support for the TDH, yet they come from a single experimental perspective, that of direct tests of sentence comprehension. The TDH has a broader potential, however, and can be coupled with other experimental methods. For example, traces are also involved in the operation of constraints on syntactic movement, as we saw in (3). The TDH predicts that violations of grammaticality, in which the trace is crucially involved, would go undetected by agrammatic aphasics; the claim is that these traces are deleted from the representation and so cannot participate in the determination of the grammatical status of a string. Such a finding, if obtained, would provide strong evidence for the claim that the deficit manifested in trace deletion is indeed representational and does not follow from a deficit to the ϑ-module.

Schwartz et al. (1987) have obtained results that provide preliminary clues regarding this issue. They conducted a "plausibility judgment" study in which their patients were asked to make judgments about whether sentences made sense. Their design intersected syntactic movement with semantic plausibility and with length: regarding the semantic factor, the patients were given semantically implausible sentences in which the source of the implausibility was either one of the arguments (12a) or two (12b); syntactically, there was an argument that was either in situ (12a) or moved transformationally (12c,d); lengthwise, the semantic violation was also nested in a "padded" sentence that contained many words (12e) but no movement. Naturally, each violation had a plausible counterpart:

(12) a. #The puppy dropped the little boy — ***success***
b. #The spoon ate the table — ***success***
c. #The table was eaten by the spoon — ***success***
d. #It was the little boy that the puppy dropped — failure
e. #The puppy ran excitedly and accidentally dropped the little boy onto the wet grass, which upset Louise. — ***success***

Five patients were tested, all diagnosed as Broca's aphasics on standardized tests. Four of them had lesions that resulted from occlusion of the left middle cerebral artery, and one had temporoparietal closed-head injury. The results were remarkable; the patients performed relatively well on most conditions, detecting and rejecting implausibility and accepting plausible sentences. They failed (and did so rather badly) only on a subset of the sentences that contained transformational movement. These were sentences in which the source of implausibility was one transformationally moved argument (12d). By contrast, the aphasics were able to detect plausibility violations if an offending argument was not moved by a transformation (12a,c). Thus in (12a) the cause of implausibility is *the puppy,* an argument that is not moved; in (12c), even though one source of implausibility, *the table,* is moved (hence undetectable), the detection of the anomaly is possible through the other argument, *the spoon,* which is also semantically anomalous but which is not moved by a transformation.

In sum, the patients in this study were unable to perform normally if, and only if, computing syntactic movement was necessary for the determination of (im)plausibility. This is obviously the prediction of the TDH, that agrammatic Broca's aphasics can detect semantic anomalies where the traces are not involved in the mediation of ϑ-role assignment. If traces are involved, however, the patients are expected to fail, as they did.

The Schwartz et al. study, though important, assessed judgment abilities only indirectly, it involved a mix between lexical semantics and movement in a task that necessitated interpretation, rather than pure judgment of grammatical well-formedness. Its findings are consistent with the TDH but are open to other interpretations as well. It is possible, for example, that traces are not missing from patients' representations but, rather, are inaccessible to interpretive tasks (ϑ-transmission) and that this is the reason for the patients' failures. Schwartz et al.'s result, then, is inconclusive as far as the theory is concerned. To investigate this matter directly, a study was recently conducted, aimed at assessing grammaticality-judgment abilities in aphasia, where the structures presented were violations of constraints on movement and a large group of controls (Grodzinsky & Finkel 1998). We tested aphasic sensitivity to violations of constraints on movement of constituents (NP and Wh-movement), each case coming with its own set of grammatical controls – cases 1 through 4 in (13). We further investigated aphasic ability to detect violations of constraints on head (verb) movement – cases 7 and 8 in (13) – for which we had independent evidence: agrammatic aphasics are capable of representing traces of such movement (see, e.g., Lonzi & Luzzatti 1993). Finally, we also included control conditions – violations of other grammatical principles (cases 5 and 6) to make sure that our patients were able to carry out the task.

We tested four nonfluent, agrammatic Broca's aphasic

(13)

Condition	*+Grammatical*	*−Grammatical*
1. NP movement	It seems likely that John will win It seems that John is likely to win John seems likely to win	John seems that it is likely to win
2. Wh movement/that trace	Which woman did David think John saw? Which woman did David think that John saw? Which woman did David think saw John?	Which woman did David think that saw John?
3. Superiority	I don't know who saw what	I don't know what who saw
4. Adjunct/complement	When did John do what?	What did John do when?
5. Filled gaps	Who did John see? Who saw John?	Who did John see Joe? Who John saw Joe?
6. Bad complements	The children threw the football over the fence The children sang	The children sang the football over the fence The children threw
7. Place of auxiliary	They could leave town Could they leave town? They could have left town Could they have left town? They have left town Have they left town?	Have they could leave town?
8. Negation	John has not left the office John did not sit	John did not have left the office John sat not

patients, all diagnosed as such, with lesions in and around Broca's area, including white matter deep to it, ranging from the operculum, to the anterior limb of the internal capsule, to the periventricular white matter. Our results uncovered a fine, highly restricted deficit, which follows directly from the TDH: The patients were very alert to grammaticality in general, yet they failed when required to compute constraints on the movement of phrasal constituents (conditions 1–4). In each of these conditions, error rates were about 40%. All others (condition 5–8) were intact (about 10% errors). Interestingly, a recent study of grammaticality judgment in Serbo-Croat reported results tending in the same direction (Mikelic et al. 1995). Finally, Wernicke's patients, though producing a slightly different pattern, were not particularly successful in making grammaticality judgments, despite the fact that their deficit is usually thought of as semantic, not syntactic. The conclusions that follow are clear:

1. Traces of constituent movement are missing from patients' representations.
2. Every other aspect of syntax is intact after a lesion in Broca's area and its vicinity.
3. Damage to Wernicke's area, which is most often thought of as the region dedicated to semantic analysis, also produces rather severe syntactic disruptions.

2.4.2. A real-time perspective. A fourth, critical angle on the deletion of traces is that of real-time processing. Information regarding the time course of language comprehension in aphasia is obviously crucial to our understanding of the underlying pathology. Indeed, extensive investigations of aphasic behavior in real time have been conducted in recent years (see, e.g., Bradley et al. 1980; Friederici 1985; Milberg & Blumstein 1981; Shapiro & Levine 1990; Shapiro et al. 1993; Swinney et al. 1989). Shapiro and his colleagues have provided a compelling demonstration of the claim that the overall processing capacities of Broca's (but not Wernicke's) aphasics in comprehension are intact. They conducted a series of studies on patients' lexical abilities, and their manifestation in real-time sentence processing (Shapiro & Levine 1990; Shapiro et al. 1993). They showed that Broca's aphasics are normal in the way they handle verbs, in that their on-line processing routines make fine distinctions among verb types in exactly the way normal speakers do. This was not the case in Wernicke's aphasia, however. Posterior lesions, then, unlike those in Broca's area, do cause damage to the lexicon.

This work must be compared to the studies on the perception of trace-antecedent relations by Broca's aphasics. It will be seen that this work, when considered together with Shapiro et al.'s results, indicates that the patients are impaired only when the linking of traces to their antecedents is at stake, and nowhere else. To make this point, however, some background is essential.

It is by now well established that normal language users demonstrate trace-antecedent relations in real-time tasks (see, e.g., Bever & McElree 1988; Love & Swinney 1996; McDonald 1989; Nagel et al. 1994; Stowe 1986; Swinney & Nicol 1989; Swinney & Zurif 1995; Swinney et al. 1988; Tanenhaus et al. 1989). The typical experiment exploits priming effects to uncover antecedent reactivation. The leading idea is that the link between a trace and its antecedent means that, in the course of comprehension, the antecedent is reactivated at the trace. Thus in (14), *the drink* will be active when heard (namely, at *1*), will then decay (*2*), but will be reactivated following the verb (*3*), because of its link to the trace.

(14) The priest enjoyed **the drink**^{1} that the caterer was^{2} serving $\boldsymbol{t}^{3}$ to the guests

This is precisely what on-line experiments on normal language users have discovered. Through different methods – cross modal lexical priming (CMLP) being a central one – reactivation of antecedents in the position of their traces has been shown for subject- and object-relatives (Swinney & Nicol 1989; Swinney & Osterhout 1990; Swinney & Zurif 1995; Swinney et al. 1982), for passives (McDonald 1989), and for other structures. The experiments take *the drink* as prime, and, while the sentence unfolds auditorily, a target is flashed on a screen at points (*1*)–(*3*). The expected finding, then, is that if a target word, say, *juice,* is presented visually to subjects at points (*1*), (*2*), or (*3*) when they are listening to the sentence, and the subjects have to make a lexical decision on it, priming effects will be documented at (*1*) and (*3*) but not at (*2*).This is what is found; priming effects are obtained only in (*1*) and (*3*).

Now, consider the TDH and the expected real-time behavior of Broca's aphasics in CMLP. Deleted traces mean no reactivation at the trace. This means that only in point (*1*) would a priming effect be obtained. Decay would explain the lack of an effect in (*2*), and the correlate to trace deletion would be a lack of priming in (*3*). Conducting such experiments is quite difficult, yet this is precisely the result of a series of carefully controlled studies of both subject- and object-relative clauses (Swinney & Zurif 1995; Zurif et al. 1993). Importantly, Broca's aphasics do prime, even if not in a fully normal fashion (see, e.g., Shapiro et al. 1993; Shapiro & Levine 1990; Swinney et al. 1989). However, when faced with a task that involves priming within a movement-derived construction, they are seriously impaired. Finally, this failure is not characteristic of all aphasics, nor is it necessarily related to general comprehension skills: Wernicke's aphasics with posterior perisylvian lesions perform normally on this task, even though their comprehension abilities are severely compromised.

We have gone through various sorts of evidence, among which are tests of real-time syntactic analysis. This review led to a new delineation of the deficit, but can this lead to an unambiguous statement regarding the underlying cause? An answer to this question would lead to an explicit theory of the function of the anterior language areas. The question, obviously, is whether this area supports a device dedicated to syntactic analysis of transformationally moved constituents and, if so, what this device is. One possibility is that the comprehension deficit follows from a general disruption to "working memory," not from a language mechanism. A recent PET finding (Jonides et al. 1997) indicates that, in nonlinguistic memory tasks that require the subject to relate two nonadjacent members in a list, Broca's area is activated in a secondary fashion. To some this may sound like proof that this area supports a memory cell, the disruption of which entails the syntactic deficits in this syndrome, yet such a conclusion is a bit hasty. Although it may turn out to be true in some future, "final" analysis, the conclusion is not warranted on the basis of the available evidence. To argue for a disruption to a generalized memory resource, one must show that this resource makes contact with formal constraints on the inner workings of this memory store, from which constraints on syntactic movement would follow. Moreover, we have seen that other real-time performances of Broca's aphasics show that their language-processing device is intact. Lexical access and insertion into sentential positions – found intact by Shapiro and his colleagues – demonstrate directly how specific the impairment is. We may then move on and consider other language-specific interpretations, namely, is Broca's area home for a processing device whose disruption precludes antecedent reactivation at the right time, or is it, perhaps, a representational medium that makes silent categories disappear? It is not possible, at this point, to decide between these options, but, either way, we have delineated the deficit and have shown that any deficit analysis, hence any theory of the role of Broca's area, must have the consequences of the TDH.

2.5 Functional neuroimaging

The discussion above has been based on aphasia results, which allow for a precise characterization of the role of Broca's area through deficit analyses. Recent technological developments in neuroimaging are beginning to make it possible to see pictures of the brain in action. The current neuroimaging literature makes only tenuous connections to the lesion-based body of knowledge and focuses, for the most part, on semantic and phonological aspects of the mental lexicon. Fewer works concentrate on the most salient aspect of language, its combinatory nature. Fewer than 10% of the works presented at a recent *Neuroimage* conference (10 of 101) investigated the computation of combinatorial operations in language processing. Still, there are some, perhaps preliminary experiments on functional imaging of sentence processing (Bavelier et al. 1997; Just et al. 1996; Mazoyer et al. 1993; Stromswold et al. 1996). The evidence they present is fully consistent with the new picture presented here.

Consider, first, the most linguistically detailed study available, Stromswold et al.'s PET investigation. They measured blood flow during visual exposure to sentences whose plausibility the subjects were requested to judge. The relevant conditions included several experimental variables: relative clauses that were either (plausible or implausible) right-branching subject gaps (15a,b), or (plausible or implausible) center-embedded object gaps (15c,d):

(15) a. The biographer omitted the story that insulted the queen
b. The biographer omitted the queen that insulted the story
c. The limerick that the boy recited appalled the priest
d. The boy that the limerick recited appalled the priest

Other experimental conditions may be ignored; comparisons that include them introduce too many confounding variables to allow an interpretation. We are left, then, with one critical comparison, between the subject relatives (plausible and implausible) and their object counterparts. The subtraction of the signals detected by the PET machine, namely, [(15c) + (15d)] – [(15a) + (15b)], revealed "hot spots" in Broca's area and nowhere else.

What can be made of this result regarding the functional role of this cerebral region? An examination of the stimulus materials reveals three dimensions: plausibility, type of relative (center-embedded vs. right-branching), and gap location (subject vs. object position). Broca's area lit up as a consequence of the interaction of all three, so no conclusion that separates these three factors can be made. As a consequence, the meaning of this result is not entirely clear, nor is there an apparent connection to a theory. Still, one conclusion can be prudently drawn: The PET finding is consistent with the claim that the computation of transformational relations is made in Broca's area, because one of the experimental variables was the location of the gap.

Stromswold et al.'s study is more linguistically sophisticated than any of the other available investigations of functional imaging of language activity. Other studies do not make detailed claims possible. Thus Just et al. (1996) used fMRI to test neuronal activity during language comprehension. They presented sentence pairs as in (16), on which the subjects were expected to make a truth-value judgment. The experimental sentences were chosen by a "complexity" measure that the authors do not specify. They included three types of fairly complex stimuli: actives with conjoined VPs (16a), center-embedded subject-relative clauses (16b), and center-embedded object relative clauses (16c), each followed by a question (italicized), to which the subjects were requested to respond by pushing a yes/no button:

(16) a. The reporter attacked the senator and admitted the error.
The reporter attacked the senator, true or false?
b. The reporter that attacked the senator admitted the error.
The reporter attacked the senator, true or false?
c. The reporter that the senator attacked admitted the error.
The reporter attacked the senator, true or false?

Several results are reported. First, although signals were picked up in Broca's and Wernicke's areas of the left hemisphere on exposure to all types of stimuli, their right hemispheric homologs remained relatively silent; second, Broca's and Wernicke's areas were equally activated; third, there appeared to be a difference in number of activated voxels among the three sentence types. Stimuli (16a–c) engaged an increasing number of voxels in this order. All cases involved very complex materials; apart from a (transformationally derived) question in every case, there was a transformation in both types of relative clauses and a complex construction, involving VP conjunction, in the third. Comparisons are therefore far from being straightforward. Still, it is important to emphasize that, although these results do not lead to specific conclusions, they are consistent with the position espoused herein.

Next, Mazoyer et al. (1993) looked at the brains of normal French speakers during language understanding. They exposed their subjects to stories in an unfamiliar language (Tamil), to French word lists, to well-formed sentences containing nonwords, to semantically anomalous sentences, and to stories in French (composed of sentences whose grammatical properties are not given). Right and left temporal regions were activated in most conditions, yet only left inferior frontal regions, in and around Broca's area, lit up on exposure to words and stories in the subjects' language (French). Listening to stories in French, moreover, activated the left middle temporal gyrus significantly more than its right hemispheric homolog.

Finally, Bavelier et al. (1997) also conducted an fMRI study, with stimuli described as "short declarative English sentences (mean length = six words, range = four to nine words) and . . . consonant strings of equivalent length to the words used (mean length – four letters)." Another control was a film of a speaker of American Sign Language, signing the same sentences. Here, again, it was found that the right hemisphere remained silent whereas the left was active on exposure to sentences in a familiar language.

Because the choice of stimuli for this experiment was not based on structural considerations, only general conclusions regarding the cerebral representation of language mechanisms are possible. Vague as they are, these results are fully consistent with the more detailed claims made on the basis of lesion studies. The same is true of all the imaging studies we reviewed; they are all consistent with the claim that the linguistic factor associated with "hot spots" in Broca's area is not syntax as a whole but, rather, mechanisms that underlie syntactic movement. Critically, there is nothing in the functional imaging results that contradicts these conclusions or speaks against the TDH-based view of the limited role of Broca's area in language processing.

2.6. Generality and counterevidence

The degree of generality of the above is appreciable; dozens of works are reviewed here, including studies of many neurologically intact subjects, as well as brain-damaged patients, tested in different laboratories at different times, in different countries and languages, on a broad variety of sentence types. The tasks reviewed come from comprehension in several languages, grammaticality and plausibility judgment, and real-time processing. The results converge. Still, it is important to note that some studies yield two types of seemingly contradictory results: those that fail to replicate previous results, which have sometimes led to the claim that the syndrome of Broca's aphasia gives an altogether inconsistent picture (e.g., Berndt et al. 1996; Druks & Marshall 1995; Martin et al. 1989), and those that document performances that run contrary to the TDH but accept basic premises (see, e.g., Hickok & Avrutin 1995; Hickok et al. 1993). Both types of presumed inconsistencies must be taken very seriously. An unstable syndrome (and certainly a nonexistent one) is the wrong object of inquiry; likewise, a false hypothesis is, most likely, the wrong one to follow. It must be revised, perhaps even abandoned, when confronted with data for which it cannot account. Thus, an old debate has been revived in recent years, in which the coherence of the clinical categorization as well as the validity of the TDH has been challenged (see Badecker & Caramazza 1985 and Miceli et al. 1989, for attacks on the syndrome-based conception; see also Berndt et al. 1997, Hickok et al. 1993, and Lukatela et al. 1995, for critiques of the TDH on the basis of new data). Many (if not most) objections have been explained away; in some cases patient selection was the cause of the inconsistency (patients that were not Broca's aphasics were included in the experimental group) and in others there were problems with experimental procedures or design; in still others, the results may have been misanalyzed or misinterpreted (Caplan 1986; Grodzinsky 1991; Zurif 1996; Zurif et al. 1989).

Still, even after this type of cleanup, a certain amount of intersubject variation persists. Grodzinsky et al. (1999) have recently conducted a survey of comprehension scores of Broca's aphasics in two contrasts that pertain to the TDH: actives versus passives and subject-gap versus object-gap relative clauses. For both sentence types, there are multiple studies; the review covers 17 different studies of active/passive, with 42 different patients, and 4 studies of subject/object relatives and clefts, with 17 different patients. All the comprehension experiments we analyzed had a binary choice design (containing the correct response and its thematic reversal). The expectation from the results of the active sentences was a pattern approaching 100% correct. For the passive case, chance performance was expected; thus, because patients are said to be guessing, the performance of each patient should be equated with a series of (unbiased) coin tosses, because such series are exactly the expression of chance behavior. Such series are known to distribute binomially, with a median of around 50% correct, so we expect our patients to follow this distribution. Moreover, we expect a statistically significant difference between the performances on actives and passives. The analysis of the actual data reveals that this is the case: The actives cluster around the 100% mark, the passives distribute binomially with a mean and median of 55%, and the two differ significantly from one another. Figure 1 shows the distribution of performances for actives (dashed line) and passives (dashed line with circles), where the latter is compared to a computer-generated model of an averaged binomial distribution (solid line).

An analysis of subject/object relative clauses and clefts produces similar results. The data thus fit the theoretical expectations, and the analysis further demonstrates that constrained within-group variation is permissible, as long as the group picture leads to scores that, on precise measurement, yield the expected results. This analysis also shows why single-case studies can be very misleading: Extreme cases, such as Druks and Marshall's (1995) patient who performed worse on actives than on passives (see Zurif, 1996, for a critique), cannot be evaluated seriously outside a group. It is only in this context that one can judge whether they conform to an expected pattern. Outside this context, these cases are seen as exceptional, which may not be true. Our analysis of the current empirical record suggests that an approach to aphasia that allows constrained variation is on the right track; the weight of the evidence indicates that the TDH has had, as far as is known, the broadest empirical coverage. Still, certain core cases have forced reformulation.

An interesting empirical objection comes from the work of Hickok and Avrutin (1995), who devised a novel comprehension task through which they documented a surprising asymmetry in agrammatic comprehension. They tested agrammatic comprehension on four types of questions,

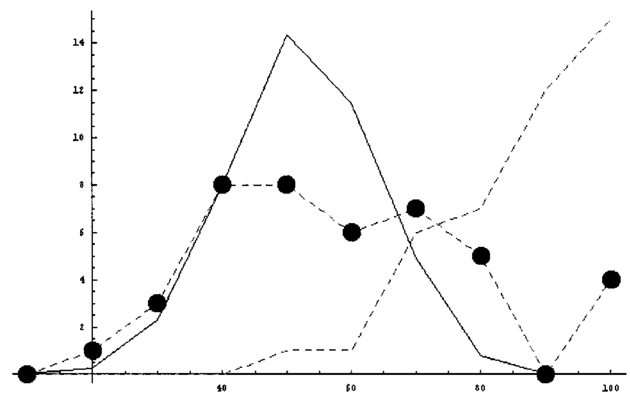


Figure 1. Distribution of performances for 42 Broca's aphasics (no. of patients vs. percentage correct) for active (dashed line) and passive (dashed line with circles) sentences. Solid line is a computer-generated simulation of a binomial distribution (Adapted from Grodzinsky et al. 1999).

along two dimensions: questions pertaining to subject (17b,d) versus object (17a,c) position and those expressed by *who* (17a,b) versus *which* (17c,d):

(17) a. **Who**$_i$ did the horse chase t_i? *above chance*
b. **Who**$_i$ t_i chased the giraffe? *above chance*
c. **[Which giraffe]**$_i$ did the horse chase t_i? *chance*
d. **[Which horse]**$_i$ t_i chased the giraffe? *above chance*

The two Broca's aphasic patients they tested were above chance on subject questions (17b,d) and at chance on the object question beginning with *which* (17c); these results are consistent with the TDH, as well as with previous data on subject/object asymmetries that we saw in (1) and (2). On (17a), however, namely, on the *who* object question, the patients were, unexpectedly, above chance, in apparent violation of the TDH.

Moreover, it has been found that in certain cases Broca's aphasics are not at chance on the comprehension of certain (transformationally derived) passives, giving rise to yet another surprising asymmetry (five patients in Saddy 1995 and Balogh & Grodzinsky 1996). We have already seen how Broca's aphasics comprehend nontransformational passives at near-normal levels. At issue here is a case of a different type: When the subject of a passive is a quantified expression (18a), near-normal performance is yielded by the very same patients who are at chance on a "regular," agentive passive sentence (18b), again, contrary to the TDH, which is indifferent to the properties of the antecedent of the trace:

(18) a. **[Every boy]**$_i$ was pushed t_i by a man *above chance*
b. **[The boy]**$_i$ was hit t_i by the man *chance*

Both findings are inconsistent with the TDH as stated. It has, therefore, been revised to accommodate them (Grodzinsky 1995a). The account builds on a common property that both cases share. Specifically, the observation is that both antecedents – *who* in (17a) and *every boy* in (18a) – have a common semantic property that sets them apart from regular NPs. These two subjects fall under the default strategy that assigns the Agent role to moved phrasal constituents that have no ϑ-role. Thus, the idea of the revision is to restrict the scope of the strategy and condition its application to a ϑ-less NP – an antecedent of a trace – on the semantic properties of that NP. The proposal is that this semantic property (which relates to the way these elements interact with discourse semantics) precludes the strategy from being applied in such cases, on semantic grounds. Whether this new account is valid remains to be seen. What is important, though, is that it is this type of account that leads to the reformulation of new empirical questions, and thus to a systematic enrichment of the database. Indeed, the overall experimental picture has gradually built more structure into the relationship between linguistic behavior and the neural tissue that supports it.

Finally, a question arises regarding the syndrome specificity of the results. In particular, is the deficit as characterized by the TDH specific to Broca's aphasia? Can it be found in Wernicke's aphasia or, perhaps, subsequent to damage to the right hemisphere? That the latter is not the case will be shown in section 5.1. Regarding Wernicke's aphasia, the record seems somewhat mixed: Wernicke's aphasics do not always carry out syntactic tasks successfully, yet their performance does not fall under the TDH. That is, the way they fail is much less uniform, and in most cases very large variation is observed for these patients. There are, admittedly, fewer studies dedicated to syntax in Wernicke's than in Broca's aphasia, but the available results are compelling (see Shapiro et al., 1993, and Swinney & Zurif, 1995, for Broca's/Wernicke's difference in real-time sentence processing; Grodzinsky & Finkel, 1998, for judgment; and Grodzinsky, 1984b, and Zurif & Caramazza, 1976, for comprehension). There are, perhaps, some syntactic abilities in the posterior language area, but they are less tangible and characterizable than those in the left frontal cortex, which leads to clear conclusions, as we have seen.

2.7. Language production without Broca's area: Pruned syntactic trees

2.7.1. Morphological correlates of error types. The focus of this target article is comprehension, but Broca's area, or so the evidence suggests, subserves not only receptive mechanisms of language but also language production. In fact, the most salient feature of Broca's aphasia has always been effortful, nonfluent, and telegraphic speech. Below, I review some recent results in the production of language in Broca's aphasia, because they emphasize two points. First, they show the difference between the deficits in expressive and receptive mechanisms of language; second, they again show the need for an abstract grammatical approach to language and the brain.

Consider, first of all, a salient cross-linguistic difference in the behavior of agrammatic Broca's aphasic speakers and the way in which this reveals how linguistic distinctions are honored by the brain. The omission of functional elements in speech production has always been the hallmark of the diagnosis of agrammatism in Broca's aphasia (see, e.g., Adams & Victor 1993, p. 417; Goodglass 1976; 1993; Marshall 1986), yet it is well established that patients either omit or substitute inflectional elements, but only if the morphology of the ambient language permits such omissions; the patients observe rules of lexical well-formedness and never produce nonwords or word parts (Grodzinsky 1984a; 1990). In both verbal and nominal inflected elements, omissions of inflectional morphemes are observed if a bare stem is a real, licit, word (+zero morphology); otherwise (i.e., in words that are −zero morphology, where omission of inflection is illicit and results in nonwords), substitution errors occur:

(19) a. *English*
Uh, oh, I guess six month . . . my mother pass away.
b. *Hebrew*
tiylu anaxnu ba'ali ve-'ani
took-a-walk (third-person pl. common gender) we my-husband and I
c. *Russian*
grustnaja malchik. stol stoyit, vot,
sad (fem.) boy (masc.). table stands (sing.), lo,
stol stoyat stoyit
table stands (pl.), stands
d. *Italian*
Cappucetto rossa andava
Little Ridinghood (masc.) Red (fem.) went

e. *Japanese*
inorimasu (correct: inorimasushita)
I-pray I-prayed

The same cerebral pathology is manifested differently, depending on the morphology of the language spoken. In English and Japanese we observe the familiar pattern of omission of the inflectional morphology, whereas in the rest of the languages we see an inflectional element substituted for another, with a resulting grammatical aberration, but lexical structure is unimpaired. *Zero morphology* is thus relevant: Broca's aphasia in a language where stems are legal words presents with omissions; otherwise, there are substitutions. Grammars (or subgrammars) align themselves accordingly:

(20)

+Zero morphology	*−Zero morphology*
English Japanese	Hebrew Russian Italian
omission	*substitution*

2.7.2. An outline of an account. An account of these phenomena must be grammatical and abstract. If we assume that inflectional features (φ-features) are underspecified in the syntactic representation of agrammatic Broca's aphasics (Grodzinsky 1984a; 1990), we get errors of inflection, whose type depends on the ±zero-morphology property of a given language: Underspecified features in a +zero-morphology language would result in omission, whereas in a −zero-morphology language the result would be substitution. There is, then, a varied manifestation of the syndrome, which correlates perfectly with an identifiable linguistic property:

(21) Broca's aphasics tend to omit inflections if they speak a language with a zero-inflectional morpheme; otherwise, they tend to substitute.

Because omission has always been a critical diagnostic sign of agrammatism in Broca's aphasia, clinicians cannot ignore the variation just presented if they are interested in a precise and universal diagnosis. They must accordingly enter the conditional statement in (21) into the clinical diagnosis of agrammatic Broca's aphasia to allow for variation in error type as a function of grammatical (morphological) properties of the language spoken. Similar to the cross-linguistic TDH, the description of speech production errors across languages leads to a parametric definition of the syndrome, one in which grammatical parameters (imported from a theory of possible grammatical variation) are embedded. We return to this issue below.

2.7.3. A more restricted deficit. More recent findings complicate the picture even more. The deficit seems to bear not only on different types of inflectional categories but also on their location in the syntactic tree. Inflectional elements are impaired or preserved depending not just on their morphology but also on their structural position. The first piece of evidence for this claim came from a Hebrew-speaking patient (Friedmann 1994), who was selectively impaired in the production of inflectional features. She had problems with tense, but not agreement. This finding runs contrary to common belief, according to which agrammatic aphasics have equal problems with all functional categories. A retrospective literature review found other cross-linguistic evidence tending in the same direction: A significant group of patients reported in the literature also showed impairment in tense but not agreement (Miceli et al. 1989; Nespoulous et al. 1988; Saffran et al. 1980), yet the opposite (impaired agreement but not tense) is never found:

(22) *Speaking English:* The kiss . . . the lady kissed . . . the lady is . . . the lady and the man and the lady . . . kissing.

(23) *Reading French aloud:*
Target: Bonjour, grand-mere, je **vous ai apporté**
good morning, grandma, I to-you have bring (pres.-perf.)
Read: Bonjour, grand-mere, je **portrai** euh je/pu/ / zeda/ a-aporté
good morning, grandma, I bring (future)

Seeking to obtain a detailed error analysis, Friedmann then created a series of tests to track the exact nature of the impairment in tense versus agreement in speech production of Broca's aphasics. The distinction made by the patients was especially important in light of recent developments in linguistic theory. According to the split-inflection hypothesis (Pollock 1989) there are structural differences between tense and agreement, each forming a distinct functional category. This hypothesis provides not only a powerful and precise descriptive tool but also a host of related issues to be examined. The tests were first conducted on one patient (Friedmann 1994; Friedmann & Grodzinsky 1997) and then extended to a larger group of 13 Hebrew- and Arabic-speaking patients (Friedmann 1998; Friedmann & Grodzinsky, in press):

(24) Yesterday the boy walked;
Tomorrow the boy ____. Yesterday the **boys** ____.
Tense condition Agreement condition

The results were remarkable. Although agreement was normal, tense was severely impaired, even though the patients' perception of time, as well as comprehension of temporal adverbs, was shown to be intact. Tense errors were mostly substitutions of inflection (with no preferred "unmarked" form), observed in repetition (25) and in completion (26) tasks. In (27) a numerical representation of error rates is presented:

(25) *Target*: ha'anashim **yixtevu** mixtav la-bank
the-people write-**future**-3-m-pl letter to-the-bank
Repeated: ha-anashim **katvu** mixtav la-bank
the-people write-**past**-3-m-pl letter to-the-bank

(26) *Target*: axshav ata holex. etmol 'ata ——(expected: halaxta)
now you go-pres-2-m-sg yesterday you —— (go-**past**-2-m-sg)
completed: axshav ata holex. etmol ata *telex*
now you go-pres-2-m-sg. yesterday you go-**future**-2-m-sg.

(27)

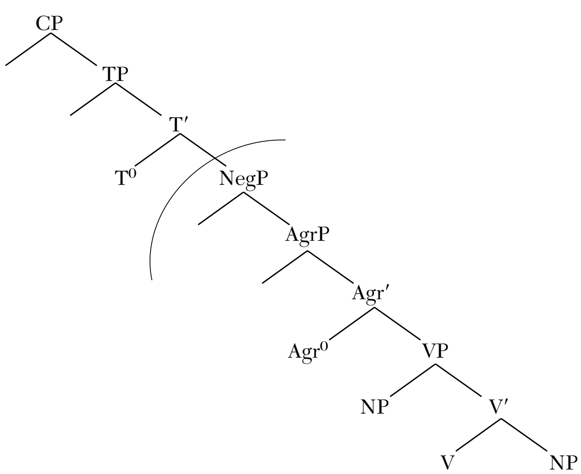
<u>Agreement errors</u>	<u>Tense errors</u>
3.9% (5/127)	42.4 % (62/146)

This dissociation suggests a deficit that implicates tense but not agreement features. This is new; agrammatic aphasia has always been thought to implicate all functional elements equally. The striking asymmetries observed appear to have been overlooked. The impairment, moreover, extends to a cluster of syntactic properties related to the Tense node (according to the split-inflection hypothesis), which are also disrupted. Observed are subject omissions, difficulties with copulas, and specific word-order problems that pertain to nodes in the syntactic tree that are beyond the Tense node, but nothing below this node is impaired. The impairment is also associated with problems in still higher parts of the tree (CP). As a result, Wh-questions and embedded clauses are nonexistent or completely ill-formed in the speech of the patients.

By contrast, other properties related to Agreement and to lower parts of the tree are left intact. The distinction that linguists have posited receives direct neurological support. The speech-production problem in Broca's aphasia affects the tree from the Tense node and above and leaves everything below it intact.

2.7.4. A restrictive, structure-dependent account: The tree-pruning hypothesis. This rather rich cluster of cross-linguistic facts has led to a description of agrammatic speech production that is stated over trees, not elements. That is, unlike every previous statement, which looked at functional elements regardless of their position in the sentence, the currently available data lead to the view that agrammatic aphasic patients produce trees that are intact up to the Tense node and "pruned" from this node and up (Friedmann 1994; Friedmann & Grodzinsky 1997):

(28) Agrammatic phrase marker. Arch represents site of deficit.



This claim receives empirical support from yet another direction. There is a salient cross-linguistic difference in the production of verbs by Broca's aphasics. In English, the speech output of Broca's aphasics contains verbs that are bare stems, yet these are located in their proper position in the sentence – always after the subject. In verb-second (V2) languages (e.g., Dutch, German), however, where inflected verbs undergo movement, the situation is different. In these languages verbs start out in sentence-final position (SOV; see den Besten 1983 and Koster 1975, but see Zwart 1993, for a different analysis) and must raise to pick up their tense features, and the result is SVO order. A nonfinite verb in a main clause (for example, in a clause that contains an inflected auxiliary) will remain in final position, and its finite counterpart will be in second position. In a patient whose syntactic tree is pruned, verbs will fail to raise, and the result will be as is observed in Dutch: In aphasic speech verbs in main clauses not only appear uninflected but are also in sentence final position, resulting in ungrammatical strings (Bastiaanse & van Zonneveld 1998; Friedmann 1998; Kolk & Heeschen 1992). Dutch agrammatics make no errors on infinitives in subordinate clauses but have major difficulties with inflecting main verbs, which they usually produce not only as infinitives but, critically, in final position. Finally, it has been observed that verbs tend to be omitted relatively frequently from the speech output of English-speaking Broca's aphasics (Berndt & Zingeser 1990). As Friedmann (1998) shows, a problem in the lexical category "verb" is apparent only if this result is looked at in isolation. When the broader context is examined, however, a clearer picture is revealed. In particular, when cross-linguistic patterns of verb omission are reviewed, as Friedmann has done for English and Dutch, it turns out that only tensed verbs are omitted (see also Bastiaanse & van Zonneveld 1998), which is precisely what the Tree-Pruning Hypothesis predicts.

Six new observations should be highlighted.

1. The agrammatic production deficit is very strongly and directly linked to grammatical variables (the Tense node and its configuration).

2. This deficit is more restricted than was previously thought, encompassing functional elements above a certain node in the syntactic tree; the rest of the representation is intact.

3. The precise description of the deficit (hence the diagnosis of the syndrome) must be stated in abstract terms that allow (as linguistic theory does) cross-linguistic grammatical variation (e.g., infinitives in sentence-final position in V2 languages).

4. The availability of the rest of the syntax to speech production in the absence of Broca's area and its vicinity means that these cortical regions can no longer be viewed as housing syntax as a whole. Rather, major parts of the human syntactic capacity reside elsewhere.

5. The deficit to mechanisms of language production, though sharing important features with its comprehension counterpart, differs from it in important ways (tree pruning vs. TDH). This conclusion may run contrary to claims regarding a parallelism between comprehension and production (see, e.g., Zurif 1980), but it does not entail a total lack of a comprehension deficit in Broca's aphasia (Kolk & van Grunsven 1985b; Miceli et al. 1983; but see Zurif 1996 and Grodzinsky et al. 2000 for counterarguments). There is a comprehension deficit, but its description is different from the production problem. In the normal language-processing device, mechanisms for the planning and construction of sentences must diverge at some point from those dedicated to the analysis of incoming strings. Nevertheless, it is most likely that both mechanisms connect to one grammatical resource; hence, to some extent at least, they may be located in adjacent cerebral areas.

6. The account is somewhat reminiscent of recent accounts of children's grammar. First, like children (Hyams 1992; Poeppel & Wexler 1993), agrammatics have at least some functional categories – those below the Tense node (TP). Second, children's and Broca's aphasics' production seem to have the same problem in verb inflection. This similarity, however, is only apparent; both groups produce incorrectly inflected main verbs, yet, whereas the aphasics substitute tense inflection, children use only the nonfinite forms, but never substitute inflection (Wexler 1994). Finally, children, like the aphasics, produce matrix clauses with nonfinite verbs that lack elements that belong to higher parts of the tree – wh-words, complementizers, subject pronouns, and auxiliaries. However, there

is a crucial difference between children and Broca's aphasics: Children are able to build these constructions (Rizzi 1994), whereas the aphasics cannot project any higher than T.

3. Clinical issues: Diagnosis and remediation

3.1. Comparative aphasiology and parametric diagnostic principles

The pathological manifestations of agrammatic Broca's aphasia as presented here are very close not just to grammar but also to grammatical variation, in that a complete clinical picture depends on the patient's language. In language production, omissions are observed in +zero-morphology languages, and substitutions otherwise; verbs appear in their correct position in most languages, but in verb-second (V2) languages they are in a sentence-final position. In comprehension, errors occur in a fashion that depends on the interaction between syntactic movement and phrasal geometry, which is determined by word-order parameters. Variation between patients in both production and comprehension thus exists, because the grammatical parameters that interact with their deficit have different values in different language types. Importantly, variation in this syndrome is not wild but constrained and can be characterized precisely; the cross-linguistic differences at issue are relatively well defined. Despite all this, an odd situation is created, in which diagnostic principles for universal identification must be established for a disease that has more than one manifestation. Broca's aphasia is thus an unusual pathology, with varying, yet well-defined clinical signs that depend mostly on cerebral structure, but also on an environmental factor – the ambient language. A precise clinical characterization must be predicated on trace deletion (and tree pruning), which interact with parametrically characterized aspects of morphology and word order. Clinical testing will accordingly abide by these two descriptions yet will vary from one language to another. Physicians and speech pathologists can no longer test, say, the comprehension of active versus passive sentences in every language but rather must test the comprehension of sentences with and without movement. Diagnosis, in other words, must rely on abstract principles, which are instantiated differently in each language.

This is something unheard of in medicine. The medical literature does not contain a syndrome that is defined parametrically. That is, there is no disease that is diagnosed by a cluster of *abstractly* characterized pathological signs, each having more than one possible concrete manifestation whose form is determined by properties of the individual patient or his environment. Broca's aphasia, oddly enough, is such a disorder.

3.2. Recovery and remediation

Once the functional deficit in Broca's aphasia is characterized more precisely, we can perhaps try to think about treatment. That this is at all possible is far from obvious. An understanding of the precise nature of the deficit by no means guarantees the success of therapy; after all, the patients have lost a piece of their cortex. Still, it may lead to the invention of better methods, which might also be equipped with better evaluation and efficacy-assessment tools. A fascinating development in this direction has been the program set forth by Shapiro, Thompson, and their colleagues, who have investigated the course of recovery in Broca's aphasia along linguistic lines, in an attempt to devise novel methods for speech therapy. They have made some remarkable discoveries. First, they found that recovery proceeds along structural lines; when a syntactic construction reappears in speech, it is accompanied by its structural analogues (Thompson et al. 1993). Second, they devised an experimental therapy for aphasics, with which they have succeeded in training patients on movement (Shapiro & Thompson 1994; Thompson et al. 1996). In one recent study, for example, they trained their patients on one construction, and subsequently monitored their abilities on three others (Thompson et al. 1997). Teaching was controlled carefully, as was the assessment of the patients' abilities on the other structures, (29) and (30):

(29) *NP-movement* a. *Passive:* The biker was lifted by the student
b. *Raising:* The student seems to have lifted the biker

(30) *Wh-movement* a. *Object-cleft:* It is the student who the biker lifted
b. *Object-question:* Who has the biker lifted?

The results were remarkable. First, patients who were unable to generate these constructions before training were now much more proficient in their use. Second, training on one structure generalized to others. Third, generalization was highly constrained by syntactic principles. Specifically, a patient trained on passive improved on Raising, but not on clefts and questions; similarly, training on questions improved clefts, but not passive or Raising.

Although still experimental, such results are important. They show once more that neural tissue abides by fine structural constraints. If valid, this training program shows that the internal structure of the grammar, specifically, aspects of syntactic movement, determines not only the breakdown pattern subsequent to focal lesion but also the progress towards recovery through the aid of external stimulation. Although many questions remain open, for example, the relation between remediation of questions in speech production and the deficit as characterized by the Tree-Pruning Hypothesis (see Friedmann, 1998, for extensive discussion), this development seems very promising.

4. Tentative conclusions and implications

The empirical evidence amassed so far leads to some conclusions.

1. Lesions to Broca's area and its vicinity do not affect semantic abilities, nor do they disrupt basic syntactic abilities. Most notably, Broca's aphasics combine lexical meaning into propositions, create and analyze sentences of considerably complex structure, and are also able to synthesize and analyze words morphophonologically. It thus follows that most human linguistic abilities, including most syntax, are not localized in the anterior language areas – Broca's area and deeper white matter, operculum, and anterior insula.

2. Broca's aphasics do suffer important, though limited, syntactic deficits. Their ability to construct full-fledged tree

structures in production is compromised, as is the link between traces and their antecedents in comprehension. *Processes underlying these highly structured syntactic abilities, and only these, are located in the anterior language areas.*

3. Mechanisms that underlie language production are at least partially distinct from the comprehension device. Parts of both are located in the left anterior frontal cortex, separate but perhaps equal, which is why a lesion there disrupts each partially, and differently. Thus, although Broca's aphasia affects both modalities, strong parallelism between the deficits is not maintained, suggesting anatomical proximity, but functional separation, between production and comprehension mechanisms.

4. Cerebral lesions provide a unique testing ground for linguistic claims. If language knowledge and use are taken to be biologically supported, then a theory of linguistic representation and use must be compatible with patterns of language breakdown. Indeed, there have been several attempts to harness neurolinguistic evidence in support of particular linguistic claims (Beretta et al. 1996; Friedmann & Grodzinsky 1997; Grodzinsky 1984b; Grodzinsky et al. 1991; 1993; Pinango et al. 1999). The richness of our language organ and the very fine patterns of break-down subsequent to brain damage are likely to yield further, ever finer results, provided the search is guided by a well-articulated theory.

5. What language is not: A modular approach

We have shown that Broca's area and its vicinity are home to one central syntactic ability and that other abilities are intact following lesions to this cerebral region, which might indicate that the rest of the grammar resides elsewhere. Where, then, do human syntactic abilities reside? Are they separate from other cognitive skills? Could grammatical transformations be the only neurological expression of a distinct language faculty? These questions are particularly acute in light of the belief, which has come back into vogue, that humans do not possess a special "language organ" (see Seidenberg, 1997, for a recent example). The apparent lack of localization of certain basic combinatorial linguistic abilities may lead to the suspicion that they are distributed over the cerebral cortex. A survey of the neurological record shows that this is not the case. The cerebral localization of syntax as a whole is restricted to the left hemisphere. Moreover, a comparison between language and other cognitive deficits upholds what has become conventional wisdom among linguists: Combinatorial aspects of language are distinct from "general cognition." Linguistic arguments and evidence to that effect have been given (see Chomsky 1995a; 1995b, for recent discussion). In the following sections, some neurological evidence will be reviewed, indicating that, unlike other combinatorial abilities, syntax, though less localized than previously believed, is localized in the left hemisphere and is distinct from other, seemingly related intellectual capacities.

5.1. Language in the right hemisphere

As a first pass we look at the nondominant hemisphere, the right for most humans. We can assert unequivocally that no combinatorial language abilities reside in the nondominant cerebral hemisphere. We have seen one direct test of right hemisphere syntax that hardly detected any activity during exposure to certain syntactic types (Bavelier et al. 1997; Just et al. 1996). Deficit analyses lead to the same conclusion.

Two sources of pathological evidence exist: split-brain patients and patients with damage to their right hemisphere. In the former case, it is by now agreed that "unlike other language functions, complex grammar skills are localized to only one hemisphere" (Lustep et al. 1995). This conclusion follows from failures of a disconnected nondominant hemisphere (left in the patient studied) to understand either active or passive sentences. Although the cerebral organization of split-brain patients may sometime differ from normal functional localization owing to their past history (often beset with childhood seizures), the findings have been consistent: Series of studies by these authors and by others have documented a number of additional failures of the right hemisphere to process syntax correctly (Baynes & Gazzaniga 1988; Baynes et al. 1992; Gazzaniga et al. 1984).

Lesion studies of right-hemisphere-damaged patients have corroborated this time and again. Van Lancker and Kempler (1987) compared the performance of right- and left-hemisphere-damaged patients in the comprehension of idioms and familiar phrases to their ability to comprehend novel sentences, which may touch on the syntactic impairment of aphasics. The comprehension of "familiar phrases" may require an ability to extract nonliteral meaning, but no combinatory capacity, insofar as these phrases are presumably stored in the mental lexicon. Novel sentences, by contrast, cannot be stored, and their comprehension requires analytic mechanisms. Subjects were asked to match pictures to sentences such as *he is turning over a new leaf,* where the pictures related either to the literal or the metaphoric meaning of the expression, and to sentences such as *when the angry girl pushes, the happy boy swings,* where the distractor picture contained reversed thematic roles. Although the precise syntactic details of the materials used are unfortunately not reported, the comparison between right- and left-hemisphere-damaged patients still resulted in an interaction: The right-hemisphere-damaged patients, worse than aphasics on the comprehension phrases with some metaphoric value, were much better than the aphasics in the comprehension of unfamiliar sentences, for which they had to use their syntactic knowledge. Similarly, Zaidel et al. (1995) have shown that right-hemisphere-damaged patients, but not their left-hemisphere-damaged counterparts, are near normal in their perception of syntactic ambiguities. This finding is especially interesting given the presence of long-distance dependencies in the stimuli, such as *the elephant is ready to lift,* which the patients successfully detected.

A series of related studies focuses on the linguistic abilities of right-hemisphere-damaged patients. Here the relevant evidence is less direct. Brownell and his colleagues (Brownell et al. 1992; 1997; Joanette & Brownell 1990) have long been looking at properties of the overall communicative skills of these patients (with an intact left hemisphere), who are deficient in a number of ways, notably in the capacity to integrate aspects of discourse, metaphor, and other communicative conventions. Notably, no results could have been obtained had the patients not been attuned to complex syntax, because in most cases the tasks require understanding complex stories and answering questions afterwards. Major syntactic deficits would preclude success in these experiments. However, right-hemisphere-damaged patients were able to perform these tasks, demonstrating their combinatorial capacity in the language domain. Thus, the evidence is that this side of the brain has an important role in communication but makes no syntactic contribution to language use.

5.2. Language and mathematics: Two distinct combinatorial capacities

Language resides in the left hemisphere, yet, given that the neurological organization of combinatorial linguistic operations turns out to be more widespread than was previously believed, suspicions immediately arise regarding its distinctness from other formal operations. That is to say, linguistic abilities may not be distinct but, rather, may follow from a general capacity to form complex combinations. In particular, mathematics and language may be the same.

These problems concerned several great European neurologists (Hecaen et al. 1961; Henschen 1920; see also Boller & Grafman 1983 and Kahn & Whitaker 1991, for historical reviews), to whom it was clear that deficits in mathematical abilities must be set apart from language, memory, or attentional problems. They realized that intact mathematical abilities can easily be masked by deficits to cognitive systems that are normally recruited for mathematical tasks. Thus, in the early days, Hecaen et al. (1961) proposed in their pioneering work a distinction between primary (independent) and secondary (consequent) acalculia. The former – our current object of inquiry – is a varied disturbance. One kind is an "impaired spatial organization of numbers" (Levine et al. 1993) in which patients tend to misalign digits while carrying out basic arithmetical operations; another is "anarithmetria," an inherent inability to carry out calculation (Benson & Weir 1972). Critically, Hecaen et al. showed that this is a neurological entity in its own right, paving the way for serious discussion and investigation.

The current record appears unequivocal. Both the clinical and the experimental evidence point to the functional independence and neurological distinctness of mathematical and linguistic capacities. Ideally, we would like to observe language impairment with a retention of the ability to carry out complex mathematical operations, and vice versa. Unfortunately, brain damage of any type, even in patients who had been skilled mathematicians, makes it very difficult, if not impossible, to carry out cognitive tasks of high complexity. Although a direct result is hard to produce, we do have, at this time, a fairly rich array of evidence for both the functional and the neuroanatomical independence of linguistic and mathematical combinatorial abilities from studies of arithmetical skills. Two points can be made:

1. Neuroanatomical loci of mathematical skills, though reasonably spread, all appear to be retrorolandic and are probably bihemispheric. That is, apart from some exceptional cases, only lesions in parietooccipital regions (most often in the left hemisphere but sometimes on the right) and certain temporal regions can bring about primary acalculia (see Boller & Grafman 1983; Kahn & Whitaker 1991; Levine et al. 1993).

2. Anterior aphasia is functionally dissociated from primary acalculia (and dyscalculia); likewise, primary acalculia is dissociated from anterior aphasia or from damage to combinatorial linguistic skills (Grafman et al. 1982; Levine et al. 1993; Rosselli & Ardila 1989).

Two of the more salient experimental results supporting these conclusions are reviewed below. Grafman et al. (1982) studied more than 100 brain-damaged patients with cerebral lesions in various loci, whom they asked to carry out arithmetic tasks involving the four basic operations, with increasing difficulty (up to problems such as $835 + 98{,}279$; $60{,}100 - 4{,}712$; 308×73; $8{,}694 \div 69$). They then evaluated the linguistic abilities of these patients on the Token Test (De Renzi & Faglioni 1967), tested them for constructional apraxia, and evaluated their intellectual abilities with the Raven Progressive Matrices Test. They found that, when age and educational level were controlled, patients with left posterior lesions performed significantly worse than all other groups on the arithmetical tests, whereas there was no significant difference between right-hemisphere-damaged patients, controls, and left-anterior-lesion patients. Only the left-posterior-lesion patients had mathematical problems that could not be attributed to linguistic, attentional, or other neuropsychological deficiencies.

It is critical to note that the scores of the anterior Broca's aphasics ($n = 30$), though significantly better than those of the posterior-lesion patients, were not significantly different from those of controls. Although we have no further details regarding these patients, based on the fact that most of them ($n = 22$) suffered a vascular accident, we can suppose that at least the majority were of the Broca's variety. Broca's aphasia, then, does not cooccur with acalculia (see also Dahmen et al. 1982, for similar conclusions).

Next, consider a study by Rosselli and Ardila (1989), who analyzed more than 60 brain-damaged patients, carefully divided into several clinical categories. The patients carried out a large array of tasks involving numbers: reading, writing to dictation and related tasks, mental computing of an orally presented problem in arithmetic (e.g., $55 + 38$; $93 - 13$; 13×12; $150 \div 30$), solving simple and complex problems in writing (up to $689 + 437$; $421 - 277$; 212×37; $818 \div 356$), reading arithmetical symbols, and counting. Broca's aphasics (diagnosed with the Boston Diagnostic Aphasia Exam; Goodglass & Kaplan 1983) made many errors on tasks requiring linguistic abilities (tests 31.1 and 31.2 below), in most instances more than other types of aphasics, but their error rate on tests of complex arithmetic (tests 31.3 and 31.4) was the lowest of all the aphasics, as shown in (31), adapted from Rosselli and Ardila (1989):

(31) Test (% error)

Patient type	1. Reading	2. Writing	3. Mental	4. Complex writing
Broca	28.8	33.3	55.0	45.6
Conduction	33.3	31.1	68.7	56.8
Wernicke	20.0	17.7	66.2	68.0
Anomia	13.3	13.3	75.0	72.0

Importantly, nonaphasic brain-damaged patients (notably prerolandic right hemisphere-damaged patients) also made a number of errors on the complex arithmetical tests.

Anterior aphasics, then, are mostly free of disorders of arithmetic (as long as these do not depend on language skills). The reverse claim appears to be true as well: Patients with primary acalculia have no aphasia. There are a number of case studies of acalculia, including cases with the elusive Gerstmann syndrome (a tetrad consisting of finger agnosia, acalculia, agraphia, and left-right disorientation; see Strub & Geschwind 1974) and others in which a disorder of mathematical ability did not cooccur with aphasia (Benson & Weir 1972; Lucchelli & De Renzi 1993; Selnes et al. 1991). Interestingly, and consistent with the previous results, naming problems were apparent in most of these cases, whereas speech output, comprehension, and repetition were intact.

Fault can easily be found in each of these studies. Their methodologies can be criticized, as can the diagnostics and patient selection, but it is very hard to ignore the uniform picture that emerges from every angle reviewed: Primary mathematical deficits do not seem to co-occur with language deficits other than those pertaining to the lexicon. With very few exceptions, the anterior portion of the left cerebral hemisphere does not house the neural substrate for mathematical abilities; linguistic deficits of the Broca's variety, by contrast, are a consequence of lesions in this location. The neurological viewpoint, then, shows that central combinatorial linguistic abilities are distinct from the combinatorial abilities pertaining to mathematics.

5.3. Broca's area and general intellectual capacities

A nonmodular view of language would attempt to make linguistic principles follow from theories of some other cognitive domain, of which general intelligence is a prime candidate. To show modularity, by contrast, is to show the distinctness of intelligence from combinatorial linguistic ability. An ideal neurological demonstration would show that the distribution of intelligence (measured by an IQ test of some sort, or some approximation thereof) in the aphasic population is identical to that in the general population. We are not likely to obtain data of this type; large-scale IQ scores of aphasic patients are neither available nor easily collected. Comparisons between the pre- and postonset IQ measures of an aphasic group of patients (with some other brain-damaged controls) have not been made either. Direct evidence bearing on the question of language and intelligence, then, is not forthcoming.

Several studies, however, provide indirect demonstrations that intellectual capacity is truly distinct from language. Through different methods, they underscore the same point: Most aphasias – Broca's, Conduction, and Anomia, but not Wernicke's – and intelligence are independent. This was first demonstrated by Kertesz and McCabe (1975), who compared aphasia type with intellectual ability of more than 100 aphasic patients. General intelligence was measured by the Raven Colored Progressive Matrices Test, a standardized measure that contains nonverbal measures of general intellectual skills and is correlated with verbal IQ. Aphasia was diagnosed by the Western Aphasia Battery, a test that scores patients on fluency, repetition, comprehension, and naming and divides them into the usual clinical categories. The mean scores in each clinical category were then compared to a neurologically intact control group. The result was that for Broca's (n = 27), Conduction (n = 11), and Anomia (n = 40) aphasics, the scores on the intelligence test were not different from those of the normal controls, whereas the other clinical categories (Global, Wernicke's, and transcortical) were significantly lower. Similarly, Bailey et al. (1981), who correlated scores on the Raven Matrices with a measure of severity of aphasia in a longitudinal study of more than 50 aphasics (of unknown clinical category), found that, although the linguistic abilities of the patients improved rather significantly over a 9-month period, their scores on the intelligence test remained unchanged.

A different angle can be found in Smith (1980), who constructed a task involving some kind of "nonverbal reasoning," in which the subjects were supposed to grasp relations such as "greater than" through a nonverbal demonstration of the experimenter on a set of wooden rods, and then carry them over, by way of analogy, to other domains (e.g., weight), where they were expected to solve a problem. Four of these patients seem to have been Broca's aphasics, and their performance was quite good, despite their language deficiency.

Finally, a comparison between IQ and auditory comprehension in a group of 98 aphasics (18 of which were Broca's aphasics) was made by Borod et al. (1982). The Broca's aphasics were the only ones to be well above the group mean in their scores on the IQ test, producing the highest scores, whereas the Wernicke's and global aphasics were the lowest (the latter being significantly below the whole group).

Each of these studies has its problems and deficiencies. The measures of language skills that were used are questionable; the diagnostics may have been imperfect; some relied on dubious tests of intellectual capacity; and doubts can probably be cast on at least some of the statistical tests. Still, the fact remains that, no matter how aspects of language and intellect are assessed, the same result is repeatedly obtained: Language deficits and intelligence in Broca's aphasia are distinct and independent; Broca's aphasics, although linguistically inferior to neurologically intact controls, are not different from them intellectually. This is also true of those suffering from other aphasias, namely, conduction aphasia and anomia (which are consequences of damage to distinct cerebral loci, the arcuate fasciculus and the angular gyrus, respectively). Moreover, sparing of intellectual skills does not seem to occur in all aphasias. Whatever one might think of the nature of the deficit in Wernicke's and global aphasias, they certainly do not represent an exclusive failure of grammatical devices, and they involve (at the very least) some lexical semantic and probably other disruptions to knowledge. Indeed, intellectual capacities are negatively affected in these syndromes.

6. Coda: Broca's legacy and the role of the language areas

Paul Broca, founder of modern neuropsychology, had three central ideas regarding the cerebral organization of cognitive functions. Recast in modern terminology, he said that (1) there is a one-to-one relation between neural substrate and behavioral function (at least for language), (2) language is distinct from other cognitive capacities, and (3) language resides in the left cerebral hemisphere and not in its homolog on the right. Broca's legacy – *modularity, neural representation,* and *lateralization* – is here to stay. After nearly 140 years, and thousands of theoretical, clinical, and experimental articles, we can conclude rather confidently that all three properties Broca ascribed to brain/language relations are true: There is a distinct and dedicated "language organ" in the human left cerebral hemisphere.

To find the language loci and characterize them precisely, we must continue to re-"redefine" the language centers. Linguistic theory is the best tool currently available for this job. Once it is used, we discover that Broca's area is more specialized than previously supposed. It handles only intrasentential dependency relations. This endeavor has another important potential benefit: It may help us to discover natural classes within our grammatical system and test the biological feasibility of grammatical theories. When we discover a pattern of grammatical impairment and sparing, it may reveal something about the internal structure of the grammar. Such arguments have been made in several domains (see, e.g., Grodzinsky et al. 1991; 1993; Hickok & Avrutin 1995). One hopes that more work will lead to further discoveries.

We may thus conclude that Broca's success was accompanied by one major error. He looked at communicative activities, ascribing no neural or cognitive value to the structure of the linguistic signal. When this mistake is corrected, his legacy can be preserved and, in fact, enriched significantly. At the same time we obtain a new, more precise, and deeper picture of the cerebral representation of linguistic functions. In this view, language is a distinct mental and neural faculty, with an inherent structure comprising a rich knowledge base and a processing device that implements it; this faculty resides in the left hemisphere. Yet, although Broca's area (and its surrounding left anterior neural tissue) is highly specialized and important, language is mostly not there.

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Which grammar has been chosen for neurological feasibility?

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Abstract: Grodzinsky's hypotheses need different theories of grammar for comprehension and for production. These predictions are undesirable. Hungarian data are incompatible with the Trace Deletion Hypothesis.

Comprehension, production, and theories of grammar. If Grodzinsky were right, we would need different theories of grammar for comprehension and for production. Grodzinsky proposes his Trace Deletion Hypothesis (TDH) to interpret comprehension deficits applying Government and Binding Theory – cf. Examples (4), (5), and (11) in sects. 2.2. and 2.3. For production deficits he proposes pruned syntactic trees in the framework of the Minimalist Program (sect. 2.7.4). Differences between Government and Binding (GB) theory and Minimalist Framework cannot be neglected.

In GB theory, the inventory of transformations has been reduced to a nonconstruction-specific rule of Move to derive correct surface structure and linear order. Because of the GB framework, for TDH the structure of a simple English clause is roughly the following – cf. 4(a) and (b) and 5(a) and (b) in section 2.2.:

$$[_{\text{SUBJECT}}\text{NP}_i\ [\text{VP}\ [t_i\ \text{V}\ _{\text{OBJECT}}\text{NP}]]$$

In the Minimalist Program, Move is conceptualized as a requirement of legibility conditions imposed on language by external mind/brain systems. In the course of computation of sentence structure, uninterpretable formal features of lexical items must be erased in a local structural relation with a matching feature of another lexical item. Move is required to create the local structural relation for checking features of lexical items and to assure that only interpretable features exist at the semantic and phonetic interface with mind/brain systems (Chomsky 1997a, p. 18). Thematic roles are assigned to phrasal constituents, D(eterminer) P(hrase)s in their root positions under lexical VP (a phrase headed by lexical verb). Then all DPs (subject, object, indirect object, etc.) having a case feature must be moved from lexical VP into a higher Agreement Projection to check their case features. After the movements have been performed, depending on lexical Verb type, English clause structure is very roughly the following:

$$[_{\text{AgrSP}}\mathbf{DP_i}[_{\text{TP}}\boldsymbol{t_i}[_{\text{vp}}\mathbf{V_j}\boldsymbol{t_i}[_{\text{AgrIOP}}\mathbf{DP_k}[_{\text{AgrOP}}\mathbf{DP_m}[_{\text{VP}}[\boldsymbol{t_k}[_{\text{V}'}[\boldsymbol{t_j}\ \boldsymbol{t_m}]]]]]]]]$$

($\boldsymbol{t}$ = trace; Agr[S/O/IO]P = agreement phrase for Subject, Object, Indirect Object, respectively; TP = Tense Projection; vp = phrase headed by a light verb; VP = phrase headed by a lexical Verb).

In this model we have a derivation in which object/Theme or dative/Recipient and so forth must be moved out of their root, thematic position. What does TDH predict in the Minimalist Framework? TDH would predict that only sentences containing one and only one argument, which is subject/Agent, will be interpretable by the Default strategy for Broca's aphasics. In the rest of the sentences the default strategy does not help: The fronted DP can be considered as Agent, but *no other* DPs have a theta role, because they have been moved. Hence the agrammatic theta assignment characterised in 4(a), 5(a), and 11(a) in sections 2.2 and 2.3 is impossible in the Minimalist Framework (because object/Theme constituents must be moved). TDH cannot be applied in the Minimalist Program; TDH needs GB theory.

Empirical problems. In grammaticality judgment tests patients were able to judge a "filled gap" condition (sect. 2.4.1.) correctly. For correct judgments the position of trace for a moved Wh-element must be preserved: Who did John see *t*? versus Who did John see ***Joe*? The Wh-element *has* the thematic role *Theme* assigned to it in root position. TDH predicts that a moved Wh-element and its trace cannot be linked to each other in sufficient time and patients should be disturbed by unavailable thematic roles. If trace was erased or unavailable how were patients able to judge the *ungrammatical* sentences of "filled gap" conditions correctly? (Movement and trace are relevant, but whether or not Wh-element is referential does not play a role here, and there is no long dependency as in "Wh-movement/that-trace condition"). It is surprising that the grammatical system of patients is capable of creating chains containing traces, but does not tolerate traces themselves.

For impairments in language production the Tree-Pruning Hypothesis (TPH) is proposed in the Minimalist Program. Under TPH, all other syntactic rules that depend on nodes of the higher part of a syntactic tree beyond Tense Projection will fail in production. According to the split-inflection hypothesis of the Minimalist Program, subject-agreement phrase projection (=AgrSP) occupies a higher position in a syntactic tree than Tense Projection, at least in English. On this basis the TPH predicts that English-speaking agrammatic aphasics should make many more errors on subjects of sentences than on objects or indirect objects. The retrospective literature review (sect. 2.7.3.) does not refer to this dissociation.

Hungarian data. Thematic roles are not configurationally identified in the Hungarian sentence; they are identified on the basis of *case endings.* The structure of the Hungarian VP is flat: All arguments of the verb are sisters of the verb, which is left peripheral in the phrase. In Hungarian, virtually any phrase can move out of its VP-internal, thematic position where it binds a trace. These fronted phrases may appear preverbally in a number of orders. Hungarian does not have a subject position at all; there is a reiterable topic position (É. Kiss 1994). The empirical observation of the linear order of constituents in a simple sentence is the following:

$$XP^*_{topic}\ XP_{quantifier}\ XP_{focus}\ [_{VP}V\ XP^*]$$

(* = unrestricted number of phrasal constituents XP)
The topic position [T] is recursive; multiple topic constructions are allowed. Hungarian sentences with topics *with different theta roles* can be equally grammatical. A Hungarian example:

$[_T$Robi-nak a pénz-t Mari tegnap] odaadta.
$[_T$Bob-dat(RECIPIENT) the money-acc(THEME) Mary-nom(AGENT) yesterday] gave-3sg
"Mary gave the money to Robert"

This means that no Default Principle can apply in Hungarian.

The TPH, however, is highly compatible with our data on speech production by Hungarian Broca's aphasics, provided the syntactic tree can be pruned from functional projections of Quantor Phrase, Negation Phrase, Focus Phrase, and Tense Phrase, as well, according to degrees of impairment in language production.

Trace deletion and Friederici's (1995) model of syntactic processing

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Abstract: This commentary discusses the relation between Grodzinsky's target article and Friederici's (1995) model of syntactic processing. The two models can be made more compatible if it is assumed that people with Broca's aphasia have a problem in trace construction rather than trace deletion, and that the process of trace construction takes place during the second early syntactic substage of Friederici's model.

Grodzinsky's target article is a linguistically sophisticated account of the neural basis of syntactic processing. Another such account is that of Friederici (1995). I would like to consider whether these two accounts can be brought more closely together than it seems at first sight.

Let us first consider the conceptual question of "Why traces?" In other words, what makes it possible for traces to be selectively impaired relative to some other syntactic phenomena? Some of the gap between Grodzinsky's and Friederici's models can be bridged if we assume that the relevant problem in Broca's aphasia is in the construction of traces rather than in their deletion.

1. Why traces? Grodzinsky argues for a very specific connection between language behavior, linguistic representation, and brain function: One specific task of Broca's area is the processing of syntactic traces. In a larger context this connection may turn out to be arbitrary. After all, there is a good deal of arbitrariness in the development of biological systems. Alternatively one may try to relate this connection to other current assumptions about the neural basis of syntactic processing. This commentary is a partial step in the latter direction.

2. Friederici's (1995) model. In her 1995 paper, Friederici proposes that syntactic processing is composed of at least four subprocesses: a first-pass construction of syntactic structure, based on information restricted to the syntactic categories of the participating lexical elements (noun, preposition, etc.); a second syntactic stage that adds lexical syntactic information such as subcategorization and thematic selection (see sect. 4); a semantic stage that adds lexical semantic information; and, if necessary, a late revision of the initial syntactic structure, based on this and other syntactic and semantic information. Each of these four stages is indexed by a brain wave that differs in either latency or topography from each of the other three.

3. Deletion versus initial construction. Conceptually, the processing of a trace must achieve at least three subgoals: the construction of an empty syntactic constituent (called a trace or a gap), the finding of a syntactic constituent that the trace is related to (called an antecedent or filler), and the establishment of an appropriate connection between the antecedent and trace.

Grodzinsky assumes that people with Broca's aphasia fail to establish a connection between antecedent and trace because the initial trace is deleted. If I am not mistaken, however, the main claims of this target article could be equally made by appealing to a problem in trace construction rather than trace deletion.

4. Traces and lexical syntactic information. Both Grodzinsky and Friederici assume a distinction between syntactic categories and other types of syntactic lexical information. For example, both a transitive verb like "kick" and an intransitive verb like "smile" have the syntactic category "verb." But only the transitive "kick" lexically specifies that it has to have a second noun phrase as an object (subcategorization) with the thematic role of "patient" (thematic selection). Now, traces or gaps are inaudible constituents. This makes it harder to establish their presence in the syntactic structure. The question of how they *can* be established depends to a great extent on the particular model one assumes for syntactic processing. One thing seems relatively clear: However gaps are established, they cannot depend on syntactic categories alone. For example, in the example above, "kick" allows for an object gap but "smile" does not.

5. A partial synthesis. Friederici assumes that traces are established in the very first stage of syntactic processing, the one where only syntactic categories are available. In light of the argument above, this seems somewhat implausible. Suppose, then, that one makes the following two assumptions: As described in 3, one can assume that people with damage to Broca's area have trouble constructing syntactic traces, not maintaining them. As described in 4, one can assume that traces are established during the second substage of syntactic processing, the one where lexical syntactic information becomes available. With these two assumptions, one reason Broca's aphasics have trouble with traces but not with other syntactic phenomena might be because traces are processed by a different subprocess than those other phenomena. This synthesis goes some way toward explaining what is special about traces and their processing, as well as allowing one to find some connection between Grodzinsky's and Friederici's models.

6. Some open questions. What this hypothesis fails to explain is what distinguishes trace processing from other processes that take place during the second stage of Friederici's (1995) model, in particular, those that also involve lexical syntactic information. For example, it does not explain why Broca's aphasics are able to determine that a sentence like "The children threw" is ungrammatical. One possible hypothesis would be that the creation of traces is different from other uses of lexical syntactic information, because it must occur no later than the processing of connections between antecedents and traces. In other words, this is in a sense the mirror image of Grodzinsky's trace deletion hypothesis: Trace deletion assumes that lexical syntactic information is available too *early* to be useful for establishing a connection between antecedent and trace, whereas the new hypothesis assumes that this lexical syntactic information is available too *late.* Broca's aphasics might be able to access lexical syntactic information by some compensatory mechanisms that operate outside Friederici's second processing stage, but at that point in time it is no longer useful for establishing a connection between antecedents and traces. To decide between those two options, one needs more detailed information about the nature and time course of syntactic processing in Broca's aphasia. Until we have this, the question is open, and waits for further research.

Why the TDH fails to contribute to a neurology of syntax

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Abstract: An important part of Grodzinsky's claim regarding the neurology of syntax depends on agrammatic data partitioned by the Trace Deletion Hypothesis (TDH), which is a combination of trace-deletion and default strategy. However, there is convincing evidence that the default strategy is consistently avoided by agrammatics. The TDH, therefore, is in no position to support claims about agrammatic data or the neurology of syntax.

Grodzinsky has probably done as much as anyone to put syntax center stage in neurolinguistics. He has also been at the heart of the arguments sustaining the credibility of Broca's aphasia as a syndrome critical to the neurolinguistic enterprise. These laudable efforts have enabled substantial progress to be made. In particular, it looks as if referential dependencies of certain kinds are central to an explanation of receptive patterns in Broca's aphasia. However, whatever evidence there is that Broca's area houses these dependencies (and little else), the Trace Deletion Hypothesis (TDH) does not provide it. This is because the TDH does not work.

"Syntactic movement," it is claimed, "is disrupted along the lines of the TDH" (sect. 2.2, para. 12), and "these TDH-based observations force a new view of the role of [Broca's area]" (sect. 2.2, para. 13). Fine, perhaps, *if* the TDH is right. But if the TDH is wrong, then observations based on it cannot support a functional claim regarding Broca's area. So let us now see why the TDH is clearly and demonstrably wrong.

Beretta and Munn (1998) sought to isolate and test the TDH default strategy, using a sentence-picture matching task in which one of the pictures matches the meaning arrived at by the default strategy. Recall that the default strategy claims that Broca's aphasics have a representation that involves two Agents in passive sentences. Why is this necessary? According to Grodzinsky (1990), it "is necessary because patients are faced with a task that forces them to map every NP in a given sentence onto a representation of a depiction of a real-world event" (p. 136). And what task is it that patients are faced with? A standard sentence-picture matching task that depicts only two participants in an action of, for example, *hitting*, in which there must be an Agent and a Theme. In this standard task, two Agents could not possibly be performing the same action. The real world could not possibly match the double-Agent representation the TDH claims Broca's aphasics have. That is the reason Broca's aphasics have no option but to choose something that conflicts with the representation that results when the syntax and the strategy assign the Agent role to different NPs.

What Beretta and Munn did was to make the real world match the double-Agent representation. They designed a task in which the real-world event did not force patients to make a choice that conflicted with their hypothesized double-Agent representation. By modifying the standard task so that there were *three* participants in each action (two Agents and one Theme), at least one of the pictures actually matched the double-Agent representation, and it was thus possible to test the default strategy unambiguously. The picture in which both Agents mentioned in a passive sentence were depicted as Agents of the action (e.g., *hitting*), though mistaken, should have been seized on by Broca's aphasics.

But they did not seize on it. All six patients performed in a strikingly uniform manner. They all scored highly on the control three-picture (two Agents, one Theme) task with active sentences, as expected, showing that whatever their results on the passives, they were not an artifact of the three-picture task. And the results on the passive sentences were clear: Patients guessed between the two pictures that did not match the double-Agent representation. Not only did they fail to seize on the two-Agent interpretation that the task clearly permitted, but they shunned it with all their might. This is exactly what would have been expected if there had been *no* second Agent in any of the pictures, and exactly the opposite of what would have been expected if there *had* been a second Agent in any agrammatic interpretation. Therefore, the results decisively refute the default strategy. In view of this, it cannot be claimed that syntactic movement is disrupted along the lines of the TDH or that TDH-based observations force a new view of the role of Broca's area.

But the results tell us something highly constructive, too; namely, that a syntactic account of the aphasic deficit is still clearly implicated. The locus of the deficit appears to be in the mapping between syntactic representation and the theta grid of the verb. After all, patients did not treat the three alternatives equally. This demonstrates that Broca's aphasics know the lexical representation of the verbs. However, they do not have intact theta-role mappings to *either* of the two NPs in a simple passive sentence – if they had the correct mapping to one role, they would have been able to infer the other. This is entirely consistent the Double-Dependency Hypothesis (DDH) (Mauner et al. 1993) in which guessing arises when there are two referential dependencies to be computed.[1]

NOTE

1. The coverage of the DDH, *pace* Grodzinsky (sect. 2.6, para. 3), is at least as broad as the coverage of the TDH (for details, see Beretta et al. 1999).

Sentence comprehension in Broca's aphasia: A critique of the evidence

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Abstract: The argument that Broca's area is preferentially involved in specific syntactic operations is based on a strong assertion regarding patterns of sentence comprehension found among patients with Broca's aphasia. This assertion is shown to be largely inconsistent with the available evidence from published studies, which indicates that only a subgroup of Broca patients demonstrate the target pattern.

Agrammatic Broca's aphasia has captured the imagination of many researchers hoping to understand the relationship between specific brain regions and discrete language functions. Early attempts at linguistic description were motivated by agrammatic patients' language production, but recently the emphasis has shifted to their comprehension. Grodzinsky offers an interpretation of the comprehension failures of Broca's aphasic patients that has substantial scientific impact: It is at once a claim about which regions of the brain are important in language processing, and about which formalization of linguistic theory is correct. It seems important, therefore, to examine the empirical basis of his account.

Grodzinsky's hypothesis about the relevance of Broca's area to syntactic processing had its genesis in the following assertion: Patients classified as agrammatic Broca's aphasics (on the basis of their language production and their clinically assessed comprehension) will show a sentence comprehension deficit characterized by chance performance on sentences that require transformations, and good performance on all others.

Support for this claim requires a very strong association between Broca's aphasia and the predicted pattern of comprehension impairment. The appearance of nonconforming cases would seem to constitute falsification of the hypothesis that Broca's area plays a privileged role in a specific linguistic operation. Grodzinsky states that the claim is supported by "massive evidence" (sect. 2.1); counterexamples are dismissed as arising from poor patient selection or other methodological problems.

The evidence available in the published literature regarding the comprehension of active and passive structures (the sentence type most often assessed) provides a different picture. Although the prediction is that active sentences will be understood normally (above chance) and passive sentences poorly (below chance), only about one-third of the Broca's aphasics described in the literature prior to 1993 showed that pattern (Berndt et al. 1996). Of the remainder, about half showed above-chance performance for both types, and the other half showed comparable difficulty (at chance) with both types. Grodzinsky et al. (1999) criticized this literature analysis, arguing that the heterogeneous results reflected poor patient selection in the studies reviewed. That is, the patients who deviated from the pattern were not really Broca's aphasics. However, a study that used exactly the classification test that Grodzinsky favors to select patients, supplemented by detailed lesion data, reported that 5 of 7 Broca's aphasics performed better than chance on both active and passive sentences; 3 of them scored 100% on passives (Goodglass et al. 1993). It is interesting that the only Broca's aphasic who demonstrated the predicted pattern had a lesion, not in Broca's area, but in the temporal lobe.

Even when Broca's aphasics demonstrate the expected pattern for active and passive sentences, they may fail to show problems with the other structures that are argued to be related (Berndt et al. 1997). This possibility has been investigated by selecting patients for attention whose comprehension performance shows a "syntactically principled deficit." That is, Broca's aphasics are included for study of more complex structures only if they demonstrate the relevant pattern for active and passive voice sentences. This preselection of patients who show the predicted pattern on active and passive sentences, followed by the testing of structures that are hypothesized to be related, constitutes a reasonable approach to evaluating Grodzinsky's claims. However, data regarding the active/passive comprehension of these preselected patients cannot be used to evaluate the *prevalence* of active/passive comprehension impairments in Broca's aphasia, as Grodzinsky and colleagues (1999) have done (see Berndt & Caramazza 1999, for discussion). For example, Figure 1 of the target article, which is used to demonstrate the frequency of occurrence of the predicted distributions of performance for actives and passives, includes data from 19 patients who were preselected to show the target active/passive pattern! Although we are rarely told how many Broca's aphasics were screened during this preselection process, Beretta and Munn (1998) report that 6 of 15 Broca's aphasics they tested showed the desired pattern. This number is not far from the estimate generated by Berndt et al. (1996) that about one-third of Broca's aphasics will show the predicted pattern on active and passive voice sentences. So the evidence favoring a strong association between Broca's aphasia and active/passive comprehension is not "massive"; rather, it is quite weak.

One additional point that can be raised concerning the privileged role for Broca's area in syntactic processing is the "syndrome specificity" of the patterns at issue (sect. 2.6). Clearly, patients with lesions in other brain areas should not show the same patterns of performance as Broca's aphasics. Grodzinsky has focused on studies of Wernicke's aphasics as the comparison group, which seems a strange choice. The comprehension pattern at issue here requires good comprehension of single word meanings (and concomitantly good comprehension of sentences that can be understood without the necessity of detailed syntactic analysis). Wernicke's aphasics are unlikely to show a pattern comparable to Broca's aphasics because their word comprehension may be too poor. However, many studies of aphasic sentence comprehension have noted that the impairments of conduction aphasics and Broca's aphasics are very similar (Caramazza & Zurif 1976; Goodglass et al. 1993; Heilman & Scholes 1976), suggesting that a systematic study of this patient group (yet to be done) might find a distribution of impairment by sentence type that is indistinguishable from that of Broca's aphasics.

It seems that the comprehension pattern based on transformational requirements is not strongly associated with the language production profile that constitutes agrammatic Broca's aphasia or, by implication, with the lesion that causes Broca's aphasia. A majority of patients with the relevant production profile (and, presumably, lesion site) do not show the target comprehension pattern. In fact, it does not appear that Broca's aphasics demonstrate a characteristic comprehension pattern that is not also found among other types of aphasic patients. It may well be that there are individual patients whose comprehension fails in exactly the manner predicted and for precisely the linguistic reasons offered by Grodzinsky. But lacking a general finding for Broca's aphasics, one that is specific to that group, the hypothesis that Broca's area has a privileged role in syntactic processing seems to be an overinterpretation of the available data.

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Broca's demotion does not doom universal grammar

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Abstract: Despite problems with statistical significance, ancillary hypotheses, and integration into an overall view of cognition, Grodzinsky's demotion of Broca's area to a mechanism for tracking moved constituents is intrinsically plausible and fits a realistic picture of how syntax works.

On the whole, Grodzinsky makes a good case for supposing that the role of Broca's area in syntax is more specific yet much more circumscribed than has been widely supposed. Moreover, his proposal that its main function lies in comprehension, and involves linking moved constituents with their extraction sites, makes a lot of sense in terms of the language faculty as a whole. If the copying theory of movement (Marantz 1995) is correct, there is no need for the speaker to keep track of these links – that will be done automatically by the fact that, throughout the combinatorial process that produces sentences, right up to their moment of utterance, the same constituent must somehow be represented at both its landing and extraction sites. It is the hearer who has the problem, that of deciding, after the extraction-site copy has failed to receive phonetic form, which of the phonetically represented sentence constituents has been moved, and from where. Therefore, it is only logical that there should be some task-specific, comprehension-specific mechanism for solving this problem.

Good though Grodzinsky's leading idea is, its execution raises some quibbles. The first concerns his use of statistics. Only once (sect. 2.7.3, Ex. 27) do we get raw scores and percentages; for the rest, we are offered only "above chance" and "chance," with no criteria or cutoff points and no way of knowing whether, in any given case, the difference between these two categories is even statistically significant.

Another issue concerns the claim that Broca's area is also involved in producing the upper parts of syntactic trees. In contrast to Grodzinsky's major claim, this just does not make a lot of sense in terms of the overall syntactic process. First, it is not obvious why the same area should subserve both an interpretative (linking traces with antecedents) and a combinatorial (adding tree nodes above NegP) function, especially when (as Grodzinsky argues elsewhere) combinatorial processes in general are handled outside Broca's area. Second, Grodzinsky presupposes the kind of expanded tree structures originally proposed by Pollock (1989); these, even before the advent of the Minimalist Program, were criticized by Iatridou (1990), and even if Chomsky (1995c) seems at least implicitly to endorse them, they are surely against the whole spirit of minimalism. Their main purpose was to provide additional landing sites for movement, and according to Chomsky (1997b, p. 191) "you only do overt movement if there's no other way for the derivation to converge." One suspects that Pollockian trees, like many other theoretical artefacts, will have limited durability, rendering it unwise to tie them in with permanent aspects of brain function.

However, there is a third and more fundamental objection to the claim that Broca's area handles high-end tree structures. The ancillary claim that Broca's aphasia "leaves everything below [the Tense node] intact" (sect. 2.7.3) flies in the face of countless examples of agrammatic speech in which "everything below the Tense node" is as disorganized as anything above it:

(1) Lower Falls . . . Maine . . . Paper. Four hundred tons a day! And, ah . . . sulphur machines, and, ah . . . wood. . . Two weeks and eight hours . . . workin' . . . workin' . . . workin'. . . Yes, and ah . . . sulphur (Goodglass 1973)

(2) Ball, prince, um, shoe . . . scrubbed and uh washed and uh . . . tidy, uh, sisters and mother, prince, no, prince, yes (Schwartz et al. 1985).

(Note that in the second example, Tense – if little else – appears intact.) Moreover, Grodzinsky's own analysis argues against the claim. According to him the appearance, in Dutch aphasic speech, of uninflected verbs in sentence-final position is a result of the fact that, with no tree structure above NegP, there is no T(ense)^0 to which the verb can raise. In the tree given as example (28) (sect. 2.7.4), however, there is an Agr(eement)^0 node to which the verb must raise in the course of the derivation to check agreement features on its way to its final landing site T^0, and a Spec-of-Agr node above it, to which the subject NP must also raise for the same purpose. Thus if the lower portion of the tree remains intact, as Grodzinsky claims, there would be every reason for Dutch aphasics to at least get verb and subject into the right relative positions (even if the verb remained uninflected). If they cannot do this, their behavior simply reinforces the evidence of (1) and (2), suggesting that the entire syntactic tree is compromised.

Finally, a logical problem arises from Grodzinsky's treatment of the relationship between syntax and other cognitive faculties in section 5. When he has spent most of his target article arguing that Broca's area is *not* implicated in the combinatorial processes that produce sentences, how can he then suppose that the high performance of Broca's aphasics on mathematical and other cognitive (nonlinguistic) tests demonstrates the independence of the neural infrastructure that subserves syntax from that which subserves other cognitive tasks? In fact, it demonstrates that math is not done in the place where traces of moved constituents are tracked, but who would ever have supposed otherwise? Because neither Grodzinsky (nor, to the best of my knowledge, anyone else) claims to know exactly where in the brain the core combinatorial processes of syntax are carried out, the issue of whether syntax shares infrastructure with other faculties simply remains wide open.

Grodzinsky's target article might have benefited from dropping this section. The target article as a whole, however, is a salutary one. Recently (e.g., Lieberman 1998; Muller 1996), we have heard repeated claims that modern neurology, and in particular brain imaging, rules out the possibility of a Chomskyan universal grammar. Grodzinsky shows that such claims are, at best, premature.

Lesion location and aphasic syndrome do not tell us whether a patient will have an isolated deficit affecting the coindexation of traces

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Abstract: Data from published case and group studies bear on the trace deletion hypothesis. The deficit-lesion correlational literature does not support Grodzinsky's claim that lesions in and around Broca's area inevitably lead to comprehension deficits specifically related to coindexation of traces or his claim that other lesions spare this function.

Grodzinsky claims that Broca's area and surrounding cortex is the sole region of the brain involved in relating traces to their antecedents, and that the role of Broca's area in syntactic processing is restricted to this operation. Most of the data he presents in support of this claim come from the performance of aphasic patients. In this commentary, I will review these data, beginning with studies in our lab and then turning to the broader literature.

Caplan et al. (1996) studied 18 aphasic patients with CT-imaged single left hemisphere strokes. Lesion location was determined by morphometric analysis based on the parcellation system of Rademacher et al. (1992). Syntactic comprehension ability was assessed with an enactment task consisting of 12 examples of each of 25 sentence types, including sentences with no referential dependencies, sentences with pronouns, with reflexives, with PRO, and with traces. We compared the performance of 6 patients with purely posterior lesions to that of 12 patients with both anterior

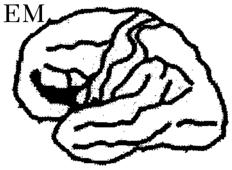
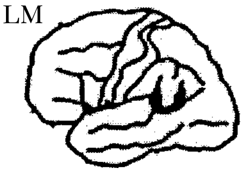
Sentence Types	Patient EM 	Patient LM 
Sentences with Full Noun Phrase		
Two-place active	12	12
Three-place active	11	12
Conjoined	9	12
Active Conjoined Theme	10	12
Three Referential expressions	8	12
Simple Referential expression	10	12
Sentences with Pronouns or Reflexives		
Reflexive, Simple NP subject	8	12
Pronouns, Simple NP subject	9	12
Simple Reflexive, Complex NP subj.	11	12
Simple Pronoun, Complex NP subj.	11	12
Simple Active Reflexive	12	12
Simple Active Pronoun	12	12
Sentences with Empty Noun Phrases		
Two-place passive	12	12
Truncated Passive	12	12
Two-place Cleft Object	9	12
Three-place passive	9	12
Three-place Cleft Object	2	12
Subject Object Relative	5	7
Object Subject Relative	7	10
Object Object Relative	7	9
Subject Subject Relative	9	12
Passive Conjoined Agent	10	12
Object Control	11	12
Subject Control	6	12
NP-Raising	5	6

Figure 1 (Caplan). Performance (number correct of 12 trials) of patients EM and LM on enactment task (for examples of sentence types, see Caplan et al. 1996)

and posterior lesions on each sentence type and on the difference in scores on 19 pairs of length-matched sentences designed to test specific syntactic operations. Among these 19 comparisons, there were 10 that are relevant to Grodzinsky's claim: 4 that compared passive and active sentences, 5 that compared object- and subject-relativized sentences, and 1 that compared raising and subject-control sentences. Performance was worse on the passive, object-relativized, and raising sentences. The groups did not differ on any of these 44 measures, indicating that patients with posterior lesions were as impaired as those with both posterior and anterior lesions on all syntactic operations, including coindexation of traces, as measured on this task.

Figure 1 presents CT lateral reconstructions and behavioral data from two cases with small lesions. Case EM had a lesion completely confined to Broca's area. He performed perfectly on passive and truncated passive sentences, and above chance on cleft-object and dative passive sentences. These four performances contradict Grodzinsky's hypothesis regarding the deficit seen after a lesion in Broca's area. EM performed at chance on object-relativized sentences and raising sentences, as predicted by Grodzinsky, but also performed at chance on object-subject relatives and on sentences requiring subject control of PRO, which should have been interpreted normally according to Grodzinsky's model. Case LM had an equivalent-sized lesion confined to the supramarginal and angular gyri. She partially fit the profile of having an impairment affecting coindexation of traces, with impaired performance on subject-object relatives and NP-raising sentences.

The literature is replete with studies showing similar off-line performances in patients with anterior and posterior lesions, or in patients with Broca's aphasia and other syndromes (Berndt & Caramazza 1999; Berndt et al. 1996; Caplan et al. 1985; 1997). Even Grodzinsky's results show this pattern (Balogh & Grodzinsky 1999). In the face of these numerous disconfirming data, Grodzinsky has tried to salvage his model by invoking two arguments: (1) performances of patients with Broca's aphasia are normally distributed around chance, implying that good performance in individual cases is a random occurrence (Grodzinsky et al. 1999; Zurif & Pinango 1999); and (2) the mechanism that produces disturbances affecting coindexation of traces is different in patients with anterior and posterior lesions, as shown by on-line studies (Swinney et al. 1996; Zurif et al. 1993). Neither argument is convincing.

The fact that there is a hypothesis that claims that certain patients make up a group in which members should perform at chance does not allow us to assume that a patient who performs perfectly or at above-chance levels on certain sentence types in one testing session would perform at chance levels over multiple test sessions, any more than the fact that IQ scores are normally distributed around 100 allows us to claim that an individual who has an IQ of 125 really has an IQ of 100, which would be seen if the subject were tested repeatedly. In fact, Grodzinsky's dismissal of good performances in Broca's aphasics is no more than circular reasoning: He hypothesizes that Broca's aphasics perform at chance, and then claims that Broca's aphasics who do not perform at chance can be considered part of a Gaussian distribution of performances because they belong to the group (Broca's aphasics) who perform at chance.

Whether or not a patient who performs at an above-chance level on a single administration of a task would really turn out to perform at chance if tested repeatedly is an empirical matter. In our studies, all the deficits we have described have been stable over sessions that have been separated by weeks (Caplan & Hildebrandt 1988a; 1988b; 1988c; Caplan et al. 1996; Hildebrandt et al. 1988; Waters et al. 1991). Grodzinsky's first attempt to save his theory is not only circular but also rests on an assumption that, where tested, has been shown to be false.

On the issue of performances of single cases, I would like to point out that the literature cited by Grodzinsky is not only lacking in cases who have been adequately studied with respect to their performance over several test sessions, but also is lacking in cases who have been studied on an adequate number of baseline control sentences. To establish that patients have deficits restricted to the coindexation of traces requires that they be shown to perform normally on sentences with reflexives and subject control of PRO (Caplan 1987a; Caplan & Hildebrandt 1986). To my knowledge, we are the only researchers who have reported case studies in which individual patients have been tested on these baseline structures, as well as on structures needed to test for coindexation of traces (Caplan & Hildebrandt 1988a; 1988b; 1988c; Caplan et al. 1996; Hildebrandt et al. 1987). Not one of the more than 60 patients we have tested has shown a deficit restricted to the coindexation of traces! Case KG (Hildebrandt et al. 1987) came the closest, but he also had difficulty with subject control of PRO in sentences that had both PRO and reflexives. Perhaps the lesson aphasia is teaching us is that brain damage does not selectively disrupt the categories postulated in Chomsky's model of syntactic structure (which, of course, is not to say that such categories do not exist).

Grodzinsky's second out – the claim that similar off-line performances of fluent and Broca's aphasics are caused by different mechanisms – is based on different on-line performances in these patient groups. Two studies report the absence of cross-modal lexical priming (CMLP) for the antecedents of traces in Broca's aphasics and the presence of this effect in Wernicke's aphasics. In the first study (Zurif et al. 1993), four Wernicke's patients who showed CMLP effects for traces also performed better on off-line tests of the ability to coindex traces than the four Broca's aphasics who did not show the CMLP effect. The differences in on-line performance might have been caused by the relative impairment of the patients rather than their lesion site (Caplan 1995). This leaves only one CMLP study (Swinney et al. 1996), of only four Wernicke's and four Broca's aphasics, to serve as the basis for arguing that similar off-line performances in these patient groups is the result of disturbances of different on-line operations.

Data from other on-line studies do not coincide with this result (Blumstein et al. 1998). We used self-paced, phrase-by-phrase listening to identify the locus of increased processing load in object- and subject-relative clauses in 20 aphasic patients and found no difference between Broca's and fluent aphasics (Caplan & Waters 1997). Both groups of patients showed longer listening times at the verbs of the subject-object compared to subject-subject relative clauses, as expected on the basis normal performance, but did not differ from each other in listening times at any point in the sentences, or on the accuracy of plausibility judgments made to each sentence.

Grodzinsky's hypothesis regarding the functional neuroanatomy of one aspect of syntactic processing in sentence comprehension does not receive support from the effects of lesions. The representation and processing of syntactic structures by the brain does not appear to be as simple as Grodzinsky makes it out to be.

Broca's aphasia, Broca's area, and syntax: A complex relationship

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Abstract: Three types of problems are raised in this commentary: On the linguistic side, we emphasize the importance of an appropriate definition of the different domains of linguistics. This is needed to define the domains (lexicon-syntax-semantics) to which transformational relations apply. We then question the concept of Broca's aphasia as a "functional" syndrome, associated with a specific lesion. Finally, we discuss evidence from functional brain imaging. The breadth and potential impact of such evidence has grown considerably in the last few years, expanding our knowledge of the multiple contributions of the "Broca's region" to phonological, lexical-semantic, and syntactic processing. "Lumping" under diagnostic labels, such as Broca's aphasia, should be replaced by more detailed linguistic and neurological descriptions of the clinical cases.

There seem to be some problems with Grodzinsky's approach:

1. On the linguistic side. The distinction between syntax, lexicon, and semantics is left to the intuition of the reader, and sometimes it turns out to be less than totally clear. Grodzinsky's central hypothesis cannot even be taken into serious consideration unless one defines the domains to which transformational relations (i.e., movement) properly apply. Many facts traditionally analyzed as "semantic" have been fully explained through syntactic properties via movement (polarity items, adverbial interpretation, quantification), and even lexical properties have been fully explained through syntactic movement. A revealing example of the consequences of adopting unclear boundaries between the lexicon, syntax, and semantics is Grodzinsky's misleading argument about passive forms involving noun phrases containing universal quantifiers and definite articles, presented as contrasting with wh-expressions. The interesting empirical evidence that they are given different interpretations by Broca's patients cannot be captured by simply assuming that wh-expressions and universal quantifiers are set apart from regular NPs because of a "semantic property." Indeed, ever since Barwise and Cooper's theory (1981) of generalized quantifiers it has been standardly assumed that definite articles can be considered members of the same natural class as universal quantifiers. If something splits universal quantifiers and wh-expressions, it is their syntactic property of requiring (covert) syntactic movement that must be interpreted.

2. Is Broca's aphasia really a well-defined functional syndrome? There is no convicing evidence that Broca's aphasia is a clear-cut "functional" syndrome in the sense proposed by Grodzinsky. Patients classified as Broca's aphasics on the basis of any of the commonly used batteries of clinical criteria show a highly variable pattern of impairment, including articulatory, phonological, lexical, and morphosyntactic disorders, in a highly variable combination specific to the individual patient. Not all patients behave as predicted by Grodzinsky's theory. Patients with different patterns of performance certainly exist (see the reports by Hicock & Avrutin 1995). In our opinion these are dismissed too lightly by Grodzinsky; even more likely, these patients may fail to be reported at all. The variability in production has been convincingly shown by Miceli et al. (1989). It has been suggested by proponents of the "strong" (i.e., functional) syndrome approach that this variability might be caused by differences in anatomical localization; thus, greater precision in anatomical criteria is needed for identifying "real" Broca's aphasics. Although most patients classified as Broca's aphasics will show structural lesions in the "big" Broca's area, there are several bona fide cases of Broca's aphasia associated with damage involving only portions of this region (white matter, insula, the basal ganglia), or even "exceptional" sites. The latter may reflect "nonstandard" cerebral organization for language, and are thus difficult to interpret; the patients with small lesions may present with "fragments" of the syndrome, and may thus provide insights for its multiple components.

3. Evidence from brain imaging. Grodzinsky points out that less than 10% of the work in this field has investigated the "computation of combinatorial operations in language processing" (sect. 2.5). However, the paradigms applied in PET or fMRI studies have included many language variables that prevented exploiting the syntactic components and their relationship with Broca's area. Indeed, the use of auditory sentence comprehension tasks or plausibility and true-value judgments in subtractive designs probably involved "large-scale language strategies," which would lead to functional correlates far beyond the syntactic effects.

Several papers have addressed the issue of phonology (Demonet et al. 1996; Paulesu et al. 1993) and demonstrate the consistent recruitment of Broca's area in phonological tasks. Functional heterogeneity has also been found within the left inferior frontal gyrus, where the opercular portion of Broca's area (Ba 44) is responsible for allowing access to words through a phonemic/articulatory route, whereas the more anterior component (the triangular portion Ba 45) seems to be related to semantic access from both words and pictures (Martin et al. 1996; Paulesu et al. 1997; Perani et al. 1995; Vandenberghe et al. 1996). The same area has also been shown to be active in normal subjects when verbs need to be generated (Martin et al. 1995). Perani et al. (in press) provide further evidence of an involvement of Ba 44 and 45 in verb processing. A relationship between this area and the semantics of motor actions has also been demonstrated. It is also involved in a network of brain structures that become active during the observation of meaningful pantomimes as compared with meaningless gestures (Decety et al. 1997). There is also PET evidence for the role of the left inferior frontal gyrus, namely, Ba 44 and 45 (Broca's area), in the "representation" of hand-motor actions. Bonda and coworkers (1996) found it activated during a self-ordered hand movement sequence. Decety and coworkers (1994), using PET, found activation of the inferior frontal gyrus during the mental simulation of actions.

Complementary evidence comes from Blasi and coworkers' PET data (in press) showing that lipreading (which requires integration across multiple domains: visual, motor, and linguistic), is associated with increased activation of ventral prefrontal/premotor cortex bilaterally. These findings corroborate a theory of an observation/execution matching system for speech perception.

Finally, the theory that the right hemisphere is silent, cited by Grodzinsky as evidence for an entire and exclusive left hemispheric representation of language processes, and as being a consequence of syntax, is no longer sustainable. A large amount of neuroimaging work has shown right brain involvement in both language comprehension and production. This was demonstrated in normal subjects but also in recovered aphasic patients. Right hemispheric activation appears to involve areas that are part of the normal, bihemispheric language network.

Broca's area and language evolution

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Abstract: Grodzinsky associates Broca's area with three kinds of deficit, relating to articulation, comprehension (involving trace deletion), and production (involving "tree pruning"). Could these be special cases of one deficit? Evidence from research on language evolution suggests that they may all involve syllable structure or those aspects of syntax that evolved through exploiting the neural mechanisms underlying syllable structure.

Grodzinsky argues convincingly that the deficits in grammatical competence arising from lesions in Broca's area are quite narrow and specific. But the evidence that he presents provokes immediate questions. Why should it be precisely these aspects of grammar that suffer? Why should the comprehension deficit, involving traces, be superficially at least so different from the production deficit, involving tensed verb forms? And why should these grammatical deficits be accompanied by a phonetic one, in the shape of laboured and effortful articulation – a deficit that is not characteristic of Wernicke's aphasia, for example? Because so little is known even now about the brain in relation to language, Grodzinsky may reasonably answer that it is a sufficient achievement to describe the symptoms of Broca's aphasia in precise grammatical terms. Even so, an explanation why just these symptoms co-occur would be welcome. I suggest that a possible explanation may emerge from an apparently unlikely source: the study of language evolution.

Why do human languages have the kind of syntax that they have? Why (to be more precise) do all languages seem to distinguish syntactically between nominal expressions, such as *that bicycle* and *John's arrival,* and sentential ones, such as *That is a bicycle* and *John has arrived?* Why, within sentences, is one nominal argument so often singled out for special grammatical treatment, as "subject" or "topic," and why does a special status attach so much more often to some positions (e.g., immediately before the verb, or after the first phrase) than to others (e.g., immediately after the verb, or before the last phrase)? The kind of grammar that humans use is only one of the many occupants of the space of possible grammars. Why, in short, has human language evolved in this particular direction rather than in other conceivable ones?

One recent suggestion (Carstairs-McCarthy 1999) takes seriously the parallels between sentence structure and syllable structure. A point was reached in hominid evolution when hominids could readily produce strings of individually meaningful calls or proto-"words." At that stage, a reproductive advantage would accrue to any subgroup in the population with a syntax, affording reliable interpretation of such strings. But where would such a syntax originate? Evolution does not tackle design problems from scratch but tinkers with what is available. What was available then was a neural mechanism for controlling vocalization that would already have begun to acquire peculiarly human characteristics (because of an increasingly L-shaped vocal tract, originally a byproduct of bipedalism) – that is, vocalization organized in syllables. Applying this mechanism to the syntax problem could have yielded a kind of language with some of the puzzling characteristics mentioned earlier: The syllable/margin distinction could have yielded the sentence/noun-phrase distinction (with verbs and auxiliaries reflecting syllabic nuclei), and the privileged status of the onset margin by contrast with the coda could be reflected syntactically in the special status of certain nonverbal constituents and positions. Syllable structure certainly does not account for everything in contemporary syntax (in particular, it supplies no basis for recursion), but it may be what set the evolution of syntax in motion.

I will not defend this scenario in detail here. Rather, I will explore what it leads us to expect about the neural machinery underlying grammar today. Principally, it leads us to expect that those aspects of grammar that are syllable-derived should still be controlled separately from those of independent and later origin; also, that the control of the syllable-derived aspects should overlap in the brain with the control of syllabically organized articulation. Consequently, it leads us to expect that among the types of aphasia found in contemporary humans should be one that combines an articulatory deficit with a deficit in those aspects of syntax that are syllable-derived, leaving other aspects spared. I suggest that Broca's aphasia conforms to this expectation closely enough to be worth investigating from this point of view.

The syntactic reflexes of the architecture of the syllable will include a "nuclear" position for the verb and "marginal" positions for nominal arguments of the verb. If the neural underpinning of this architecture is damaged, however, the sufferer will not recognize the presence of empty arguments ("traces") because the structural positions that these traces would occupy are inaccessible. Consequently, effects that Grodzinsky attributes to Trace Deletion will arise naturally. Similarly, the lack of a "nuclear" position entails *a fortiori* the lack of vacant nuclear position, and hence the absence of any movement to fill such a position. One of Grodzinsky's Tree Pruning effects (the failure of verbs to raise to second position in aphasic speech in German and Dutch) makes sense in this light, "second position" being one of the ways in which syntax mimics the status of the nucleus within the syllable. As for the fact that verbs suffer more than nouns do (along with characteristically verbal morphosyntax, such as tense marking), this may be attributable to the fact that "verb" and "auxiliary" owe their sharp syntactic differentiation from nouns to the special status of the nucleus as the core of the syllable, and to the sharp structural differentiation between nuclei and margins. Verbs and auxiliaries will thus suffer disproportionately when basic clausal structure is flattened through a lesion in the brain region that controls both it and the syllabic organization of speech.

This is a mere sketch of a possible new way of looking at Broca's aphasia. Much more research is needed to flesh it out. But, as a working hypothesis, it has two attractive features. First, it unifies the articulatory and syntactic deficits, both in comprehension and production, as effects of one underlying cause. Secondly, it is compatible with an account of language evolution that (I believe) unifies a range of superficially quite disconnected facts about language-as-it-is, thus helping to suggest reasons why language evolution should have taken the course it has rather than one of the many conceivable alternatives.

Grodzinsky's latest stand – or, just how specific are "lesion-specific" deficits?

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Abstract: Deficits observed in Broca's aphasia are much more general than Grodzinsky acknowledges. Broca's aphasics have a broad range of problems in lexical and morphological comprehension; furthermore, the classic "agrammatic" syntactic profile is observed over many populations. Finally, Broca's area is implicated in the performance of many linguistic and nonlinguistic tasks.

Grodzinsky has penned a highly imaginative account of aphasic deficits and their neural correlates, the latest in a series of proposals that he has put forward in the last 15 years for a grammar-specific faculty in the human brain (e.g., Grodzinsky 1984a). His proposals are famous for their strength, clarity, and falsifiability. Below we provide evidence that falsifies his latest stand.

First, Grodzinsky claims that the receptive deficit in Broca's aphasia is restricted primarily (perhaps exclusively) to grammar (e.g., "the patients seem to have no impairment in their lexicon in comprehension; the part of the lexicon that interfaces with sentence grammar is intact." sect. 2.1). This is misleading. It is well established that Broca's aphasics have marked deficits in both phonological and lexical processing, receptively and expressively (Goodglass 1993). In fact, some of the first demonstrations of impaired lexical priming in Broca's aphasia were conducted at the same institution where Grodzinsky conducts his English-language work (e.g., reduced, delayed or deviant word-word priming in Prather et al. 1991; see also Milberg et al. 1988).

Second, Grodzinsky asserts (sect. 2.1) that the grammatical comprehension deficit in Broca's aphasia is quite restricted, affecting syntactic movement operations while leaving other aspects of grammar intact (such as computation of agreement and case). This is incorrect. There is now a large cross-linguistic literature showing that Broca's aphasics (and other groups as well) are markedly impaired in the use of agreement and case information to assign agent-patient roles (Bates et al. 1987; Heeschen 1980; MacWhinney et al. 1991). Furthermore, although these patients often perform above chance on grammaticality judgment tasks, they are significantly less accurate in detecting subject-verb agreement errors than violations of movement (Devescovi et al. 1997; Wulfeck et al. 1991).

Third, the core of Grodzinsky's argument revolves around a specific type of syntactic deficit that is supposed to be unique to Broca's aphasia: a deficit in the movement operations associated with (inter alia) the processing of nonstandard word order. This is supposed to result in chance performance on passives and object clefts despite above-chance performance on actives and subject clefts. In fact, this very pattern has been observed in all forms of aphasia. For example, Dick et al. (1998) compared a large number of anomics, Wernicke's, conduction, and Broca's aphasics and found cases with Grodzinsky's signature "agrammatic profile" in all aphasic groups, including anomics (i.e., patients with word-finding deficits who do not display clinically significant signs of expressive agrammatism). The presence or absence of this agrammatic profile also failed to correlate with any particular lesion site, and appeared often in patients with lesions sparing Broca's area. We note that the same profile is observed in children who are still acquiring their language, and it can be reproduced in college students who have to perform exactly the same task under "stressful" conditions (e.g., a combination of low-pass filtering and compression of speech). In short, this profile has absolutely no localizing value.

Finally, Grodzinsky insists that the neural tissue in and around Broca's area is specialized for and dedicated to these syntactic operations, declaring that "the neurolinguistic localizing schema of language perception might not have permeated the clinical literature, yet it is currently accepted in cognitive neuroscience" (sect. 1.1). In fact, very much the opposite is true. Not only do functional imaging studies show language-related activation in widely distributed and overlapping networks (see Müller, this volume, for further comments), but a steadily increasing number of studies show that regions in and around Broca's area are activated during nonlinguistic tasks, such as object manipulation, mental imagery of tools, and sequential finger tapping cued by a drawn hand (Krams et al. 1998; Rizzolatti & Arbib 1998). Such "promiscuity" of activation does not lend much support to a language-specific role for Broca's area.

To summarize: The "core data" of agrammatism that Grodzinsky uses to define the putative role of Broca's area is observed in a wide range of populations, with different etiologies, including normal adults processing under stress. Patients with damage in and around this region display a range of deficits inside and outside of the grammar. Finally, imaging studies of normals show that Broca's area itself is involved in many different linguistic and nonlinguistic tasks. In short, the pattern of selective deficits and activations that are essential to Grodzinsky's proposal are not so selective after all.

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Nonlinguistic transformation processing in agrammatic aphasia

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Abstract: Grodzinsky's characterization of the syntactic function of Broca's area is convincing, but his argument that this transformation processing capability is specific to language is less so. Based on predictions from simulation studies of sequence learning, we report a correlation between agrammatic patients' impairments in (a) syntactic comprehension, and (b) nonlinguistic sequence transformation processing, indicating the existence of a nonlinguistic correlate of agrammatic aphasia.

Grodzinsky rightly emphasizes that it has only been through the application of "a new, highly abstract and precise approach" (sect. 0, para. 5) that Broca's area, characterized initially as processing motor aspects of speech, has come to be characterized in terms of its role in the computation of transformational syntactic relations. The methodological point is that to expose precise functional processing roles, one must systematically apply experimental procedures that involve specific dissociable processes. Grodzinsky has done this in his functional characterization of Broca's area in the computation of transformational relations between moved constituents and their extraction sites.

Having established this syntactic specificity of Broca's area, Grodzinsky goes on in section 5 to argue that this transformation processing is specific to language, citing data that failed to demonstrate correlations between general linguistic capacities and general intellectual capacities. Based on his own "precise approach," however, it seems that there may be a serious methodological shortcoming in this argument. In the studies cited, the tests of language capacities did not specifically measure the target behavior of syntactic movement processing; the tests of general intellectual capacities were likewise quite nonspecific. Given the early failure of nonspecific tests to yield a proper characterization of syntactic processing in Broca's aphasia, well described in section 0, it is clear that nonspecific testing of (a) linguistic capacities and (b) general nonlinguistic intellectual capacities in these patients is also doomed to fail to reveal specific nonlinguistic deficits and their correlations with syntactic transformation processing. To determine whether this transformation processing is indeed specific to language, one should test Broca's aphasics in (a) linguistic tasks that specifically address movement transformation processing, and (b) nonlinguistic tasks that require the processing of transformations of serial order in nonlinguistic sequences.

We have developed such nonlinguistic protocols to study the dissociable processing of surface and abstract structure of nonlinguistic sequences (Dominey 1997; Dominey et al. 1998). In these protocols, the two sequences ABCBAC and DEFEDF have different serial orders or surface structures, but share the same abstract structure 123213, and are considered to be isomorphic sequences. We note two important distinctions concerning abstract structure: First, whereas knowledge of surface structure is sequence-specific, knowledge of abstract structure, once learned, can transfer to an open class of isomorphic sequences. Second, from the perspective of movement-related transformations, the abstract structure 123213 has the interesting property that the second triplet (213) is a transformation of the first (123). It can thus be considered a noncanonical abstract structure, whereas 123123 is canonical.

In simulation studies we have demonstrated that whereas surface structure can be learned by a recurrent neural network, abstract structure learning requires additional capabilities to represent transformational relations between repeating elements (Dominey 1997; Dominey et al. 1998). We now consider the possibility that such a dual process model could provide part of the basis for thematic role assignment in canonical and noncanonical sentences, with the appropriate abstract transformations for noncanonical sentences being signaled by the presence of patterns of function items in the surface structure. This suggests that impaired syntactic comprehension in agrammatic aphasia is related to an impairment in serial order transformations on noncanonical forms, and that this impairment is not restricted to natural language (Lelekov et al., submitted).

We recently tested this by studying the ability of seven agrammatic aphasic subjects to learn and process the noncanonical abstract structure of nonlinguistic sequences. Subjects were required to learn the noncanonical abstract structure 123213 (by studying a set of 10 isomorphic sequences derived from this abstract structure) and then to classify 20 new letter strings by whether they corresponded to the learned target abstract structure. Performance in this task was compared to syntactic comprehension, as evaluated by the nine-sentence type "who did what to whom" task developed by Caplan et al. (1985). Agrammatic patients demonstrated performance impairments for syntactic comprehension and letter-sequence classification tasks that are significantly correlated ($r^2 = 0.86$, $p = 0.003$).

To verify that the failure on the nonlinguistic task was related to an impairment in processing noncanonical forms, we also tested these patients with the same abstract structure classification task, using the canonical abstract structure 123123. We then compared canonical and noncanonical performance (in terms of percentage of correct responses) across the linguistic and nonlinguistic tasks to test whether the processing of transformations in noncanonical order would be specifically impaired both for linguistic syntax and for nonlinguistic abstract structures. For both tasks, noncanonical processing is selectively impaired with respect to canonical processing, as revealed by a Task (linguistic vs. nonlinguistic) × Order (canonical vs. noncanonical) ANOVA. There was a significant effect for Order [$F(1,5) = 31.7$, $p = 0.0025$], indicating that the processing of canonical order was significantly superior (83%) to the processing of noncanonical order (37%). Most important, the Order × Task interaction was not significant [$F(1,5) = 0.053$, $p = 0.8$], indicating that this impairment for noncanonical order processing holds for both linguistic (canonical 74% vs. noncanonical 30%) and nonlinguistic (canonical 92% vs. noncanonical 45%) Tasks (Lelekov et al., submitted).

Syntactic comprehension deficits accordingly seem to result, at least in part, from an impairment in serial order transformations on noncanonical forms not restricted to natural language. More generally, through the use of "a new, highly abstract and precise approach" we now have evidence that there is a nonlinguistic correlate of the transformation processing impairment described by Grodzinsky, and that both within and outside of natural language, this transformation processing remains highly specific and dissociable from other sequence processing capabilities, as suggested by previous results from simulation (Dominey 1997; Dominey et al. 1998), experimental psychology (Dominey et al. 1998) and neuropsychology (Dominey & Georgieff 1997; Dominey et al. 1997).

The gratuitous relationship between Broca's aphasia and Broca's area

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Abstract: Many authors assume that Broca's area subserves the functions that are lost in patients with Broca's aphasia. This commentary attempts to clarify the relationship between Broca's area and Broca's aphasia and suggests that statements about the neurology of patients' specific language functions might be better supported by their individual structural neuroimaging data.

Grodzinsky argues that patients with Broca's aphasia have deficits in intrasentential dependency relations. He assumes that Broca's area subserves this relational function because of previous assumptions that all patients with Broca's aphasia have lesions in

Broca's area. This assumption reflects a long-standing and common misinterpretation of the role of Broca's area in Broca's aphasia and deserves to be clarified.

It is true that many neurology textbooks state that lesions to Broca's area result in Broca's aphasia, but the fact is that lesions restricted to Broca's area alone never lead to a persistent Broca's aphasia. Even surgical removal of Broca's area leads only to a transient mutism, after which the patient returns to normal. Grodzinsky correctly cites Mohr (1976), who established that persistent Broca's aphasia results only from lesions that extend beyond Broca's area and generally include surrounding frontal cortex, the insula, and underlying white matter. In our work, we have found that chronic Broca's aphasics do indeed tend to have larger lesions. However, we find that only 50 to 60% of our patients with lesions including Broca's area have persistent Broca's aphasia (Dronkers & Jovanovich, forthcoming; Dronkers et al. 1992).

In addition, we have seen several patients with chronic Broca's aphasia (as determined by the Western Aphasia Battery and the Boston Diagnostic Aphasiz Exam) whose lesions completely spare Broca's area. In fact, 15% of our right-handed chronic stroke patients with single, left hemisphere lesions and Broca's aphasia do not have lesions in Broca's area at all. If the relationship between Broca's area and Broca's aphasia is so poor, why has the idea of a perfect relationship been promoted for so long?

The persistence of this idea stems from a long history of misinterpretations. First, most people assume that Broca regarded the posterior inferior frontal gyrus as a language area. This is not the case. Broca never stated that this area supported language functions. He concluded that it had something to do with articulation, as his two patients had difficulty in speech production. He was quite convinced that they understood everything that was said to them and that their language was therefore intact. He refused to call the disorder an "aphasia," rather calling it an "aphemia," from the term "phemi," "I speak," "I pronounce" (Broca 1861b). It was Trousseau who later coined the term "aphasia," triggering a letter from Broca who felt the term was not appropriate for this articulation deficit (Broca 1864).

Second, we assume that Broca had numerous patients to support his claim. In fact, his case was based largely on two initial patients whose brains he preserved (Broca 1861a; 1861b). Other cases were presented to him as instances of a speech disorder with involvement of the posterior inferior frontal gyrus. These cases are not all documented and the extent of the lesions is often not known. The damage most likely involved neighboring frontal regions, including the underlying insula and white matter. In fact, Broca's first case is now known to have had a much larger lesion than was apparent on the surface of the brain and also involved these underlying areas, not just Broca's area (Signoret et al. 1984).

A third assumption we make is that no one ever refuted Broca's claim by presenting contradictory cases. Actually, during the century numerous cases were presented with lesions to Broca's area and no Broca's aphasia, or, Broca's aphasia with no lesion to Broca's area (Bramwell 1898; Marie 1906; Mohr 1976; Moutier 1908). These cases, like the ones discussed above, were not uncommon. Because they do not fit the traditional model and no new model has been introduced, they tend to be dismissed as exceptions. Nevertheless, they represent a significant number of cases.

A fourth assumption is that new functional neuroimaging data support the traditional model of language in Broca's area. Actually, Broca's area has been implicated in everything from speech production and episodic memory encoding to gesture recognition and mirror drawing. In truth, Broca's area is probably activated any time a task requires subvocalizing, and this does not support its role exclusively in any particular cognitive function aside from articulation. In addition, neuroimaging studies of language activate many other brain regions besides Broca's area but these are frequently ignored. Studies that focus only on regions of interest such as Broca's area minimize the importance of other integrated brain regions that also contribute to the intricacies of language.

Finally, we often forget that Broca's aphasia is not a single entity with a solitary deficit. It is a syndrome with many individual deficits. Broca's aphasics have difficulty not only with complex grammar, but with naming, articulatory planning and the execution of articulatory movements, repetition, reading, and writing. It would be foolish to assume that all these functions could be located in one brain region. Indeed, at least one of them (articulatory planning) has been shown to involve a discrete area of the precentral gyrus of the insula (Dronkers 1996), not Broca's area at all. Thus we see that localizing Broca's aphasia to one area is too simplistic; rather, several individual brain areas may subserve the different functions affected in patients with Broca's aphasia.

In short, the relationship between Broca's area and Broca's aphasia is not as straightforward as we once thought. Instead, with the contributions of linguistics and psycholinguistics our concept of Broca's aphasia has grown and become more intricate. Our knowledge about the role of Broca's area has been enhanced by technologies that allow us to view the brain areas affected by the injury *in vivo* and then make educated statements about the relationships between brain regions and functional deficits. Behavioral studies that provide structural neuroimages or reconstructions of patients' individual lesions (rather than broad general descriptions) are particularly helpful in drawing conclusions about the neurology of certain behavioral functions. Whether intrasentential computations reside in an area of frontal cortex remains to be seen, but we would be hindering our progress in understanding brain-behavior relationships if we assumed that Broca's area subserved this function merely because it might be affected in patients with Broca's aphasia.

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Intact grammars but intermittent access

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Abstract: Grodzinsky examines Broca's aphasia in terms of some specific grammatical deficits. However, his grammatical models offer no way to characterize the distinctions he observes. Rather than grammatical deficits, his patients seem to have intact grammars but defective modules of parsing and production.

It is a fact about natural languages that words and phrases may be pronounced in displaced positions. In *What did you buy?, what* functions as the direct object of *buy,* but does not occur in the usual direct object position, to the right of the verb. Some generative models have used a movement operation to characterize this phenomenon. Here, Grodzinsky claims to have shown that language is not located in Broca's area. Rather, this area is more specialized than previously thought and deals with two syntactic functions: the movement operation alluded to above insofar as it affects the understanding of language (sect. 2.1) and "the construction of higher parts of the syntactic tree in speech production" (Abstract). There are problems at two levels.

First, Grodzinsky claims (sect. 2.4.1) that the key comprehension deficit of Broca's aphasics relates to the movement of phrasal constituents, NPs and wh-phrases, but not of heads. There are difficulties in recovering the movement of phrases, but not the movement of heads: He writes that "agrammatic aphasics are capable of representing traces of [head] movement" (sect. 2.4.1). His distinction is based on a grammaticality judgment test but, because strategies independent of traces may be involved (for example of the type he sketches in sect. 2.6), there could be other explanations. However, his claim taken at face value raises a problem: There is no straightforward way of isolating movement of phrases from the movement of heads in the 1980s-style Government-Binding (GB) model he uses. Such models postulate an operation Move Alpha, by which both phrases and heads move, leave traces, and are subject to the same locality restrictions.

Grodzinsky's data do not fit neatly with his GB model, which does not cut the empirical pie in the way he needs. The problem is compounded under more recent Minimalist approaches. Not only is there no ready distinction to be found here between the movement of heads and phrases – there is no movement operation at all. Minimalist analyses dispense with a distinct level of D-structure and therefore with D-structure-to-S-structure mappings. They also dispense with familiar top-down phrase-structure rules. Instead, phrase structure is built bottom-up through the successive merger of elements from an "array" of lexical items. For a sentence like *I will visit London, visit* is merged with the NP *London* to yield a VP; then the inflectional element *will* is merged with that VP to yield an IP, and so on. One type of merger involves copying an element. For the expression *What did you buy?, what* is copied and merged with *[did you buy what],* yielding *[what [did you buy what]],* with subsequent deletion of the lower *what* (we omit structural labels). This is how "movement" phenomena are handled but Minimalist approaches involve no movement as such. There is no unitary operation corresponding to GB movement. Elements are merged successively, sometimes copied and sometimes deleted; it is hard to imagine what particular function would be compromised in the people Grodzinsky describes. He notes that functions that build phrase structure are not compromised in his subjects, but in Minimalist syntax there is little beyond structure-building functions.

Now to the second problem. If GB and Minimalist syntacticians do not have the means to capture naturally the descriptive generalizations that Grodzinsky reports, how should they revise their claims about grammars? They will surely want to know on what basis the descriptive generalizations are made. In his discussion of grammaticality judgments involving movement of phrasal constituents (sect. 2.4.1), he tells us that he "tested four nonfluent, agrammatic Broca's aphasics . . . with lesions in and around Broca's area, including white matter deep to it, ranging from the operculum, to the anterior limb of the internal capsule, to the periventricular white matter." So, even for just these four patients, the anatomical damage covers more than Broca's area and includes subcortical tissue. His patients show comprehension deficits relating to the movement of phrases and production deficits relating to the higher elements of tree structures. He does not argue that there are production deficits relating to movement, or comprehension deficits relating to the topmost parts of phrase structure. So movement needs to be there to account for production capabilities and the full tree needs to be available to characterize comprehension functions. If a grammar is a representation of an individual's linguistic knowledge, which can be used for various purposes, then Grodzinsky's results actually suggest that there is no damage to the grammar, but rather two defects in the way it is used for the purposes of parsing and production, presumably two distinct modules.

Grodzinsky's claims are based on proportions and tendencies, often just "chance" versus "above chance." For constructions involving movement of phrasal constituents, "error rates were about 40%." (that means 60% correct), as opposed to 10% in comparable constructions not involving that kind of movement (90% correct; sect. 2.4.1). Similarly, the data on agreement and tense errors (sect. 2.7.3) show a difference in frequency (3.9% versus 42.4%); the difference is not absolute, and if tense is available 57.6% of the time, despite the "pruning," it is available more often than not. There is a good deal of variation being glossed over, both linguistic and anatomical, which shows that he has not yet met his goal of "a new, highly abstract and precise approach" (sect. 0). In addition, there is much normal behavior: If subjects achieve 60% correct on relevant tasks, there is no basis for saying that they lack the relevant parts of the grammar (see Crain & Thornton 1998 for an enlightening discussion of such statistics). Grodzinsky presents two important and interesting features of agrammatism, but there are other salient features that his proposals do not address. If "most human linguistic abilities, including most syntax, are not localized in the anterior language areas" (sect. 4), why do patients who have sustained lesions in these areas have problems with verb retrieval and why is their speech typically "effortful, nonfluent, and telegraphic" (sect. 2.7.1)? Broca's aphasics generally have slow reaction times (Shapiro & Levine 1990) and it is clear from agrammatism samples in any introductory aphasia text that there is far more going on than what Grodzinsky describes. Therefore, there must be more ways to cut the empirical pie.

Grodzinsky has certainly made a healthy innovation in examining aphasias in terms of grammatical deficits, integrating work on pathologies with grammatical theory. However, what the variability suggests – both within and across individuals, both anatomically and in linguistic behavior – is not a precise grammatical deficit. His grammatical models offer no obvious way to characterize his empirical distinctions. Furthermore, if individuals often behave in accordance with a normal, intact grammar, then they must have an intact grammar, but with somewhat intermittent access to it. Perhaps the modules that use the grammar (parsing and production) are defective in some fashion. In saying this, we recognize that we assume a burden of argument that goes beyond the scope of a *BBS* commentary.

Syntax in the brain: Linguistic versus neuroanatomical specificity

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Abstract: We criticize the lack of neuroanatomical precision in the Grodzinsky target article. We propose a more precise neuroanatomical characterization of syntactic processing and suggest that syntactic procedures are supported by the left frontal operculum in addition to the anterior part of the superior temporal gyrus, which appears to be associated with syntactic knowledge representation.

The title of Grodzinsky's target article, "The neurology of syntax: Language use without Broca's area," suggests that it provides detailed information about the functional neuroanatomy of syntactic abilities. The description he presents, however, is detailed only with respect to the psycholinguistic aspects of syntactic processing he proposes; it clearly lacks a similar precision with respect to neuroanatomy.

This is rooted in the fact that Grodzinsky's main empirical evidence for the claim that Broca's area is the "neural home to mechanisms involved in the computation of transformational relations between moved phrasal constituents and their extraction sites" (sect. 0) stems from lesion-based studies. Nature (in almost all cases) fails to offer lesions circumscribed enough to allow a precise description of the language-brain relationship. Thus the claim Grodzinsky formulates with respect to the Broca's area – as he acknowledges – can only hold for Broca's area as a large area (including surrounding left anterior neural tissue). He defines this area according to Mohr (1976) to "'encompass most of the operculum, insula, and subjacent white matter'" (sect. 0). These subregions in the left frontolateral region, however, subserve a number of different linguistic and nonlinguistic functions.

The functional neuroimaging evidence Grodzinsky cites is restricted to those studies investigating syntactic aspects (Bavelier et al. 1997; Just et al. 1996; Mazoyer et al. 1993; Stromswold et al.

1996); other studies are neglected. The language-related imaging studies Grodzinsky cites do not – as he claims – speak in favor of the Trace Deletion Hypothesis-based view of the limited role of Broca's area in language processing. The story is incomplete as he neglects language processing studies of phonological and semantic aspects; moreover, he fails to present fully the relevant data by Mazoyer et al. (1993). He reports that their data showed an involvement of the left inferior frontal regions on exposure to (a) words and (b) stories in the subject's native language. What he does not report is that no such activation was found for (c) syntactically correct sentences containing pseudowords. The full data set is not compatible with Grodzinsky's view as this view would predict conditions (b) and (c), but not (a), to show activation in the left inferior frontal gyrus.

Recent functional neuroimaging studies (fMRI) have shown that the left inferior frontal gyrus (including Broca's area) is active during semantic processing when strategic aspects are in focus (Kapur et al. 1994; Thompson-Schill et al. 1997), and that this area (in particular the region between BA 44 and BA 6) is also active during phonological processing when phoneme segmentation and sequencing, in contrast to phoneme perception, is required (Burton et al. 1999; Démonet et al. 1992; 1994; Fiez et al. 1995; Paulesu et al. 1993; Price et al. 1994; Shaywitz et al. 1995; Zatorre et al. 1992). Moreover, this area is active during working memory (Fiez et al. 1996a; Grasby et al. 1994; Paulesu et al. 1993), and is obviously involved (in addition to the basal ganglia and the cerebellum) when temporally defined sequential structures are to be processed (Penhune et al. 1998; Schubotz et al. 1999).

We are in sympathy with Grodzinsky's conclusion that Broca's area and its vicinity does not house most linguistic abilities, but only processes underlying highly structured syntactic abilities, with major parts of the human syntactic capacities residing elsewhere. We feel that this claim can be formulated more precisely, however, given the data at hand. Here we do not discuss whether the assumed highly structured syntactic abilities whose failure we observe in Broca's aphasia are correctly described by the Trace Deletion Hypothesis (as we have done elsewhere, e.g., in Friederici & Gorrell 1998; Friederici & Graetz 1987; Frisch & Friederici 2000); rather, we will discuss new and relevant data from recent fMRI studies on sentence processing, on the basis of which functional neuroanatomy of syntax can be formulated more accurately.

FMRI studies focusing on syntactic processing have shown a selective increase in the activation of the left frontal and left temporal operculum when processing syntactically well-structured but semantically empty speech (sentences containing pseudowords) was compared to word list processing (Friederici, in press; Meyer et al. 1999). In addition, the processing of sentences with moved constituents, in comparison to those with unmoved constituents, increases activation in both Broca's and Wernicke's areas (Cooke et al. 1999; Just et al. 1996). Taken together, these data suggest that parts of the left inferior frontal gyrus, as well as anterior portion of the superior temporal gyrus, support syntactic processes.

Activation in the anterior temporal lobe was found in a number of PET studies as a function of sentence compared to word-list reading in a number of studies (Bottini et al. 1994; Mazoyer et al. 1993). It is not clear that this activation is a pure reflection of syntactic processes, however, as it was found that activation in this area does not vary as a function of syntactic complexity but as a function of the presence of syntactic structure per se. Thus, the area seems to play a very basic role in syntactic processing rather than supporting the processing of complex structures in particular.

Our data (Friederici, in press; Meyer et al. 1999) in particular seem to indicate that the deep *left frontal operculum,* a cortical area in the vicinity of the anterior insula, is primarily involved in syntactic processing. Moreover, the finding that this area is active not only during syntactic processing in comprehension, but also in production (Indefrey et al. 1999a) challenges Grodzinsky's view that syntactic processes in production and comprehension are separated anatomically in the left frontolateral cortex. The evidence suggests that the distinction between production and comprehension as observed in Broca's aphasia may be better characterized as a function of the involvement of the temporal language cortex and its interaction with the inferior frontal region rather than a function of separate subregions in the left inferior frontal gyrus.

The distinction between declarative and procedural syntactic knowledge (Friederici 1990) may be relevant when it comes to functionally specifying the frontal and temporal language cortices in the left hemisphere with respect to syntactic processes during production and comprehension. The grammatical knowledge (lexicon and syntax), which is independent of any timing parameters, may be associated with the superior temporal gyrus, with syntax involving the anterior temporal operculum (in front of Heschl's gyrus) in particular, whereas the procedural knowledge, which depends on temporal parameters and sequencing constraints during processing, is associated with the left frontal operculum, located close to Broca's area.

Agrammatic comprehension of OVS and OSV structures in Hebrew

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Abstract: This commentary brings further support for the Trace Deletion Hypothesis (TDH) from a new study of OVS (Object-Verb-Subject) and OSV (Object-Subject-Verb) sentences in Hebrew, which are active constructions that involve object movement but no change in morphology. The comprehension of these constructions in Broca's aphasia is impaired, and the performance is at chance level, as predicted by the TDH.

One of the impressive aspects of the Trace Deletion Hypothesis (TDH) is that it correctly predicts performance on a wide range of syntactic structures in a variety of languages. These structures were carefully selected to allow empirical testing of contradicting predictions that different theories of agrammatic comprehension make. Thus, they make comparison between competing accounts possible, and provide insight into the nature of the comprehension deficit in Broca's agrammatic aphasia.

In what follows I will describe a critical case from Hebrew that allows us to examine the TDH and compare it with other accounts. Two structures that are created by left dislocation, the active OVS and OSV, provide such a critical test case, as they distinguish syntactic movement from thematic-role ordering and morphological complexity.

The basic word order in Hebrew, like in English, is SVO. OVS and OSV are also possible, but secondary, and are formed by focalization or topicalization (Shlonsky 1997). They are created by left dislocation – moving the object (together with its accusative marker) to the beginning of the sentence (in OVS the verb also moves after the object; see Examples 1 and 2):

(1) OVS
et ha-rofe ha-ze mecayer ha-xayal.
ACC the-doctor this paints the-soldier.

(2) OSV
et ha-rofe ha-ze ha-xayal mecayer.
ACC the-doctor this the-soldier draws.

These structures form a minimal pair with simple active sentences: They are active, just like their SVO counterparts, and contain exactly the same elements without morphological change. However, unlike the simple active, their derivation requires a movement of the object, and the thematic role order is reversed.

These properties make OVS and OSV a good test case for comparing the predictions of the TDH with those of other theories of agrammatic comprehension.

Three theories make three different predictions regarding this construction: A theory that claims that the canonicity of thematic-role order determines agrammatic comprehension, and that agrammatics assign thematic roles by their linear position (Caplan 1983), predicts *below-chance* performance on OVS and OSV structures, because the order of the arguments is reversed, hence the object is bound to receive an agent role, and the subject, a theme role (but see Piñango, in press, for a different type of canonicity approach that predicts chance performance in these sentences).

A theory that blames the additional morphology (in passive and perhaps also in relative clauses) for the deficit in comprehension would predict *above-chance* performance on the OVS/OSV sentences that are not morphologically different from the simple active. Finally, the TDH predicts that because the object moves and cannot receive its thematic role through a chain, it receives an agent role by the strategy, and the subject retains its agent role. An agrammatic patient who is left with two agents and is forced to guess who the real agent is would perform at *chance* level.

A study I am currently conducting examines the comprehension of this construction in Hebrew-speaking agrammatic patients. The results to presented below are preliminary, taken from the one subject who has already completed the 250 test sentences. This subject suffered a massive left frontal hemorrhage five years prior to testing, and was diagnosed as a Broca's aphasic according to the Hebrew version of the Western Aphasia Battery (WAB); he had characteristic nonfluent agrammatic speech, with short, simple, and ungrammatical utterances, and tense inflection errors. He (and the other participating patients) were selected by clinical evaluation using the WAB, by imaging information, and their speech output; no preliminary selection according to comprehension pattern was made.

Comprehension was assessed using a picture-selection task. The patient heard a (semantically reversible) sentence and was asked to select the picture that correctly described the sentence, from two pictures presented. The foil was a picture in which the roles were reversed. The experiment included OVS and OSV sentences, as well as active SVO, object relatives, and subject relatives, randomly ordered. A healthy control subject matched to the patient in age, gender, and education scored 100% correct on all 5 conditions.

The results presented in Table 1 show that although OVS and OSV are active sentences, performance on both of them was not significantly different from chance (using the binomial test, $p > 0.05$). The active SVO, on the other hand, was significantly above chance ($p < 0.001$). On subject and object relatives, the patient performed like other reported patients: Subject relatives were significantly above chance ($p < 0.001$), and object relatives were at chance ($p > 0.05$).

Thus, the prediction of the TDH is corroborated by the findings: The performance on these two types of active sentences was *at chance.* This result cannot be accounted for by a theory that assumes thematic-role assignment by linear order, nor can it be explained by morphological complexity. The performance difference between SVO and OSV actives in Hebrew is similar to the findings of Hagiwara and Caplan (1990) for Japanese, which is an SOV language, thus providing additional support for their case from an SVO language.

Table 1 (Friedmann). *Agrammatic comprehension in different constructions*

Structure	% Correct (correct/total)
Active-OSV	46% (23/50)
Active-OVS	52% (26/50)
Active SVO	90% (45/50)
Object relative	42% (21/50)
Suject relative	86% (43/50)

Another intriguing result of this study is that verb movement did not interact with the comprehension deficit: Although OVS and OSV differ in verb movement, the performance on them was the same (no significant difference using χ^2, $p > 0.05$). This further supports Grodzinsky's claim that agrammatics are able to represent traces of verb movement.

Finally, the chance performance in these structures raises an interesting question regarding the integration of trace deletion in recent syntactic theories. As Grodzinsky notes, within the framework of the VP-internal subject hypothesis, together with later suggestions about NP-movement (Chomsky 1995a; Pollock 1989), the subject moves out of the VP (where it receives its theta-role) in many cases. In the case of actives and subject relatives, although the subject moves, it receives the appropriate theta-role from the strategy. However, in OVS structures a problem arises: How does the subject that moved out of the VP receive its Agent role, given that its trace has also been deleted? Without the agent role assigned to the subject, and given the R-strategy that assigns roles by linear position in the absence of a thematic-role, below-chance rather than chance performance is expected.

Cutting a long story (too) short

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Abstract: Both linguistic and empirical evidence fail to support Grodzinsky's account of Broca's aphasics' comprehension problems. We address concerns regarding Grodzinsky's referring to the *internal subject hypothesis,* the importance of case information in thematic role assignment, the processing of passives, and the adequacy of Grodzinsky's linear strategy.

The internal subject hypothesis. Grodzinsky has kept pace with linguistic theorizing, incorporating its recent developments into his own proposals. This is true for the *internal subject hypothesis* (ISH; cf. McCloskey 1997 for a review). ISH assumes that *all* argument NPs are base-generated VP-internally where their thematic roles are assigned, and then move up the tree.[1] This implies that *all* thematic roles (*including* the one for the subject) are assigned VP-internally and are then transmitted via traces to the NP arguments in their surface positions (cf. Grodzinsky 1995a, p. 35 and sect. 2.2, para. 8 of the target article). But this assumption poses a serious problem for Grodzinsky's argument because active and passive sentences are identical in this regard. Given that thematic role transmission via traces is what is impaired in Broca's aphasia, Broca's should not be able to assign any subject TH-role grammatically. The assumption that one thematic role is assigned grammatically is crucial for Grodzinsky's proposal of *thematic competition* (sect. 2.2, para. 11). This mechanism is supposed to explain chance performance and is characterized by "two Agents in the representation" (sect. 2.2, para. 11). One of these agents is assigned grammatically, whereas the second is assigned via a linear strategy to a moved NP that is thematically stranded (because of trace deletion). But if the subject always receives its TH-role indirectly (as ISH implies), it is unclear how thematic competition should evolve, particularly in object-relatives. In this case, Broca's aphasics should assign all thematic roles exclusively via a strategy. Given that the strategy alone does not lead to thematic competition, Broca's aphasics should then perform above chance in subject-before-object constructions (which they in fact do), but they should perform *below* chance in object-before-subject constructions (which they do not). Thus Grodzinsky's approach is not compatible with ISH, as it would not then predict chance performance in object-relatives.

Processing case information. One of Grodzinsky's claims is that the processing of syntactic information such as case (sect. 2.1, para. 5) is preserved in Broca's aphasics. In languages with overt case marking and free word order, case information is a crucial cue for thematic role assignment. What is surprising is that Grodzinsky does not comment on how Broca's aphasics make use of this information when assigning thematic roles. There is some evidence that they in fact do. Heeschen (1980), for example, found that Broca's performed *above chance* in German declarative (noncanonical) object-before-subject constructions with unambiguously case-marked NPs. That case information improves Broca's comprehension in some languages like German, seems not to be universal. Hagiwara and Caplan (1990) have shown that Japanese Broca's aphasics show no benefit from the highly case-inflected nature of their language. It is unclear how Grodzinsky would explain these results. Does case information help overcome trace deletion? Can it override the linear strategy? How can it be a language-specific effect?

Processing passives. Grodzinsky presents data from Broca's aphasics' performance on passive sentences in several languages to support his ideas. He neglects published evidence incompatible with his view, however. Grodzinsky explains the chance performance in English Broca's patients with a competition between two assignments of Agent, whereby one Agent role is assigned grammatically to the by-phrase and the second one is assigned via a linear strategy to the subject NP (as the first NP). Grodzinsky overlooks data by Friederici and Graetz (1987), however, indicating that thematic competition does not predict Broca's performance on Dutch passives. Contrary to Grodzinsky's prediction, Broca's patients performed *above chance* for all passive constructions, independent of the position of the by-phrase. The authors suggested that in languages with a less strict word order than English (e.g., Dutch or German), Broca's might rely more on the grammatically assigned Agent as indicated by the by-phrase than on a linear strategy (NP1 = Agent).

The case of *truncated passives* (passive constructions without a by-phrase) introduces a paradox in Grodzinsky's account. Broca's patients perform at chance on these constructions (Martin et al. 1989) but Grodzinsky's approach predicts performance above chance. The status of the verb's Agent role in such constructions is unclear. It may be suppressed, in which case thematic competition does not apply because there is only the Patient theta role to assign. Alternatively, the implicit Agent role may be assigned to the passive morphology (Baker et al. 1989). In this case there would be two theta roles that may compete. However, the implicit Agent is clearly nonreferential. Grodzinsky (sect. 2.6, para. 7) excludes nonreferential NPs from the linear strategy to account for other nonconforming examples (Hickock & Avrutin 1995; Saddy 1995). Whatever the status of the Agent in truncated passives, Grodzinsky cannot explain the data.

Linear strategy. Grodzinsky's linear strategy may hold for some structures in English and in some other languages, but it is certainly not universal, as shown in the previous sections. Friederici and Gorrell (1998) point out that its application also fails to account for the finding that Broca's aphasics showed a verb-object-subject reading in English sentences like: "Is hitting the cow the pig?" (Bates et al. 1987). Here, a linear strategy would predict a verb-subject-object reading because it would assume that the first NP is assigned Agent and not the second one. Friederici and Gorrell (1998) proposed a strategy that is based on *structural prominence* of thematic assignees. Structural prominence accounts for the above data and provides a grammatically based alternative to Grodzinsky's linear strategy.

At first sight, Grodzinsky's proposal appears to be an elegant approach to explain Broca's aphasics' performance cross-linguistically. Unfortunately, the literature shows that Grodzinsky's equivalence of movement and comprehension problem (sect. 2.2, para. 6 and sect. 2.3.1, para. 6) cuts the story too short.

NOTE

1. Note that in Minimalism (Chomsky 1995a), contra Grodzinsky (sect. 2.2, para. 2), the "movement" relation is mediated by a different mechanism (feature checking) compared to Government and Binding theory (GB). It is unclear that Minimalism is compatible at all with Grodzinsky's proposals.

The left frontal convolution plays no special role in syntactic comprehension

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Abstract: Grodzinsky's localization claim can be questioned on empirical grounds. The Trace Deletion Hypothesis fails to account for a number of comprehension facts in Broca's aphasia and conduction aphasics show similar comprehension patterns. Frontoparietal systems are recruited during sentence comprehension only under conditions of increased processing load and/or attentional demands.

Grodzinsky makes an *indirect* argument for localization of the transformational component of syntactic comprehension, which rests minimally on the following premises. (1) Broca's aphasics' comprehension disorder is adequately characterized by the Trace Deletion Hypothesis (TDH). (2) This disruption is a consistent feature of Broca's aphasia, but not of other aphasias. (3) Broca's aphasia is associated with damage to Broca's area and its vicinity. Because the last premise is generally accepted as true, if Grodzinsky could demonstrate the validity of the first two premises, then his conclusion would be warranted. Unfortunately, two classes of empirical observations seriously question their validity.

Unexplained comprehension patterns in Broca's aphasia. The TDH fails to account for comprehension difficulties on the following sentence types: (1) *The matrix clause of center-embedded relatives.* Comprehension of the thematic relation between the subject (dog) and predicate (is brown) in "The dog that chased the cat is brown" is poor (Hickok et al. 1993). The TDH cannot explain this fact, even assuming that this relation is mediated by a trace. (2) *Locative prepositions.* Broca's aphasics perform at chance on sentences like: "The dog is behind the cat" (Crerar et al. 1996; Kolk & van Grunsven 1985a; Schwartz et al. 1980). According to the TDH these should be comprehended on a par with actives. (3) *Simple actives.* Comprehension of actives is far from perfect (see Grodzinsky's Fig. 1). The TDH predicts that performance on passives should be variable, but makes no such prediction for actives because the correct theta-roles are assigned to both noun phrases.

Agrammatic comprehension in posterior aphasia. Several cases of left posterior-lesioned aphasics have been reported with a comprehension pattern like Broca's aphasics. There are at least two case studies of conduction aphasia in which prototypical agrammatic comprehension was noted (Caplan et al. 1986; Friedrich et al. 1985). And Goodglass et al. (1993) found that a group of 7 Broca's and 7 conduction aphasics showed the same degree of comprehension asymmetry between active and passive sentences. Individually, 4 out of 7 Broca's performed worse on passives, whereas 5 out of 7 conduction aphasics did so. Only one patient in each group showed the prototypical pattern (actives above chance, passives at chance); of note, the one Broca's patient had a *posterior* lesion with frontal cortex spared.

An alternative hypothesis. Suppose we assume that all of the basic machinery for syntactic comprehension is contained within temporal lobe systems. This is consistent with the observation that temporal lobe damage is the most reliable predictor of auditory comprehension deficits (Naeser et al. 1987), and that temporal lobe structures are the most reliably activated regions in imaging studies of sentence comprehension (Schlosser et al. 1998). We might then hypothesize that frontal (and perhaps parietal) systems are recruited in sentence comprehension *only under conditions of increased processing load and/or attentional demands.* Such conditions could be induced by a variety of factors, such as slowed presentation rate, listening to long lists of irrelevant sentences, task demands, syntactic complexity, and so forth. Under these conditions conscious processing strategies dependent on frontoparietal systems may be recruited to augment normal sentence comprehension mechanisms. In fact, there is much evidence suggesting that left frontal cortex is recruited under nonautomatic, effortful task conditions during linguistic processing (Fiez 1997): "Verb generation" produces robust frontal activation, but only when subjects are unpracticed; difficult semantic judgments produce more activation than simpler ones; and phonemic monitoring tasks yield more frontal activation than passive listening (Zatorre et al. 1996). Of direct relevance, Cooke et al. (1999) found that object relatives activated Broca's area only when sentences were long (7 words between NP and trace), not when they were short (3 words). The linear relation between left frontal regions and working memory load (Braver et al. 1997) is also supportive of the present hypothesis. Indeed, it may not be coincidental that lesion sites associated with agrammatic comprehension overlap with regions supporting verbal working memory.

Conclusions. We conclude that the TDH is an inadequate characterization of the sentence comprehension facts in aphasia, and that agrammatic comprehension is not exclusively associated with frontal lesions. Grodzinsky's localization claims therefore cannot be maintained. This does not imply, however, that the TDH is useless in explaining aspects of comprehension. It works as well as it does because it captures one dimension of processing load, that associated with structural dependencies. But syntactic load is just one of many factors that contribute to processing demands in laboratory experiments. It is the processing demands *in general* that necessitate the implementation of consciously controlled comprehension strategies mediated by frontoparietal systems.

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The grammar of agrammatism

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Abstract: There are reasons for reservations with respect to the postulated function of Broca's area. Evidence for the psychological reality of the relevant traces does not exist. In addition, because the syntax of non- (or partly) configurational languages is not described in terms of empty categories, no receptive agrammatism should be observed in these languages. Aphasia should not be examined in isolation from its cognitive components.

Understanding agrammatism in typical Broca's aphasia involves methodological difficulties, as does understanding any other intact or disordered cognitive behavior. Theoretical controversies surrounding such an inquiry are often goal-driven. It is sometimes less obvious whether the goal is to describe and classify the behavioral patterns, to find evidence for a particular structural (e.g., linguistic theory) or a particular processing account (e.g., psycholinguistic model), to find a strategy to enhance mental capabilities or to improve cognitive treatment, to localize the cognitive function of a specific neurological subsystem, or a combination of these. The Grodzinsky target article appears to fall into the category of seeking evidence for a particular linguistic (sub)theory by considering (cross)linguistic patterns and a broad range of evidence. Its conclusion is that the neurological subsystem associated with Broca's area only subserves a specific property of syntactic comprehension that involves movements of syntactic constituents. The article is an excellent state-of-the-art review of syntactic research into receptive agrammatism in Broca's aphasia, but there are grounds for reservations about its appropriateness for describing, explaining, and predicting the kind of behavioral and neurological disorder examined.

Linguistic intuitions do not necessarily reflect linguistic processing issues. It might turn out that (some) syntactic traces postulated by generative linguists are mental illusions because linguistic intuitions may reflect cognitive processing reality only to a limited extent. Real-time *auditory* sentence processing evidence with healthy (English) speakers is accordingly not conclusive. For example, in (reversible) passive sentences no *re*-activation of the antecedent (object) at the NP trace position has been found, only a trend to significant priming 1,000 msec downstream from the trace position (cf. Nicol & Swinney 1989). Thus, no processing evidence supports the psychological reality of NP-traces in passive sentences. However, Grodzinsky's account is based strongly on Broca's aphasics' ability to comprehend reversible passive sentences. Their poor performance is obviously not related to their inability to realize NP-traces because these traces do not seem to exist. The failure to find evidence for NP-traces does not necessarily imply that NP-traces are not cognitively real; however, as long as there is no evidence for NP-traces it is certainly premature to postulate that this type of category exists. In addition, it should be pointed out that there is no unambiguous support for the psychological reality of other types of traces involving a syntactic movement (for example, in "raising" such as *Audrey*$_{(i)}$ *seems* t_i *to be invited* or in "questions" such as *Whom*$_{(i)}$ *will Audrey invite* t_i?). Several *re*-access effects in English may be triggered by the verb's argument structure. For example, in SVO languages (such as English) the object may be *re*-accessed by default at the offset of the verb. Thus, much more psycholinguistic evidence is required before certain aspects of the generative program can explain the performance of typical Broca's aphasics in sentence comprehension tasks in functional terms.

Let us assume for a moment that the syntactic traces suggested for English are psychologically real. In this case it might turn out that typical Broca's aphasics whose native language is English do indeed suffer from a disorder in perceiving traces involved in syntactic movements. The question arises, however, whether Broca's aphasic patients would still show a syntactic comprehension deficit if their native language were not fully configurational (Rizzi 1985).

The trace theory describes only languages with configurational structures. "Configurationality" is defined in terms of precedence and dominance. In the hierarchical structure [S[NP1][VP[V][NP2][PP[PP][NP3]]]] the first noun phrase (NP1) precedes the verb phrase (VP), the verb (V), the second noun phrase (NP2), and so on, and is dominated by the sentence node (S); again NP2 precedes the prepositional phrase (PP), the preposition (P), and so on, and is dominated by S and VP, and so forth. Theta-roles (e.g., Agent, Patient, Goal) are assigned by heads of phrases to NPs that bear configurationally defined syntactic relations. In non- or partly configurational languages such as Warlpiri or Hungarian this structural configuration does not provide sufficient information to differentiate syntactic roles configurationally. It is known that Broca's aphasic patients assign theta-roles by chance, and use pragmatic default strategies when they get the opportunity (the "animacy strategy" or "subject-first strategy"; Schwartz et al. 1980).

Given that these pragmatic strategies and other factors such as syntactic preference strategies (canonical word order) are controlled, it might turn out that Broca's aphasics from nonconfigurational languages assign theta-roles randomly in different kinds of sentence structures. They would be impaired in using grammatical markers to determine the syntactic role of NPs but they would not be impaired in the perception of moved constituents. For example, Hagiwara and Caplan's (1990) Japanese data on active sentences simply show that the Broca's aphasics do not compute Case particles and prefer SOV rather than OSV word order. (In addition, the patients may consider not assigning the same syntactic-semantic role twice in a sentential clause). Thus, parsing in Japanese may rely on grammatical markers, but not on a configurational strategy (as in English). Moreover, if we accept language-specific theories of agrammatism, the neurological structure responsible for language processing would be determined by language-specific properties. According to this account, we would have a result that is contrary to the generative approach that intends to describe the language system as a biological faculty.

A number of alternative factors are crucial for the performance of Broca's aphasic patients in receptive tasks. For example, the temporal parameters for *re*-accessing lexical information seem to be impaired (e.g., Hillert 1999); a working memory deficit (e.g., impaired articulatory sequencing) might in turn be responsible for this. It is also unknown to what extent Broca's aphasic patients are sensitive to different forms of morphologically realized Case information in transitive and ditransitive structures during real-time sentence comprehension. A crosslinguistic approach is certainly the right one, but only when language-specific properties, as well as other important cognitive factors involved in sentence processing, are sufficiently considered. Until now, the theoretical account in question does not seem to have the power to account for receptive agrammatism in Broca's aphasia.

Comprehension deficits of Broca's aphasics provide no evidence for traces

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Abstract: The data provided by Grodzinsky demonstrating a syntactic comprehension deficit in Broca's patients provide no evidence for the theoretical concepts of movement, trace or "trace deletion." The comprehension deficit data can be more economically accounted for with traditional grammatical concepts that are less theory-internal and more empirically based.

All of the comprehension data Grodzinsky presents can be accounted for without reference to movement, traces or "trace-deletion." The issue is significant because, despite the essential role of movement and traces in orthodox transformational grammar (Chomsky 1981; 1986; 1995a), there exists substantial evidence that movement and traces are linguistically unnecessary (Ades & Steedman 1982; Gazdar et al. 1984; Kaplan & Zaenen 1989; Kay & Fillmore 1999; Pollard & Sag 1994, Ch. 9; Sag 1999; Sag & Fodor 1994) and psycholinguistically unjustified (Pickering & Barry 1991; Sag & Fodor 1994). The pattern of Grodzinsky's comprehension data can be economically formulated without reference to movement or traces, let alone trace-deletion, using only the traditional notions of argument and logical subject.

The concept of logical subject is illustrated in example (1). The logical subject of the verb *drive* appears in bold type and coarguments of the logical subject appear in italics:

(1) a. **Leslie** drove *my car.*
b. *My car* was driven by **Leslie.**
c. *My car* has been driven too much lately.
d. It was **Leslie** who drove *my car* that day.
e. It was *my car* that **Leslie** drove that day.
f. *My car* drives like a Model T with square wheels.
g. *My car,* **Leslie** drives whenever possible.

Pretheoretically, there is something uncomplicated about a simple, active, declarative clause like (1)a. The semantic argument of the verb that is realized as syntactic subject in such a clause, is traditionally called the logical subject. (Actually, "logical" is a misnomer. Logic has nothing to do with the matter.) In Chomsky's early formulation of transformational grammar (1957), the deep structure subject was a formal incarnation of the traditional notion of logical subject. Sentences like (1)a were licensed by a set of context-free phrase structure rules augmented by a set of obligatory transformations and were called "kernel sentences." Sentences like (1)b–f were derived from kernel sentences by the application of optional transformations.[1]

The concept of logical subject is closely related to the predominant clause type of a language. English is considered a Subject-Verb-Object (SVO) language because of the favored status of sentences like (1)a. Chinese is also an SVO language. Japanese is an SOV language. So, by happenstance, these three languages share the property of having the subject come first in the canonical clause. Consequently, the interpretive strategy employed by the English-, Chinese- and Japanese-speaking Broca's patients may be formulated as follows, where the initials LSF stand for "Logical Subject First."

(2) LSF: A logical subject precedes its coarguments.

Grodzinsky's comprehension data are tabulated in full in Table 1, where logical subjects are shown in boldface and nominal coarguments of logical subjects in italics. (The numbering is Grodzinsky's.) The words "success" and "failure" in the third column indicate whether or not Grodzinsky's subjects did better than chance at interpreting the sentence type in question.[2] According to the LSF strategy, failure of comprehension by the Broca's patient should occur if and only if a NP in italics precedes a NP in boldface within a clause.

Examples 4a and b in Table 1 illustrate the active/passive alternation in English, in which only the former has the logical subject first, as required by LSF, and is successfully interpreted by the Broca's patients. In 5a and b the clause in question is the bracketed relative clause. Again, LSF makes the correct predictions.

Table 1 (Kay). *Grodzinsky's comprehension data, showing linear order of* logical subject *and* nonlogical-subject *arguments*

(4) a	**The boy** pushed *the girl.*	success
b	*The girl* was pushed by **the boy.**	failure
(5) a	The boy [**who** pushed *the girl*] was tall.	success
b	The boy [*who* **the girl** pushed] was tall.	failure
(6)	*The girl* was admired by **the boy.**	failure
(1) c	Show me the girl [**who** pushed *the boy*].	success
d	It is the girl [**who** pushed *the boy*].	success
e	**The boy** was interested in *the girl.*	success
f	**The woman** was uninspired by *the man.*	success
(2) c	Show me the boy [*who* **the girl** pushed].	failure
d	It is the boy [*who* **the girl** pushed].	failure
e	*The woman* was unmasked by **the man.**	failure
(7) a	**Taro-ga** *Hanako-ni* nagutta.	success
	Taro Hanako hit	—
	Taro hit Hanako.	—
b	*Hanako-o* **Taro-ga** nagutta.	failure
	Hanako Taro hit	—
	Taro hit Hanako.	—
(8) a	*Taro-ga* **Hanako-ni** nagu-rare-ta.	failure
	Taro Hanako hit-pas-past	—
	Taro was hit by Hanako.	—
b	Okaasan-ga [**musuku-ni** *kaze-o* hik-are-ta].	success
	mother-nom [son-by cold-acc catch-pas-past]	—
	A mother had her son catch a cold on her.	—
(10) a	zhuei *gou* de **mau** hen da	failure
	chase dog that cat very big	—
	A/the cat that chased a/the dog was very big.	—
b	**mau** zhuei de *gou* hen xiao	success
	cat chase that dog very small	—
	The dog that the cat chased was very small.	—

Example 6 has the same structure as example 4b and LSF again makes the correct prediction of failure.

Examples 1c and 1d are to be contrasted with examples 2c and 2d. In the former case the relative clause has the preferred bold > italic order: Success is predicted by LSF and is observed. In the latter case, the dispreferred italic > bold order occurs in the relative clause and failure of the Broca's subjects is correctly predicted by LSF. Examples 1e and 1f illustrate adjectival predications, where the (raised) logical subjects of the adjectives *interested* and *uninspired* appear as the initial NPs of their clauses and are correctly interpreted. Despite superficial similarity to 1f, 2e represents, not a copula-plus-adjective construction, but a true passive, with the logical subject realized in a *by*-phrase. (*The man unmasked the woman* is well formed; **The man uninspired the woman* is not.) Again in 2e, failure is correctly predicted by the LSF strategy.

Turning now to the Japanese examples, 7–8, the so-called unscrambled and scrambled examples, 7a and 7b, respectively, present an analogue to the English active/passive alternation, 4a and 4b: When the logical subject precedes its coargument(s) (7a), Broca's patients interpret successfully, as predicted by LSF. When the order is reversed in 7b, unsuccessful interpretation occurs, again predicted by LSF. The *-rare*-passive in 8a behaves like the English passive: Precedence of a nonlogical-subject argument produces unsuccessful interpretation, as predicted by LSF. The analysis of 8b is slightly more complex. *Okassan* ('mother') is not the logical subject of *hik*- 'catch,' but it is not an argument of *hik*- at all. The fact that *okassan-ga* precedes the logical subject of *hik-are-ta*, namely **musuku-ni** ('son-accusative'), does not therefore constitute a violation of LSF. The appearance of **musuku-ni** to the left of *kaze-o* 'cold-accusative' correctly predicts the observed absence of difficulty for Broca's patients in interpreting sentences of this type.[3]

The Chinese examples in 10a and b are intransitive sentences containing transitive relative clauses. The relative clause has a cat chasing a dog and the main clause describes the size of one of these animals. The facts of relevance are the relative orders of the logical subject **mau** 'cat' (of *zhuei* 'chase') and its coargument (*gou* 'dog'). In (10)a *gou* precedes **mau** and LSF correctly predicts failure. In (10)b **mau** precedes *gou* and LSF correctly predicts success.

Grodzinsky's comprehension data show a pattern of syntactic deficit in Broca's aphasics, but they do not furnish evidence for the reality of movement or traces.[4] More generally, the data presented in Grodzinsky's target article suggest that Broca's aphasics may rely on a small number of parsing strategies based on the most frequent construction types of their languages.

NOTES

1. Without, however, leaving traces, which did not exist in early TG. It is possible that early transformational grammar, lacking traces, would have been capable of describing Grodzinsky's comprehension facts in terms of the degree to which a clause fit the pattern of "kernel" sentences.

2. To simplify the discussion and because Grodzinsky's examples 1a and b and 2a and b are repeated (with trivial interchange of *girl* and *boy*) in his 4a and b and 5a and b, I have listed examples 4, 5, and 6 before 1 and 2 in Table 1 and eliminated examples 1a and b and 2a and b. Similarly, Grodzinsky's examples (11)a and b have been eliminated from Table 1 because they are structurally identical to his examples (5)a and b and yield the same results.

3. To be sure, the Japanese Broca's aphasia patient is going to need something beyond the LSF strategy to process this type of sentence successfully. Limitation of space prohibits further discussion of this point.

4. Grodzinsky states that "the presentation here is compatible with . . . the Minimalist Program (Chomsky 1995a)" (sect. 2.2). This is not obvious. According to the Minimalist Program, all NPs and all verbs must move from their original location to the neighborhood of an appropriate functional head to get their features "checked." Hence every sentence contains traces. The Trace Deletion Hypothesis for Broca's aphasics combined with a Minimalist grammar would appear to predict that Broca's aphasics would not have intact syntactic comprehension for any sentence.

Could grammatical encoding and grammatical decoding be subserved by the same processing module?

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Abstract: Grodzinsky interprets linguistic differences between agrammatic comprehension and production symptoms as supporting the hypothesis that the mechanisms underlying grammatical encoding (sentence formulation) and grammatical decoding (syntactic parsing) are at least partially distinct. This inference is shown to be premature. A range of experimentally established similarities between the encoding and decoding processes is highlighted, testifying to the viability of the hypothesis that receptive and productive syntactic tasks are performed by the same syntactic processor.

One of the issues addressed in the target article concerns the cognitive architecture of human syntactic processing. Grodzinsky argues that the sentence production deficit in agrammatic patients should be characterized in different linguistic terms than their sentence comprehension deficit: Tree Pruning versus Trace Deletion. From this, he infers that "mechanisms for the planning and construction of sentences must diverge at some point from those dedicated to the analysis of incoming strings" (sect. 2.7.4). In other words, he interprets linguistic differences between agrammatic comprehension and production symptoms as support for the hypothesis that the modules underlying grammatical encoding (sentence construction) and grammatical decoding (parsing) "are at least partially distinct" (sect. 4). In this commentary, I do not wish to take issue with Grodzinsky's characterizations of the basic disorder in the two grammatical processing modalities, or with the assumption that these modalities share "one grammatical resource" (sect. 2.7.4). My aim is to show that the inference from differential symptomatology to distinct processing modules is premature.

To prevent misunderstandings, I assume that the mechanisms "for the planning and construction of sentences" and for "the analysis of incoming strings" in the above quotations refer to *syntactic* processors and do not include other mechanisms involved in language production (e.g., planning of the conceptual content or the phonological and phonetic shape of utterances) and language comprehension (such as auditory or visual word recognition or semantic interpretation). Otherwise, the assertion of (partially) distinct mechanisms underlying language production and comprehension would be trivially true.

The problem inherent in the above-mentioned questionable inference is that the *ceteris paribus* condition has been overlooked. Suppose that, contrary to what Grodzinsky is arguing, our cognitive system has a single processing mechanism for syntax assembly that is used for *constructing* syntactic structures (grammatical encoding in sentence production), as well as for *reconstructing* syntactic structures (parsing, grammatical decoding in sentence comprehension). When functioning as encoder, this processor operates on the basis of lexico-syntactic information associated with conceptual structures ("messages"). When in decoding mode, such information derives from word strings recognized in the input. These and possibly further differences between the two modalities of syntactic processing may be said to constitute different processing contexts. The differential linguistic symptomatology Grodzinsky observed in the two modalities thus may be a consequence of differences between the processing contexts in which the single syntactic processor is deployed. Therefore, the conclusion that "mechanisms that underlie language production are at least partially distinct from the comprehension device" (sect. 4) does not necessarily follow.

One could object that this line of reasoning has no practical consequences because the single-processor assumption for grammatical encoding and decoding is highly unlikely *a priori* and at variance with empirical data. A popular argument in support of this view is based on the phenomenon of self-monitoring of overt or covert speech, which seems to involve the simultaneous operation of grammatical encoder and decoder, that is, of two syntactic processors. However, a single syntactic processor can accomplish self-monitoring by switching between encoding and decoding modes ("timesharing"). Various additional empirical and theoretical arguments have been advanced in favor of dual-processor architectures for syntactic processing but they are dubitable at best (Kempen 1999).

More important, a comparison of empirical data on grammatical encoding (formulating) and decoding (parsing) suggests that these processes operate on very similar principles. Consider the following commonalities (for details and references, see Kempen 1999):

1. Sensitivity to conceptual factors. The formulator takes conceptual structures as input. The syntactic parser interacts with the conceptual interpretation process concerning the plausibility of the conceptual message implied by the current parse tree.

2. Direct mapping between conceptual (thematic) and syntactic relations. The formulator assigns conceptual-to-syntactic relationships directly, without intermediate steps that reshuffle the mappings, such as active-to-passive transformations. Similarly, the parser maps syntactic-to-thematic relationships in one step.

3. Incremental processing. Syntactic trees grow from left to right, in tandem with the unfolding of a conceptual message (in formulating) or a string of words (in parsing).

4. Determinism. When analyzing a sentence, the parser comes up with one analysis; likewise, the formulator delivers one sentence expressing a given conceptual message.

5. Similar empirical profiles. Parsing and formulating have been found to react similarly to experimental manipulations such as the following:

a. Lexical frame preferences. Words often have more than one lexical frame (subcategorization frame) associated with them; for example, many verbs can be used transitively or intransitively. In such cases speakers may prefer one frame to another. Lexical frame preferences have been shown to affect sentence production and sentence comprehension in similar ways.

b. Syntactic priming. Speakers tend to repeat a syntactic construction in consecutive utterances when the conceptual message and the lexical material afford them the opportunity. Structural similarity of consecutive sentences also facilitates comprehension.

c. Agreement errors. Speakers sometimes violate rules of grammatical agreement, for example, number agreement between subject and verb of finite clauses. The factors controlling the incidence of such errors have been studied in much detail. Sentence comprehension appears to include an agreement-checking component that is sensitive to the same factors.

d. Structural complexity effects. Structurally more complex sentences are harder to understand and, all other things being equal, occur less frequently in spoken or written text corpora.

This list of similarities testifies to the viability of the hypothesis that in human language users receptive and productive syntactic tasks are performed by the same syntactic processor. In conjunction with the foregoing this implies that the differential linguistic symptomatology Grodzinsky observed in agrammatic sentence production and sentence comprehension does not undermine the position that in human language users grammatical encoding and grammatical decoding are subserved by the same processing mechanism. In Kempen (1999), I propose an account of the differential symptomatology within a single-processor framework.

Agrammatic sentence processing: Severity, complexity, and priming

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Abstract: Grodzinsky's theory of agrammatic sentence processing fails to account for crucial empirical facts. In contrast to his predictions, the data show that there are (1) degrees of severity and (2) problems with sentences that do not require movement, and that (3) under the right task circumstances, full-fledged syntactic trees are constructed.

1. Only one degree of severity? No! With respect to comprehension, Grodzinsky claims that the ability to represent or process traces is completely gone in Broca's aphasia. The a priori likelihood of this claim is very low. Is there any aphasic deficit that is all-or-none? With respect to agrammatic comprehension, there is direct evidence to the contrary. In Kolk and van Grunsven (1985a) we demonstrated that our group of Dutch-speaking agrammatics had a performance profile that exactly matched the profile of an English-speaking group of patients, studied by Saffran et al. (1980) and Schwartz et al. (1980). This profile was defined over three different sentence types and two different tasks (sentence picture matching and sentence order tasks). The only difference between the two groups was in their absolute level of performance: The first group performed about 25% better. It seems hard to escape the conclusion that the two groups had the same underlying impairment but a different degree of severity. What is Grodzinsky's response to this evidence? In a recent meta analysis (Grodzinsky et al. 1999), he excluded 9 out of the 11 patients described in the Kolk and van Grunsven study, despite the fact that they all presented with high rates of function word omission, which is generally taken as the central grammatical symptom of Broca's aphasia. The deviations from the classical Broca pattern that motivated this exclusion had to do with the fact that these patients were somewhat less impaired with respect to prosody, articulation, and/or phrase length (see Berndt & Caramazza, 1999, for a comment on this selection procedure).

Severity variation is also apparent in production. Hofstede and Kolk (1994), for example, report large amounts of individual variation in the omission of determiners and prepositions. In this area Grodzinsky – surprisingly – does acknowledge the existence of severity variation (see Friedmann & Grodzinsky 1997). Severity variation is assumed to arise from variation in the syntactic location of the defective node in the phrase marker. According to these authors, however, damage to a particular node is still an all-or-none affair. Although this is a much more defensible claim than the one for comprehension, it is not supported by empirical fact. If the Tense node is damaged, then the agrammatic aphasics should invariably present with an excessive use of the infinitival form in languages like German and Dutch. However, this aspect is found to show substantial variation, as well. Hofstede and Kolk (1994) computed a parameter called "finiteness omission," which includes (over)use of the infinitive. In their group of 19 Broca's aphasics, this parameter varied from about 10% up to more than 90%.

2. Just a movement deficit? No! Besides difficulties with noncanonical structures, agrammatic aphasics also exhibit problems with embedding. In particular, when presented with sentences with center-embedded clauses, they make many errors in interpreting the matrix clause, which has canonical word order (cf. Hickok et al. 1993; Kolk & Weijts 1996). In the latter study, performance on the matrix clause was as impaired as performance with noncanonical sentences. Grodzinsky comments on these findings by claiming that "such difficulties have little to do with structure, but rather with some general processing difficulty these patients may be suffering from" (Grodzinsky 1995b, p. 475). What is the empirical evidence for this claim? Or could it be that

Grodzinsky believes that a problem with traces cannot derive from a processing disorder? In fact, both the difficulty with noncanonicity and the one with embedding can in principle be accounted for by a processing model that incorporates trace formation as a processing component (c.f. Haarmann et al. 1997). It is even possible that trace formation does not constitute an independent source of processing load and that the canonicity effect arises as the consequence of word-order strategies (c.f. Kolk & Hartsuiker 1999; Kolk & Weijts 1996). If this sounds too farfetched, it is important to realize that for Grodzinsky as well the difference between canonical and noncanonical sentences results from strategy use and not from the presence or absence of traces.

3. Just tree truncation? No! For agrammatic production, Grodzinsky puts forward the hypothesis that the agrammatic syntactic trees are truncated at the level of a particular node, typically the tense node. As support for this hypothesis, Grodzinsky points out that in Dutch agrammatic speech, verbs in main clauses not only appear uninflected, but are also in sentence-final position. He says this has to do with the fact that Dutch is an SOV-language, meaning that the base-generated position of the verb is after the object. To produce an inflected verb in a main clause (which has the SVO-order), the verb has to move and this movement is prohibited by the truncation. The result is an absence of inflection and no change in verb position. As supporting evidence, Grodzinsky refers to the study of Kolk and Heeschen (1992), which would show the predicted pattern. And indeed, the right side of Table 5 in Kolk and Heeschen (1992) does show the required pattern: uninflected verbs in final position. The table also has a left side, however. This left side demonstrates that these same patients produced an even larger set of utterances with inflected verbs in SVO position. Similar findings are reported in de Roo (1999). This means that although the patients make excessive use of root infinitives, they are – on the whole – by no means unable to produce inflected verbs in medial position (contrary to the claim made by Bastiaanse & van Zonneveld 1998). This is also demonstrated in the study by Hofstede and Kolk (1994), who showed that finiteness omission strongly decreased in a picture description task as compared to spontaneous speech. Parenthetically, Grodzinsky dismisses utterances with clause-final uninflected verbs as "ungrammatical." Yet such utterances are certainly part of the elliptical register in normal speech (Kolk & Heeschen 1992).

There is another recent finding that seems hard to reconcile with the truncation hypothesis. This finding concerns the production of the passive, which is notoriously hard for aphasics. The truncation hypothesis would explain this difficulty as a consequence of an inability to perform movements to a position in the tree higher than the affected node. This account predicts that agrammatics cannot be induced to produce passives. However, Hartsuiker and Kolk (1998) elicited passives from agrammatic patients in a sentence priming task. They showed that repetition of an unrelated passive consistently led the patients to produce considerable numbers of passive picture descriptions, which they did not in spontaneous speech.

The brain does not serve linguistic theory so easily

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Abstract: It is a major move from the claim that the core linguistic problem in Broca's aphasia is the inability to deal with traces, to the claim that this is the syntactic operation only and that it is exclusively supported by Broca's region. Three arguments plead against this move. First, many Broca patients have no damage to Broca's area. Second, it is not only passive, but also active jabberwocky sentences that activate the frontal operculum in a judgment task. Third, the same area is involved in a phrase-building production task that does not require tense processing.

Most of the evidence Grodzinsky marshals in support of his Trace Deletion Hypothesis (TDH) is aphasiological, the more or less carefully tested linguistic performance of Broca's patients. For the sake of argument I will accept both the evidence and Grodzinsky's linguistic analysis thereof. It is, however, a major additional step to attribute the core linguistic problem in Broca's aphasia, the inability to deal with traces, to damage in Broca's area and/or its immediate vicinity. Damage to that region is neither a necessary nor a sufficient condition for the syndrome of Broca's aphasia to arise (Willmes & Poeck 1993). TDH predicts that Broca's patients without damage to that region should be perfectly all right as far as dealing with traces is concerned. And damage to just that region, even if it does not result in Broca's aphasia, should be sufficient to create the processing problems predicted by TDH. Is that really intended? If so, does the author have any evidence to support these strong claims?

Probably, only functional neuroimaging can provide the critical test, but the available studies are too few and not specific enough to test TDH. The only conclusion they allow Grodzinsky to draw is that the evidence does not speak against the hypothesis. Two comments are in place here, both concerning recent neuroimaging studies. First, there is the follow-up study to Stromswold et al. (1996) by Caplan et al. (1998). In this PET subtraction study, subjects read sentences that contained a center-embedded or right-branching relative clause and judged them for their plausibility. As in the Stromswold et al. study, the center-embedded condition triggered more activation in the pars opercularis of Broca's area than the easier right-branching condition did. Clearly, because these sentence types differ in the distance between phrase and extraction site, the additional activation of Broca's area in handling center-embedding structures is supportive for TDH. It should be added, though, that there are also straightforward phrase structural differences between the two types of sentence. If the specific expertise of Broca's area is phrase structural (contrary to TDH), then these differential activations were still to be expected.

Second, two even more recent studies do test a critical aspect of TDH, namely, that Broca's region does *not* support any other syntactic operations than the specific ones formulated in TDH ("only these," target article, sect. 4). These two tests violate TDH. One study, an event-related fMRI experiment by Meyer et al. (1999) compared (among other things) a syntactic jabberwocky condition (in German) to a rest condition. Subjects judged the test sentences on "being syntactic" and on "containing pseudowords." Broca's region, in particular, the left frontal operculum (i.e., the small region directly caudal to Broca's area), was activated by both active SVO sentences and by their passive equivalents. There was no statistical difference between the two conditions. The activation by passive jabberwocky is predicted by TDH, the activation by active SVO jabberwocky violates the "only" clause of TDH.

The other study, by Indefrey et al. (1999b), is a PET study of syntactic production. Here, subjects saw Michotte-type launching events. In condition 1 they provided full-sentence responses (such as the German equivalent of *the red square launches the blue ellipse*). In condition 3 they provided word list responses (such as *square red ellipse blue launch*). A single, highly specific difference activation was obtained for the full syntactic response – in the frontal operculum. The major syntactic difference between the two response types is phrase-structural, not transformational. Still, Grodzinsky could argue that in a production task TDH predicts involvement of Broca's region in tense processing, moving the German clause-final verb to its tensed second position. But here condition 2 of the experiment gets in the way. The subjects' task was restricted to NP building, producing responses of the type *red square, blue ellipse, launch* (notice that in German, this involves establishing gender agreement between noun and adjective). This condition also evoked significantly stronger activation of the frontal operculum (and of no other area) than the word list condition did (though less so than the full sentence condition). I see no way of reconciling these results with TDH.

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Agrammatism, syntactic theory, and the lexicon: Broca's area and the development of linguistic ability in the human brain

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Abstract: Grodzinsky's Tree-Pruning Hypothesis can be extended to explain agrammatic comprehension disorders. Although agrammatism is evidence for syntactic modularity, there is no evidence for its anatomical modularity or for its localization in the frontal lobe. Agrammatism results from diffuse left hemisphere damage – allowing the emergence of the limited right hemisphere linguistic competence – rather than from damage to an anatomic module in the left hemisphere.

Using the Principles and Parameters framework, Grodzinsky tries to show that agrammatism is the result of damage either to a primary linguistic processor devoted to syntactic transformational operations or to a nonlinguistic slave system critical for running specific syntactic routines (e.g., phonological short-term memory). He further argues that Broca's area (or a topographically extended "grand Broca" anterior perisylvian area) would be the crucial site for this functional processor. On the one hand we would like to push further the implication of his Tree-Pruning Hypothesis; on the other, we wish to discuss his historical premises, and to reconsider his sharp assumptions in favor of a left frontal localization of the syntactic processor.

1. Linguistic perspectives on agrammatism. Grodzinsky's Trace Deletion Hypothesis (TDH) explains the failure of agrammatic patients to comprehend constructions involving transformations such as reversible passives and SO reversible relative clauses. However, it falls short of explaining the nonmandatory character of this failure (Berndt et al. 1996; Luzzatti et al. 1999; Miceli et al. 1983). Agrammatism is a condition in which production is also severely affected. In spontaneous speech and in reading (phonological dyslexia), agrammatic patients often omit unbound functional morphemes (e.g., articles, prepositions) or substitute inflectional affixes so as to produce less marked lexical forms. Although Grodzinsky does not focus on this, he tackles the production weaknesses from a particular perspective that has proven very fruitful in accounting for language acquisition (Wexler 1994) and specific language impairment (SLI) in childhood (see Clahsen et al. 1997; Rice & Wexler 1996).

Basing his position on linguistic theory Grodzinsky claims that agrammatic patients are selectively impaired in the use of tense but not of agreement morphemes. This selective impairment has a structural implementation that is expressed by the tree-pruning hypothesis, according to which the tense node that dominates the Agreement node is pruned (see his Fig. in Ex. 28). Consequently, every projection above tense phrase, TP (for example, complementizer phrase, CP) is deemed to be absent from an agrammatic grammar. Whether or not the tree-pruning hypothesis is correct and generalizable to other aspects of the agrammatic disorder (see above), it paves the way for a whole range of interesting questions and falsifiable predictions.

One can attempt a unification of the comprehension and production disorders by exploiting the consequences of the tree-pruning hypothesis. Assuming the correctness of Grodzinsky's assumptions about the clausal architecture (but see Belletti 1990; Guasti & Rizzi 1999), the tree-pruning hypothesis offers an immediate explanation for the difficulties that agrammatic patients have in comprehending relative clauses. The syntactic representation of agrammatic patients cannot include the tense node or any higher one; specifically, it cannot include the CP, a node that is required to accommodate relative clauses. If agrammatics cannot assign the appropriate structure to relative clauses, they can hardly interpret them correctly, regardless of their ability to handle traces. An explanation along similar lines can be devised for passives. The subject of a sentence must move to the specifier of TP (see the tree in Ex. 28 in Grodzinsky's target article). Again, if TP cannot be projected, subjects cannot be moved there. Whatever representation agrammatic patients assign to a passive sentence, it is not the correct representation, and this suffices to explain their failure to interpret reversible passives. This perspective raises different theoretical and empirical questions, but it is a natural development of Grodzinsky's approach.

2. Historical background and coda. Paul Broca was neither a connectionist nor a diagram maker but a surgeon with rough psychological knowledge who had the opportunity to make the postmortem observation of Monsieur Tan-Tan's cerebral lesion and tried to support Gall's and Bouillaud's functional localization of the speech faculty in the frontal lobes. He also was the first in demonstrating the left-right functional asymmetry of language processing in the brain.

A decade later, Wernicke drew his famous diagram in which he introduced the dichotomy between auditory and motor images of words. Since Wernicke, and during the next 30 years, Broca's area was for the German scholars the site of what we now call the phonological output lexicon, and for most French scholars (e.g., Lecours & Lhermitte 1976; Marie 1906) it was a center for articulatory motor control. This characterization of Broca's area as a center for the motor control of articulation is extremely reasonable, because Broca's area is right in the middle of the associative cortex for the bucco-pharyngeal and laryngeal praxic motor control. The association between morpho-syntactic disorders and frontal lesions was first made at the beginning of this century after Bonhoeffer's (1902) description of agrammatism.

3. Agrammatism and syntactic modularity. Grodzinsky claims, and we fully agree, that agrammatism is evidence for a modular organization of language in the left hemisphere. We doubt, however, that agrammatism provides clear support for functionally localizing a single linguistic processor in the left frontal lobe that is devoted to specific syntactic computations. A century of anatomy-function correlations in aphasia and more recent brain imaging (PET, fNMR) studies have provided scant evidence for the localization of single aphasic features or of clusters of symptoms (e.g. Caplan et al. 1996; Vanier & Caplan 1990).

Agrammatism seems to reflect extensive damage to the left hemisphere (LH) linguistic representations (functionally – but not necessarily anatomically – modular), which causes the emergence of less developed right hemisphere (RH) linguistic abilities. These are evident (1) at the lexical level in word class (nouns > verbs > function words), word frequency and imageability effects, (2) at the morphological level in a limited ability to process bound morphemes, (3) at the syntactic level (TDH, etc.), and (4) at the level of short-term memory in reduced short-term phonological capacity.

In this perspective the variability observed among aphasic (and agrammatic) patients is not the result of the isolated involvement of anatomically independent processors, as suggested by Grodzinsky; it arises as a consequence of the variable degree of linguistic competence that the RH displays after a lesion of the LH.

A big "housing" problem and a trace of neuroimaging: Broca's area is more than a transformation center

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Abstract: Grodzinsky presents interesting data on Broca's aphasia, but because of obsolete ideas about neurofunctional organization and an inadequate review of the neuroimaging literature, he fails to put these data into perspective. Rather than supporting a specific linguistic function of Broca's area, the findings should be viewed in terms of working memory functions of the inferior frontal cortex.

Grodzinsky's attempt to characterize Broca's aphasia as a highly specific deficit defined in terms of abstract linguistic principles yields some interesting results. Anyone who is familiar with the principles of functional organization in the brain will agree with him that most linguistic and indeed syntactic functions are not "localized" in Broca's area.

Apart from this, Grodzinsky's target article is reminiscent of Fodor's (1983; 1985) "modularity of mind" and the way Fodor drew inspiration from Gall's phrenology (Gall & Spurzheim 1810) and from classical localizationist neurology. Grodzinsky promises to discuss the *neurology* of syntax and shows why linguistic sophistication is insufficient when accompanied by neuroscientific views that are not state-of-the-art. He critically reviews the nineteenth century localizationist notion of language "centers," but his own objectives appear to be informed by the very same obsolete metaphor (albeit adapted to the conceptual framework of generative grammar). Thus, Grodzinsky tells us that Broca's area does not "house" syntax as a whole, presupposing that issues of neurofunctional organization can be dealt with in "residential" terms. He even suggests that "language *resides* in the left hemisphere" (my emphasis; sect. 5.2). The latter statement is probably not meaningful enough to be true or false, but it is clearly misleading. The same applies to Grodzinsky's claim that Broca's area exclusively "handles" intrasentential dependency relations.

Contrary to Grodzinsky's assertion, the neuroimaging literature is inconsistent with his views. Caplan and colleagues (Caplan et al. 1998; 1999; Stromswold et al. 1996), to whose work he refers, have indeed presented their PET studies as support for a special role of the left inferior frontal cortex in syntactic processing. Unfortunately, this conclusion is based entirely on a privileged statistical treatment of Broca's area (to which lower significance thresholds are applied than to other brain regions), motivated by the *a priori assumption* of this regions' syntactic specialization. Grodzinsky accepts this somewhat circular procedure and states that in the study by Stromswold et al. (1996) the comparison of center-embedded versus right-branching sentences resulted in activation in Broca's area and *nowhere else.* In truth, in the studies by the Caplan group that compared comprehension of sentences of differential syntactic complexity, the most robust activations occurred in regions *outside* Broca's area.

Admittedly, this is not incompatible with Grodzinsky's position, for processing unrelated to transformations involving moved constituents could account for activations in other brain regions. However, his claim that Broca's area deals *exclusively* with such transformations is clearly at odds with the neuroimaging literature. The vast majority of language imaging studies have used lexicosemantic tasks that did not involve transformational computations – and yet the left inferior frontal cortex is among the most consistent sites of activation in these studies (but for some examples, see Cuenod et al. 1995; Grabowski et al. 1998; Herholz et al. 1996; Martin et al. 1995; McCarthy et al. 1993; Ojemann et al. 1998; Petersen et al. 1989; Vendenberghe et al. 1996; Warburton et al. 1996). The claim that Broca's area is not involved in lexicosemantic functions is therefore unwarranted.

Grodzinsky's assertion that language "is" in the left hemisphere goes along with his apodictic statement that no combinatorial language abilities "reside" in the nondominant cerebral hemisphere. As he concedes, the "dominant" hemisphere is not always the left. Yet he seems to suggest that there is always a distinctly dominant hemisphere, that is, one that "houses" the entirety of combinatorial language abilities (the other hemisphere doing something completely different). Grodzinsky discusses split-brain and adult lesion patients for supportive evidence. Of greater relevance, however, are patients with left-hemisphere lesions occurring *before* functional hemispheric asymmetries are established. Behavioral (Mariotti et al. 1998; Vargha-Khadem et al. 1997; Vargha-Khadem & Mishkin 1997) and neuroimaging studies (Müller et al. 1999) document the brain's developmental potential to allocate language processing to the contralesional right hemisphere. Furthermore, early left lesions are often associated with *bilateral* organization of language (Helmstaedter et al. 1997; Rasmussen & Milner 1977; Rey et al. 1988). Whereas the left hemisphere typically has a competitive edge over the right in linguistic (including syntactic) processing, hemispheric asymmetries cannot be captured by all-or-none distinctions, contrary to Grodzinsky's assertion (Dean 1986).[1]

Grodzinsky uses the terms "Broca's area" and "Broca's aphasia" as if these terms had established definitions. He suggests that some findings that are inconsistent with his Trace Deletion Hypothesis (TDH) may result from the inclusion of patients other than "Broca's aphasics." Rather than understanding individual variation as a phenomenon from which we can learn about neurofunctional organization, Grodzinsky considers variation to be noise. Because his objective is to identify the functional specialization of Broca's area, criteria for "Broca's aphasia" should be lesion site and size. Precise information about lesion location is accordingly crucial – but mostly not provided.

Although Grodzinsky's proposal is thus misleading for many reasons, the findings he reviews remain quite interesting. Unfortunately, his modularist zeal prevents him from putting these findings into a context in which they actually make sense. In numerous functional imaging studies, the dorsolateral prefrontal cortex of the left hemisphere, including "Broca's area," has been found to activate during working memory tasks (Cabeza & Nyberg 1997; Callicott et al. 1999), especially for verbal materials (Braver et al. 1997; Fiez et al. 1996b; Paulesu et al. 1993). Grodzinsky briefly mentions a corresponding study by Jonides et al. (1997), rejecting the conclusion of Broca's area supporting a "memory cell." I agree: There is no such thing as a "memory cell" in the brain, so it cannot be in Broca's area. Apart from this, the working memory imaging studies falsify Grodzinsky's postulate of a narrow functional specialization of Broca's area (transformational computations only). Moreover, they suggest that trace deletion may occur in patients with left anterior lesions because of reduced working memory capacity, especially in the comprehension of verbal sentential material.

NOTE

1. For example, Tzourio et al. (1998) found relative symmetry of temporal and frontal activations in left-handers (compared to right-handers) during story comprehension.

The Trace Deletion Hypothesis in relation to partial matching theory

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Abstract: Grodzinsky has argued that the traces deleted in Broca's aphasia are "phonetically silent but syntactically active" (sect. 2.). If we assume such traces to be visuospatial in nature, and adopt the term "overwriting" from the author's partial matching theory (1998), we can account for the errors made by Broca's aphasics in comprehending Grodzinsky's Examples (5a), (5b), and (6).

I wish to suggest that Grodzinsky's analysis of certain comprehension errors associated with Broca's aphasia is also consistent with the view that Broca's aphasics compensate for a deficit in phonetic encoding by using visuospatial encoding.

I shall concentrate only on Grodzinsky's Examples (5) and (6) from section 2.2 of his target article. Example (6) was used to demonstrate that when Broca's aphasics were given the sentence "The girl was admired by the boy" and were then asked to choose between a picture of a girl and a picture of a boy in answer to the experimenter's question "Who was admired?" these patients erroneously chose the picture of the boy. But given Example (5b), the sentence "The boy who the girl pushed was tall" and asked "Who was pushed?" Broca's aphasics chose the picture of the boy and the picture of the girl about equally often (chance performance). And, given example (5a), the sentence "The boy who pushed the girl was tall" and asked "Who did the pushing?" Broca's aphasics correctly chose the picture of the boy.

Broadbent (1958) introduced late twentieth-century views on short-term memory by hypothesizing that, following multiple simultaneous sensory inputs (for example, two nonidentical messages heard simultaneously, one by each ear, or heard and seen simultaneously), one or more of the inputs could be held temporarily in a short-term store for a few seconds while the most dominant message was being processed by the subject. Unfortunately, this model is insufficient to explain why Broca's aphasics should perform above chance on Example (5a), "The boy who pushed the girl was tall" but only at chance on Example (5b), "The boy who the girl pushed was tall." In both examples, exactly four words intervene between the words "the boy" and the words "was tall"; if forgetting of "the boy" was caused by a failure of temporary short-term storage of "the boy" while the patient was processing "who pushed the girl" (5a) or "who the girl pushed" (5b), both examples should have yielded chance performance.

However, this dilemma arose from our assuming that the contents of temporary storage in the cases of both Examples (5a) and (5b) consisted of four words encoded phonetically. If the two examples were encoded in such a way that the contents of temporary storage were not identical, we might come closer to an explanation of why Broca's aphasics performed differently on the two examples. I shall suggest here that in Example (5a) the words "the boy who pushed the girl" are encoded by subjects as one visuospatial event, namely, a visualization of a boy's pushing a girl. But in Example (5b) the words "the boy who the girl pushed" are encoded as two visuospatial events, namely: (1) a boy's continuing to exist (in a visual representation), while (2) a girl is pushing that boy, who is continuing to exist (in a visual representation). If there are interactions between the two visuospatially encoded memory representations in Example (5b) that are absent from Example (5a), which consists of a single visuospatially encoded memory representation, then the dilemma might be resolved.

The 1958 version of Broadbent's model had stressed that there might be a decaylike automatic forgetting of the information being held temporarily in the short-term store. The model said little about how new information coming into that store might destroy or erase older information already in that store. Experimental evidence (obtained by Waugh & Norman, 1965, among others) led to Broadbent's model's being adjusted by others to include forgetting in store determined by the contents of the store themselves. Adding new contents could lead to a "reduction of trace strength" of items already in store (Wickelgren & Norman 1966) or to their being "knocked out" of the store (Atkinson & Shiffrin 1968).

But it was Broadbent himself who, in a later article, suggested that the word "overwriting" best described the process whereby an item X that was being held in short-term store was rendered difficult to retrieve if an item Y, similar or even identical in sensory content to X, entered the store a few seconds later (Broadbent & Broadbent 1981). These authors carried out a short-term recognition task in which a sequence of seven drawings was displayed, each drawing consisting of an assemblage of three nonsense shapes. Participants had to judge whether an eighth drawing was old or new with respect to that sequence. Data obtained by Broadbent and Broadbent suggested that, if a particular nonsense shape Y occurred twice in the sequence, the later occurrence of Y "overwrote" the memory trace of the earlier occurrence of Y.

Nairne (1990) then extended the notion of overwriting to the auditory modality, and Neath and Nairne (1995) were able to develop this idea into a model that could account for such well-known phenomena as the forgetting of letter sequences because the letters rhymed with each other (for example, GTBVP is harder to recall than is GFQKR), and the disrupting effects on memorization caused by concurrent articulatory suppression, where the participant has to say not-to-be-memorized sounds aloud while seeing or hearing to-be-memorized letter sequences (Penney 1989). In particular, Neath and Nairne showed that, in short-term memory tasks using alphanumeric material, letter or digit sequences presented to the auditory sense usually had a strong advantage over material presented to the visual sense insofar as the auditory material yielded serial recall performance superior in accuracy to that associated with the visual material.

More recently still, Murray et al. (1999), studying short-term recognition for digit triples such as 384 or 792, demonstrated that the auditory superiority effect obtained in their investigation arose mainly because auditorially presented digit triples were easier to encode phonetically as unified groups (Gestalten) than were visually presented digit triples. Murray et al. (1998) were able to make this observation the starting point for the development of a theory of short-term forgetting they called "partial matching theory." This theory included an overwriting assumption, namely, that if a triple such as 384 had appeared early in a to-be-remembered sequence, and a triple such as 367 had appeared later in that same sequence, and if 384 had been only partially encoded as 3–, then the triple 367 could overwrite the partial triple 3– because the two triples shared the same initial digit, 3. This would mean that the short-term memory trace representing the triple 384 had been effectively deleted.

We now apply partial matching theory to the comprehension errors associated with Broca's aphasia with respect to Examples (5a), (5b), and (6), assuming visuospatial encoding throughout.

In Example (5a), we suggested that the words "the boy who pushed the girl" is encoded by the subject as a single (well-grouped) visuospatial event of which the boy is one component. Hence, there is little phonetic or visuospatial overwriting of the words "the boy" by the words "who pushed the girl," any more than there is overwriting of the 3 by the 8 and the 4 in a well-grouped trace of the triple 384.

In Example (5b), the words "who the girl pushed" were hypothesized to be represented by two visuospatial events, a boy's continuing to exist, and a girl's pushing that boy who continues to exist. Because "the boy's continuing to exist" comprises all of the visuospatial mental representation of the first event and most of the visuospatial mental representation of the second event, the trace of the first "the boy's continuing to exist" event is overwritten by "the boy's continuing to exist" component of the second event. This claim is made by analogy with the way in which 3– can be overwritten by 367, where 3– is a partial encoding of 384, and

384 entered short-term store a few seconds before 367. Hence, for a patient with Broca's aphasia, Example (5b):

The boy who the girl pushed was tall.

would be encoded in memory, following overwriting, as:

——— the girl pushed was tall.

In response to the question "who was pushed?" such a patient would have to guess and therefore choose a picture of a boy with the same probability as a picture of a girl (that is, the patient would respond at chance level).

In Example (6), the mode of argument applied to Example (5b) yields exactly the same prediction, namely, chance performance. But Grodzinsky has pointed out that the words "the boy" plays a role in Example (6) of Experiencer rather than of Agent. We begin by letting Example (6) be represented by two events that parallel those used in Example (5b), namely, (1) the girl's continuing to exist, and (2) the boy's admiring the girl who is continuing to exist. As in Example (5b), (2) overwrites (1), leaving a visuospatial representation of "a boy's admiring." But a psychological verb like "admiring" has as an object a content that is inseparably associated with the admiring itself; on the other hand, a movement verb like "pushing" has as an object another object that is easily dissociable from the pushing itself, especially in a mental visuospatial image. It is suggested that, when "the girl" is no longer present in the visuospatial representation of (Example (6), the concept of "admiring" is also absent from that representation because of the inseparability of "the girl" from "the admiring." This had not been the case in Example (5b); when "the girl" was no longer present in the visuospatial representation of Example (5b), the concept of "pushing" still remained in that representation because of the easy separability of "the girl" from "the pushing."

For Broca's aphasics, then, the final representation of Example (6) is just:

"---- the boy"

When asked "who was admired?", such patients would erroneously choose a picture of a boy because all that they retain of Example (6) is "the boy."

Grodzinsky maintains that a trace in the context of linguistic utterances is "a phonetically silent, yet syntactically active category" (sect. 2.2, para. 1). The above remarks are consistent with Grodzinsky's assertion.

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Agent-assignment, tree-pruning, and Broca's aphasia

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Abstract: I wholeheartedly endorse Grodzinsky's program of attempting to tie the particular deficits observed in Broca's aphasics' comprehension and production to changes in their mentally represented model of grammar. At the level of detail, however, I see problems with two specific changes that Grodzinsky posits. One is a default Agent-assignment strategy in comprehension. The other is the hypothesis that production involves pruning all functional projections above Agreement Phrase.

The correct, that is, psychologically real, theory of grammar is severely underdetermined by the standard means of collecting linguistic data, namely, judgments of acceptability by native-speaker linguists. As a result, frameworks for grammatical analysis proliferate, each providing descriptive generalizations that account for the data in question. Many grammarians would agree that this problem can be resolved only by expanding the sort of data that might bear on the correct theory. For this reason, papers like the present target article are very welcome. Grodzinsky argues that comprehension by Broca's aphasics is impaired as a result of the deletion of traces from their syntactic representations, accompanied by their having recourse to a default Agent-assigning strategy. Furthermore, their production is impaired by the loss ("pruning") of levels of phrase structure higher than that of the Agreement and negation Phrases. If he is right, then recent models of grammar in the principles-and-parameters tradition receive support over more surface-oriented lexicalist models. It is only in the former that all movements are posited to leave traces and, again, only in the former that a hierarchy of functional projections is posited that is rich enough to derive the presumed consequences for production.

I am not convinced, however, that the data cited in the target article allow these conclusions to be drawn with any degree of certainty. Part of my skepticism arises from the suggested comprehension strategy that "always assigns an Agent label to clause-initial NPs" (sect. 2.2), leading in some cases to two Agents within the same simple clause. But because we are told that Broca's aphasics are able to detect violations of argument structure (sect. 2.1), "never violate constraints on thematic structure" (sect. 2.1), and "never violate the theta-criterion" (Grodzinsky 1995a, p. 32; see also Lapointe 1985), their grammars should prohibit a double Agent-assignment in a sentence like Example 4b (sect. 2.2). Perhaps the following is what is going on instead. The deletion of all traces results in their not knowing that the sentence is a passive. They would therefore have no reason to consider an analysis in which the surface subject is a Theme. This sentence, however, does have two potential Agents: the subject of *push* and the object of *by.* Chance performance arises from their sometimes opting for one analysis and sometimes for the other. Hence it is not necessary to assume that they have a single representation of this sentence with two Agents.

Psychological verbs such as *admire* do not take Agents at all. Hence it seems unlikely that Broca's aphasics would posit one in sentences like Example 6 (sect. 2.2). I offer the following as perhaps a more plausible account. Because the aphasics do not know that the sentence is a passive and because *admire* does not take a Theme subject, they reject the correct analysis, leading them to guess incorrectly that the subject is an Experiencer. Such an analysis is more faithful to Grodzinsky's statement that "they reversed ϑ-roles roles systematically" than is his own analysis, in which *the boy* is labeled "Experiencer" in both the active and the passive.

A host of analytical problems arise with (5a) and (5b). The head of the trace in these sentences is actually *who,* not *the boy,* though Grodzinsky portrays the latter as the head in his derivations. The NP *the boy* is the subject of *was tall* and is therefore a Theme. (It is generally assumed that the head NP of a relative clause is linked to the relative by a rule of predication, not by trace binding.) So in (5a) if the Agent-assigning strategy were to turn *the boy* into an Agent (implausibly, as noted above) and did the same to *who,* given that it is the initial NP in the embedded clause, then it, too, should become an Agent. But that should not impair comprehension, because the Agents are associated with different predicates. So above-chance performance would be predicted, though not for the reasons put forward by Grodzinsky. On the other hand, perhaps "clause-initial NP" is meant to be restricted to those in the subject position or to those in matrix clauses, leading to no thematic assignment to *who* at all. The various interpretations of the strategy thus need to be clarified. The lack of clarity is more acute in (5b). Chance interpretation cannot be the result of *the boy* and *the girl* both being assigned the Agent role, because they belong to different clauses. Rather, given Grodzinsky's assumptions, it would have to be a consequence of *who* and *the girl* both being Agents. Perhaps he did not adopt this alternative because, having exempted the question word *who* from the Agent-assigning strategy on the basis of its being a nonreferential quantifier (Grodzinsky 1995a; see also the target article, sect. 2.6), he is opting for the same treatment of the relative pronoun *who.* In any event, unclarities abound in the treatment of these sentences.

The Tree-Pruning Hypothesis (sect. 2.7.4) is also far from problem-free. Let us begin with the question of subjects. Grodzinsky appears to be assuming the original version of the "Split INFL" hypothesis (Pollock 1989), in which the subject raises to Specifier of TP. We are told that aphasic production involves "subject omissions" (sect. 2.7.3), but also that "the speech output of Broca's aphasics contains verbs . . . located in their proper position in the sentence – always after the subject" (sect. 2.7.4). Now which situation is the Tree-Pruning Hypothesis designed to explain – the former with omitted subjects or the latter with subjects present? It is not clear. I would have guessed the former, because the pruned TP would deprive the subject of its final landing site. But Grodzinsky seems to be assuming the latter, because the Hypothesis is invoked to explain subject-bare verb ordering. I do not understand how, given "minimalist" assumptions, an uncrashed derivation can arise if the subject NP is prevented from raising to its final landing site.

An important question revolves around the inability of Broca's aphasics to produce sentences with complex embedding (sect. 2.7.3). Because pruning any CP would prune any sentence(s) dominating that CP as an automatic consequence, an odd prediction follows, namely, that only the most deeply embedded sentence could be produced. An aphasic who wanted to produce a complex sentence like, say, *I really doubt that Mary would be willing to give John a hand* would end up saying something like *Give John a hand.* Everything dominating that clause would have been pruned. Could that be right?

Finally, it is worth pointing out that Grodzinsky's analysis of the embedding hierarchy is incompatible with that in more recent "minimalist" models, in which AgrP is higher than TP, not lower (see, for example, Chomsky 1995a, p. 173). Such a circumstance would vitiate his predictions for aphasic production. Now that, in and of itself, is not a damning criticism of the target article. Perhaps the original proposed order of embedding was right (as is still maintained in Pollock 1997) and Grodzinsky has provided additional evidence why it *must* be right. Nevertheless, it is disconcerting to see majority opinion about clause structure move away from the view argued for on the basis of aphasic speech.

To conclude, at the level of theory, the target article is a welcome step forward. If only more neurolinguists and psycholinguists would follow Grodzinsky in his attempt to provide independent evidence confirming (or refuting) proposals that were put forward on the basis of introspective data. However, at the level of detail it does not fulfill its mission. Given what the reader is presented, the particular claims do not follow directly from the evidence.

Language, mathematics, and cerebral distinctness

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Abstract: The cerebral distinctness of the linguistic and mathematical faculties does not entail their functional independence. Approaches to language that posit a common foundation for the two make claims about design features, not location, and are thus not affected by the finding that one ability can be spared by a neurological accident that compromises the other.

Based on evidence for the "cerebral distinctness" of language and mathematics, Grodzinsky argues for the independence of the two capacities (sect. 5.2) – a conclusion that might be taken to undermine approaches to language that propose a common cognitive foundation for the two.

One such approach, categorial grammar (see Wood, 1993, for an introduction), has its roots in mathematics. One of its key claims is that various operations employed in mathematical logic (functional application, functional composition, and type-raising in particular) are directly reflected in the syntax of natural language (e.g., Steedman 1993, p. 227) and may even be primitive operations of cognition (Steedman 1993, p. 253).

It has also been suggested (e.g., O'Grady 1997, pp. 307 ff) that a basic architectural feature of syntactic representations – binary branching – is derived from a computational constraint shared with the mathematical faculty that forces combinatorial operations to apply to pairs of elements. Thus, we are no more able to combine the words *Harry, ran,* and *quickly* in a single step than we are to add simultaneously the numbers 5, 3, and 9. In each case we must proceed in a pair-wise fashion, a limitation that helps explain the existence of syntactic representations with a binary architecture (e.g., [Harry [ran quickly]]) in language.

If proposals such as these are right, then the language faculty and the mathematical faculty are alike in significant ways. Crucially, nothing in the results reported by Grodzinsky challenges this conclusion. This is because the types of claims that are at stake here pertain to the *composition* of the language faculty, not to its *location:* The key point is that the language faculty shares design features with the mathematical faculty, not that they are located in the same place. It is thus perfectly possible for one faculty to be compromised as the result of brain damage while the other functions normally, just as it is possible for one hand to be injured while the other is spared, despite its similar design.

Grodzinsky's findings do, however, provide a challenge of a different sort, namely, that of explaining why and how two separate mental faculties happen to share important design features. It seems clear that we must rule out at least one explanation (if it ever was entertained): The similarity cannot be attributed to a common location in the brain.

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Scrambling, indirect passives, and *wanna* contraction

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Abstract: Grodzinsky's general approach to the neuroscience of language is interesting, but the evidence currently available has problems with pragmatic infelicity in experiments involving Japanese scrambling and the interpretation of experimental results on Japanese indirect passives. I will suggest a more direct way of testing the Trace-Deletion Hypothesis (TDH).

I am in full agreement with Grodzinsky's claim that "[l]inguistic theory is the best tool currently available" (sect. 6) for investigating the language centers in the brain. I cannot imagine any serious neuroscientific study of language without a consideration of the vast recent advances in linguistic theory. This commentary will concentrate on some specific linguistic points made by Grodzinsky.

1. Scrambling in Japanese. Grodzinsky refers to Hagiwara (1993) in his discussion of the crosslinguistic validity of the Trace-Deletion Hypothesis (TDH). One of Hagiwara's experimental findings is that although Japanese-speaking Broca's aphasics can handle SOV sentences with canonical word order such as (1a) (=Grodzinsky's 7a), they cannot handle OSV sentences with "scrambled" word order such as (1b) (=Grodzinsky's 7b).

(1) a.
Taro-ga Hanako-o nagutta.
-NOM -ACC hit
Taro hit Hanako.

b.
Hanako$_i$-o Taro-ga t_i nagutta.

The same kinds of findings have been reported in the Japanese acquisition literature (e.g., Hayashibe 1975; Sano 1977); it had long been thought that these reflect properties of young children's grammar. However, it was shown by Otsu (1993) that children's failure to handle scrambled word order in earlier experiments is caused by pragmatic infelicity rather than children's grammatical incompetence. Specifically, the use of scrambled sentences is pragmatically acceptable only when the scrambled element, for example, *Hanako* in (1b), has already been established as a discourse topic. To test this, Otsu conducted experiments using the methodology of previous studies and simply added a contextual sentence before each test sentence, thereby establishing the scrambled element as the discourse topic. The result was quite straightforward. Whereas many 3- and 4-year-olds in the control group who were given scrambled sentences without a discourse context (as in previous experiments) failed to comprehend them, the children in the experimental group who were given scrambled sentences with the above-mentioned discourse context had virtually no difficulty understanding them.

Thus, although I agree with Grodzinsky and Hagiwara that we need an explanation for the failure of Broca's aphasic patients to comprehend scrambled sentences, the evidence currently available does not preclude the alternative pragmatic account. Notice that the same observation also applies to English and Japanese passives as discussed in the target article.

2. Indirect passives in Japanese. Turning to Japanese passives, Grodzinsky refers to Hagiwara's (1993), results of an experiment with direct passives, which she claims involve movement, and indirect passives, which she claims do not. The finding was that Broca's aphasic patients can comprehend indirect passives correctly, whereas they cannot comprehend direct passives.

Grodzinsky attempts to account for this difference with TDH. Patients are not able to comprehend direct passives because these involve movement (and hence correct comprehension must involve the trace). On the other hand, Grodzinsky claims that indirect passives are comprehensible to patients because they do not involve movement. The latter analysis is incorrect, however, if we adopt the VP-internal subject hypothesis as Grodzinsky does in the target article (sect. 2.2). Thus, in (2) (=Grodzinsky's 8b), *okaasan* is initially located within the VP, and later moved outside of the VP (to the subject position).

(2)
Okaasan-ga musuko-ni kaze-o hik-are-ta
mother-NOM a son-by a cold-ACC catch-PASS-PAST
Mother had (her) son catch a cold on her

The nonlinguistic linear-default strategy that assigns the Agent role to traceless clause-initial NPs (sect. 2.2) does not account for the patients' apparent success, because the clause-initial NP in this case (e.g., *okaasan* in 1) bears the Experiencer role, not the Agent role.

Notice also that in indirect passives in Japanese the surface subject is almost always adversely affected by the event denoted by the sentence. For this reason they are sometimes called "adversative" passives. It is not clear from Hagiwara's experiment, which uses a picture identification task, whether Broca's aphasic patients are able to grasp this semantic aspect of indirect passives.

3. More direct testing of TDH. Finally, I would like to suggest that there is a more direct test for TDH than the various experiments referred to in the target article – namely, a test involving the *wanna* contraction in English. It is well known that (3) is ungrammatical if it is syntactically related to (4); it is grammatical if it is related to (5). The standard explanation for this is that in (4) the trace that intervenes between *want* and *to* blocks the contraction just as the lexical NP *John* intervening between *want* and *to* blocks the *wanna*-contraction in (6; cf. (7). Notice that the trace in (5), in contrast, does not intervene between the two words.

(3) Who do you wanna visit?
(4) Who$_i$ do you want ti visit?
(5) Who$_i$ do you want to visit t$_i$?
(6) You want John to visit.
(7) *You wanna John visit.

TDH, if correct, predicts that Broca's aphasic patients are not able to make these judgments. It would be worthwhile to test this prediction.

Unpruned trees in German Broca's aphasia

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Abstract: Grodzinsky proposes that agrammatism leads to a "pruning" of the syntactic tree in speech production. For German, this assumption predicts that syntactic processes related to functional projections AgrP and CP should be impaired. An analysis of spontaneous-speech data from four Broca's aphasics with respect to subject-verb agreement and verb placement, however, indicates that phrase-structure representations in agrammatism are intact.

Grodzinsky advocates an account of agrammatic speech production referred to as the Tree-Pruning Hypothesis (Friedmann & Grodzinsky 1997), according to which syntactic structures in agrammatic aphasics are "pruned" at the tense node. This account implies that all those syntactic operations that necessarily rely on the functional projection TP and up can no longer be performed. In the following, I will summarize results from an investigation of speech-production data from four Broca's aphasics with agrammatic speech production who were diagnosed by the standard Aachen aphasia test-battery (Huber et al. 1983). The results will show that this prediction is not borne out for German agrammatism (Penke 1998).

For German, Figure 1 shows the phrase-structure representation that is assumed in standard GB-analyses (cf. Haegeman 1991; 1994). In main clauses, the verb starts out in the VP-final V^0-position and then successively moves over the tense node (T^0) and the agreement node (Agr^0) – which ensures correct subject-verb agreement – to the COMP-position. Note that in contrast to Hebrew, the Agr^0-node in German is placed above T^0, reflecting affix-order in German verbs. The subject has to move to SpecAgrP

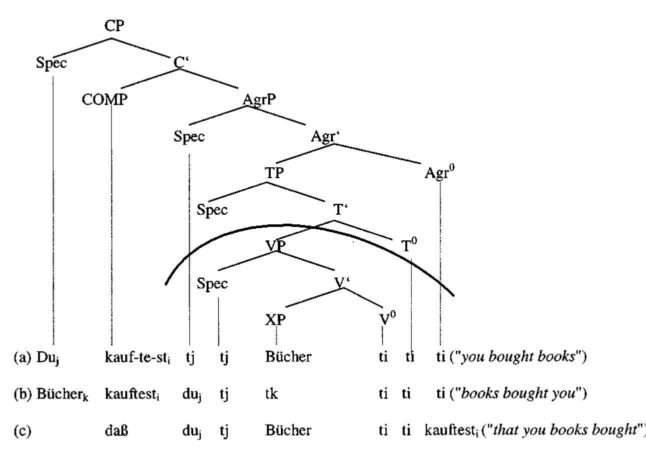

Figure 1 (Penke). Phrase-structure representation for German. Arch represents assumed site of agrammatic deficit.

to enter into an agreement relation with the verb. In addition, the subject or any other constituent has to move to SpecCP, giving rise to SVX- (see a), or XVS-word order (see b) in main clauses. In subordinate clauses, the COMP-position is filled with a lexical complementizer. Therefore, the finite verb cannot move to COMP but stays in the clause-final Agr^0-position. This explains the sentence-final position of finite verbs in subordinate clauses (see c).

Under the assumptions sketched above, pruning of German phrase structure at the T^0-node would cause the loss of the functional projections T^0, Agr^0, and COMP. Therefore, grammatical processes that depend on these functional categories should be impaired in German Broca's aphasics.

To ensure agreement between subject and verb, both constituents have to move to AgrP. A loss of AgrP would result in the inability to mark subject-verb agreement systematically. However, of the 914 inflected verbs produced by the 4 agrammatic subjects analyzed in Penke (1998), 811 (89%) were correctly marked for subject-verb agreement (correctness values for the 4 subjects: 79%, 92%, 94%, and 94%). These correctness values are significantly better than those that would be expected if the five German agreement affixes were applied randomly. They show that subject-verb agreement is systematically marked in German agrammatism.

As German is a V2-language, the finite verb has to move to the COMP-position in main clauses. If the syntactic tree is "pruned" the finite verb will fail to raise and will show up in sentence-final position. However, a look at the verb-placement data in main clauses for the 4 subjects reveals that of 615 verbs with correct subject-verb agreement and for which verb placement could be unambiguously determined, 607 (99%) were correctly placed in V2-position (range 96% to 100%). Thus, in accordance with the target grammar, finite verbs show V2-placement in main clauses in agrammatism. Further evidence for the preservation of the CP-layer in agrammatism is provided by data on verb placement in subordinate clauses. In contrast to main clauses, the finite verb cannot raise to COMP in subordinate clauses because the complementizer is base-generated in COMP. Therefore, the finite verb remains clause-finally in the Agr^0-position. The speech production data of the 4 subjects contained 96 subordinate clauses with a finite verb. In 95 of these clauses, the finite verb was correctly placed clause-finally. Moreover, subordinate clauses were regularly introduced by a complementizer that is base-generated in COMP (only 2 of the 103 relevant subordinate clauses lacked a complementizer).

In summary, (1) the high correctness values for subject-verb agreement, (2) the preservation of verb-placement patterns in main and subordinate clauses, and (3) the regular occurrence of complementizers in subordinate clauses suggest that both the Agr^0-projection and the COMP-projection are left intact in German agrammatism.

Further cross-linguistic evidence for the preservation of phrase-structure representations in agrammatism comes from studies on verb movement in Dutch, Italian, and French agrammatic aphasics. Kolk and Heeschen (1992) analyzed verb-placement patterns in short interviews conducted with eight Dutch Broca's aphasics. They report that 99% of the produced finite verbs were correctly placed in COMP (V2). Lonzi and Luzzatti (1993) studied spontaneous speech data of Italian and French agrammatic aphasics. Both in French and in Italian, the Agr^0-node is placed above the tense-node, mirroring the order of verbal affixes (cf. Haegeman 1994). A "pruning" at T^0 would render verb movement out of VP impossible. Note that movement of finite verbs in French and Italian can be traced by the relative order of verbs and adverbs: A verb preceding an adverb that is at the left boundary of VP has moved to T^0 or Agr^0. Of the 50 relevant utterances in their data sample, finite verbs were correctly moved out of VP in 49 cases, which indicates that at least the functional projection TP is still intact in agrammatism. The data on German, Dutch, French, and Italian aphasics accordingly imply that functional projections from T^0 up are present in agrammatic speech production and provide evidence against Grodzinsky's Tree-Pruning Hypothesis.

No evidence for traces in sentence comprehension

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Abstract: Grodzinsky claims that "normal language users demonstrate trace-antecedent relations in real-time tasks." However, the cited evidence is equally compatible with a traceless account of processing. Moreover, Pickering and Barry (1991) and Traxler and Pickering (1996) have demonstrated that the processor does not wait until the purported trace location before forming the dependency. Grodzinsky's claims about Broca's area should be interpreted in terms of a transformation-free account.

Grodzinsky claims that "normal language users demonstrate trace-antecedent relations in real-time tasks." (sect. 2.4.2), and cites experiments that suggest that antecedents are reactivated at the point in the sentence where the trace occurs in transformational accounts (e.g., Nicol & Swinney 1989). However, Pickering and Barry (1991) and Pickering (1993) have pointed out that the purported trace is adjacent to the verb in such experiments, and hence that the results are equally compatible with a "direct association" between antecedent and verb. They argued that these accounts can be distinguished by extracting an element that is not adjacent to the verb, such as the second post-verbal argument of a ditransitive (e.g., *on the saucer* in *Mary put the cup on the saucer*). If *on the saucer* is extracted, then trace-based accounts assume that its trace occurs after *cup.* It is therefore possible to separate the verb and the trace by an arbitrarily large amount of material. Most interestingly, it is possible to construct sentences that involve multiple extractions, as in (1):

(1) John found the saucer [on which]$_i$ Mary put the cup [into which]$_j$ I poured the tea t_j t_i.

In this sentence, the trace-based account assumes nested antecedent-trace dependencies. Such nested dependencies should cause great processing difficulty (e.g., Chomsky 1965), but in fact do not (and including further extractions does not make the sentence impossible to process). In contrast, the trace-free account predicts disjoint dependencies, where one dependency is formed before the antecedent for the next occurs:

(2) John found the saucer [on which]$_i$ Mary [put]$_i$ the cup [into which]$_j$ I [poured]$_j$ the tea.

Clearly, this account accords with the lack of processing difficulty with this sentence.

Experimental evidence provides stronger support for this account. Traxler and Pickering (1996) monitored eye movements while participants read implausible sentences like (2), along with plausible control sentences:

(3) That's the pistol in which the heartless killer shot the hapless man yesterday afternoon.

All accounts predict difficulty when readers discover that a sentence is implausible. According to the trace-based account, this should occur at the trace-location, after *man.* But if traces do not mediate the processing of such sentences, the association should occur as soon as the verb *shot* is read. The data supported this account: Processing difficulty, measured in terms of first-pass reading time, occurred at *shot* (and therefore well before the trace location).

Hence, comprehenders do not understand sentences containing unbounded dependencies by applying procedures that can be associated with transformations or traces (see also Boland et al. 1995). Work on syntactic priming in language production also provides evidence against the psychological reality of transformations (Bock & Loebell 1990; Bock et al. 1992; see Pickering & Branigan 1999). The implication is that the grammatical component of the language processor is best described by a theory that eschews the use of transformations, for example, versions of Head-Driven Phrase Structure Grammar (Pollard & Sag 1993, Ch. 9) and Lexical-Functional Grammar, Kaplan & Zaenen 1988.

To accommodate these data, Grodzinsky needs to reformulate his account so that the linguistic operations that take place in Broca's area are compatible with a trace-free account of syntax. This may be possible. He claims (sect. 2.3.2) that "the performance of Broca's aphasics is predicted only by the location of the trace and its interaction with the strategy [of assigning an agent label to clause-initial NPs]." But this is not correct – after the trace is deleted, according to his account, its location is necessarily irrelevant. So any account that eschews transformations and traces but treats passives, unbounded dependencies, and so on, as a class may be able to account for the data. In my opinion, it would be necessary to obtain a much wider range of data, from a large number of languages, to see what constructions make up this class that Grodzinsky argues are dealt with in Broca's area.

On the proper generalization for Broca's aphasia comprehension pattern: Why argument movement may not be at the source of the Broca's deficit

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Abstract: The comprehension problem in Broca's patients does not stem from an inability to represent argument traces. There can be good comprehension in the presence of (object) traces and impaired comprehension can result in constructions where there are no (object) argument traces. This leads to an alternative understanding of Broca's comprehension, one that places the locus of the impairment in an inability to construct syntactic representation on time.

The Trace Deletion Hypothesis (TDH) represents the right kind of generalization because (a) it tells us that the damage in Broca's patients is minimal and this makes lesion studies relevant for the study of brain-language relations; and (b) it allows an understanding of brain-language relations where functional localization is governed by independently motivated theoretical models (i.e., linguistic theory). Building on these two important points I argue that there is a better linguistically based generalization that does not require traces and for which there is clear empirical evidence.

Grodzinsky discusses evidence from off-line comprehension from several sentential contrasts that vary with respect to the presence of argument movement. Presence of movement, which creates an argument trace, is predicted by the TDH to create problems in Broca's comprehension. However, and as Grodzinsky himself notes, not all types of argument displacement result in *chance* performance (sect. 2.2). This is what motivates the incorporation of an extra-linguistic element in the generalization: the *agent*-first strategy. So, there is argument movement in both subject relatives (*The girl$_i$ who$_i$ t$_i$ pushed the boy is smart*) and object relatives (*The boy$_i$ who$_i$ the girl pushed t$_i$ is big*); however, chance performance results only for object relatives. The same goes for actives (*The girl pushed the boy*) and passives (*The boy$_i$ was pushed t$_i$ by the girl*) and for subject (*It was the girl$_i$ who$_i$ t$_i$ pushed the boy*) and object clefts (*It was the boy$_i$ who$_i$ the girl pushed t$_i$*), where chance performance occurs only for object clefts and passives, even though all four constructions are taken to involve some kind of argument displacement.

As it happens, in the set of constructions described above, whenever chance performance is observed (object relatives/object clefts and passives), argument displacement takes place in the form of movement from object position: NP$_i$ . . . . V t$_i$. Moreover, this kind of syntactic displacement has the consequence of reversing the canonical order of thematic roles licensed by the verb: When performance is reported as above-chance either the agent or the experiencer argument (as the case may be) precedes the theme argument in surface representation of the given constructions. In constructions where performance is reported as chance the reverse is true: The theme argument always precedes the agent/experiencer argument in syntactic representation. This holds for all the constructions Grodzinsky reports, including the evidence from Chinese and Japanese. It suggests that even though the evidence for Broca's comprehension allows a movement-based characterization, it does not distinguish movement itself from one of its consequences – deviation from canonical order of thematic roles in surface representation.

One way to distinguish between these two views – one that appeals to argument displacement (i.e., object movement), and another that appeals to order of thematic roles in syntactic representation – is by showing (a) that object movement that does not reverse the order of thematic roles in syntactic representation does not cause unimpaired performance, and (b) that absence of (object) movement can result in impaired performance if the order of thematic roles in syntactic representation has been reversed.

Both cases have been reported. In Piñango (1999) I present evidence for the first case by capitalizing on a linguistic phenomenon known as unaccusativity (Levine & Rappaport-Hovav 1995). The unaccusativity hypothesis proposes that verbs of certain semantic classes base-generate their arguments in object position. This argument then moves to subject position, leaving behind an argument (object) trace. An example of an unaccusative verb is *spin,* which has both a transitive and an intransitive version: *The man*$_i$ *spun* t$_i$, versus *The woman spun the man.* Sentences such as *The man*$_i$ *spun* t$_i$ *because of the woman* were tested; these are analogous in syntax and meaning to the passive version *The man*$_i$ was *spun* t$_i$ *by the woman* (however, only in the passive does the verb license two arguments). Results show that whereas Broca's patients have problems with reversible passives, they performed at above-chance levels in sentences with the unaccusatives. However, in both types of sentence the linguistic analysis dictates that the correct interpretation can only be obtained through the representation of an antecedent-trace relation that, by the TDH, should result in impaired performance. Crucially, the main factor that differs in the two conditions is whether thematic roles have been reversed. In the unaccusatives, there is no reversal. The intransitive verb licenses only one argument: spin ⟨theme⟩.

Piñango (1998) presents evidence for (b), the second case, by testing psychological verbs of the *frighten* type. In the active construction, these verbs have the peculiarity of showing a reversal of thematic roles ⟨theme, experiencer⟩ as in the sentence: *the woman frightened the man.* Predictably, the verbs exhibit canonical order ⟨experiencer, theme⟩ in the passive: *The man$_t$ was frightened t$_t$ by the woman* (e.g., Pesetsky 1995). Results from testing four Broca's patients using a picture-matching task reveal an effect only for canonicity: chance for actives and above-chance for passives. This evidence, in addition to that presented by Grodzinsky, clearly supports a canonicity account over a movement account: Chance performance can result in the absence of movement as in *frighten*-actives, and above-chance performance can result in the presence of (object) movement actives with unaccusatives and *frighten*-passives.

The notion that canonicity in syntactic representation, not syntactic displacement, is central to the problem of Broca's comprehension is, of course, not new. It can be found throughout the literature in different guises (e.g., Caplan & Futter 1986; Caramazza & Zurif 1976; Grodzinsky [this issue]; Hagiwara & Caplan 1990; Linebarger et al. 1983). However, those proposals differ from the present instantiation of that insight, in that they consistently attribute sensitivity to canonical order to a heuristic, and in doing so, they fail to see the import that such regularity has for the linguistic system. That is, they fail to see that rather than being just a reflection of extra-linguistic knowledge, sensitivity to canonical order of thematic roles by Broca's patients is a reflection of a preserved linguistic construct.

That linguistic construct is argument structure. For present purposes, argument structure can be defined as a level of semantic representation where the thematic roles licensed by a predicate in a sentential structure are expressed. It constitutes the default principle of linking between semantic representation and syntactic structure. This linking principle reflects an organization of thematic roles that is based on semantic priority observed across languages of the world via linear order in syntactic representation: agent and experiencer arguments precede patients and recipients (e.g., Bresnan & Kanerva 1989; Jackendoff 1990; Piñango 1998).

The question to ask now is: If Broca's patients are sensitive to canonical order of arguments, why do they not perform below-chance in sentences that violate this order? The answer to this question lies in the observation that these patients show a slowness in speed of lexical activation so that they are unable to build syntactic structure quickly enough to prevent semantic linking from emerging (e.g., Piñango 1999; Prather et al. 1997). Nevertheless, once their system finishes building the representation, including forming all antecedent-trace relations, a problem arises if the product of the default linking is in conflict with the product of syntactic linking. This happens precisely in the cases where syntactic representation violates canonical order of arguments. Chance performance results because in these constructions (e.g., agentive passive in English), sometimes semantic linking will prevail, and sometimes syntactic linking will. It is the competition between these two possible systems of correspondence that results in chance performance (Piñango 1999).

This way of understanding Broca's comprehension maintains that the impairment is minimal, and a processing, not a knowledge limitation. It is also more forgiving of specific linguistic theoretical assumptions, because not all theoretical approaches invoke argument movement as a way to deal with syntactic displacement, as it capitalizes on relations (argument structure-syntax correspondence) for which all linguistic frameworks must account.

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From Broca's aphasia to the language module: A transformation too large?

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Abstract: This commentary focuses on the larger implications of Grodzinsky's hypothesis. Although Grodzinsky argues persuasively that the syntactic comprehension deficits in Broca's aphasia involve mainly an inability to comprehend sentences requiring a transformational movement of phrasal constituents, his larger claim for a distinct and dedicated "language organ" in the left hemisphere is much less tenable.

Grodzinsky has written a thorough and insightful review of the syntactic comprehension deficit in Broca's aphasia, emphasizing the cross-cultural linguistic literature, as well as functional imaging data and other sources. He has put forth a fairly convincing argument that the syntactic processing deficits in Broca's disorders are real but confined mainly to transformational syntactic movements. Grodzinsky provides much less evidence for the intriguing notion that a selective syntactic production deficit exists in Broca's involving only tense construction, and the lack of parallelism between the comprehension and production deficits is obviously troubling.

To ascribe to Broca's aphasics a relatively specific syntactic deficit does not, however, directly translate into proof of a "distinct and dedicated 'language organ'" (sect. 6, para. 1) – that is, a language "module." Indeed, on-line phrasal transformations would seem impossible without the general multiplexing capability provided by working memory, which has been convincingly shown to play a role in sentence comprehension. Parsing of sentences is related to memory constraints (Abney & Johnson 1991), and sentence comprehension ability in normals correlates very well with working memory capacity (Daneman & Merikle 1996). As Grodzinsky acknowledges (sect. 2.4.2), short-term verbal memory tasks activate the inferior frontal convexity (Smith & Jonides 1997), which may explain why Broca's aphasics generally have more problems with *longer* sentences when they deviate from the active (Berndt 1997). Grodzinsky's hypothesis also fails to account for the finding of Grossman (1980) that Broca's aphasics have a general problem of hierarchical memory organization that is not limited to language (see also Greenfield 1991).

The fact that Broca's aphasics may retain mathematical skills, nonsyntactic linguistic abilities, and an overall high degree of intellectual functioning cannot be used to infer the existence of a distinct, left-hemispheric syntax module. For example, mathematical skills are more dependent than speech production and comprehension on visual and even tactile skills. Indeed, eye movement biases are more likely to occur during a mental arithmetic task than during a sentence comprehension task (Previc & Murphy 1997), which indicates the greater use of visual imaging in the former situation. Moreover, the tendency for young children to learn to count using their fingers (usually of their dominant right hand) helps to explain the linkage of finger agnosia with acalculia in the left-parietal Gerstmann syndrome (Benton 1992) and the generally greater posterior left-hemispheric involvement in mathematical skills (Boller & Grafman 1983). The fact that other linguistic processes are not as dependent as syntax on the left anterior frontal lobe is also easily understood in that they may be more closely tied to visual object recognition (e.g., naming) and housed posteriorly, or tied to emotional responses (e.g., proverb interpretation), and therefore more dependent on the right hemisphere (Bryan 1988). Were we to sing our sentences, grammatical constructions would presumably be better understood by the right hemisphere; indeed, recovery of sentence comprehension in aphasia can be aided by adding melodic intonation to sentences (Albert et al. 1973). Finally, how can general intellectual functioning – which is usually measured visually, includes a large amount of nonverbal visuospatial tasks, and is only modestly affected even by large lesions of the prefrontal cortex (Hebb 1939) – be seriously degraded by damage only to Broca's area, which essentially represents only orofacial association cortex on the left side?

In searching for the origins of the language module, one also faces great obstacles. Although Grodzinsky does not explicitly argue that the language module is hard-wired, such a claim is implicit if one claims that language is not built on or derived from more basic mental functions that may also be at least partly housed in Broca's area. Yet language largely survives a drastic reduction of the cerebral cortical mantle (as in hydrocephalus) and even elimination of its favored hemisphere (as in left-hemispherectomy), *if these insults are sustained early in life* (Bishop 1988; LeBeer 1998; Ogden 1988). This is because primordial language processing enters largely virgin brain tissue during development (e.g., the unique myeloarchitecture of Broca's area develops only after the first year of life – Simonds & Scheibel 1989) and requires various cultural experiences to be fully lateralized (e.g., illiterates rarely become severely aphasic following left-sided lesions – Lecours et al. 1988). Even following damage in adulthood, recovery from aphasia occurs in a substantial percentage of cases, presumably because of the residual linguistic (including syntactic) capability of the right hemisphere (Papanicolaou et al. 1988), which is normally suppressed by the left hemisphere.

It would seem more appropriate to view Broca's area as the confluence of higher order orofacial control, auditory processing, working memory, and sequential programming and analysis. Why the speech area (as well as areas for other propositional mental activity) resides on the *left* side of the brain may have more to do with primordial auditory asymmetries and neurochemical lateralizations than forces specific to language per se (see Previc 1991; Previc, in press). The critical issue is: Can nonlinguistic tasks be designed to produce deficits in Broca's patients that resemble the transformational grammar failures that Grodzinsky notes? What if patients are presented with a pattern of high-frequency tones (in the speech range) and they have to reproduce them in one order if the final tone is identical to that of the previous one or reproduce them in a different order if the final tone is not? Or, what if patients are required to change the order of an arithmetic operation depending on the relationship between the final digit and the preceding one? (For example, the number sequence x, y, z would require the operation $[(x + y) * z]$ if $z > y$ and $[z + (x * y)]$ if $y > z$). Would Broca's aphasics have trouble with these tasks, which would involve auditory presentations in the speech range, working memory demands, serial transformations and dependencies, and hierarchical organization? If they would not, then perhaps Grodzinsky is justified in claiming Broca's area as a brain area dedicated exclusively to specific language functions. But, the existence of a separate language or syntax module cannot be claimed merely because Broca's aphasics do not regularly suffer from acalculia, prosodic or semantic language disturbances, or general intellectual loss.

Aphasia research and theoretical linguistics guiding each other

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Abstract: An elaboration on some loose ends in Grodzinsky's analysis shows that data from the field of aphasia contribute to the formulation of theoretical linguistic principles, and provides extra arguments in favor of Grodzinsky's claim that linguistic theory is the best tool for the investigation of aphasia. This illustrates and emphasizes the importance of communication between researchers in the field of (Broca's) aphasia and of theoretical linguistics.

Linguistic theory divides language into subcomponents, or modules, such as the lexicon and the computational system (grammar and the parser; cf. Chomsky 1993). The grammar, in turn, consists of phonology, morphology, semantics, and syntax. This modular distinction allows us to isolate and explain language deficits precisely and without confusion. As Grodzinsky argues, Broca's aphasics have a deficit in their syntax, namely, they delete traces, but they try to avoid this problem, sometimes successfully, by using other linguistic modules (such as semantics) or even general cognitive principles (such as linearity). This explains why Broca's aphasics do not perform poorly overall on syntactic texts. On the other hand, the investigation of language deficits in general, and Broca's aphasia in particular, can provide us with new insights in linguistic theory. Thus, communication between the two fields is crucial, and works both ways. In the following I will concentrate on two issues that illustrate how linguistic theory and research on Broca's aphasia benefit from each other. The first issue concerns subject theta-roles, the second, object theta-roles.

As Grodzinsky mentions in section 2.2, recent linguistic theory posits that subjects are base-generated in VP-internal position, and subsequently move to the specifier of IP to check Case and phi-features. If this is true, the Trace Deletion Hypothesis (TDH) predicts that even subjects in ordinary declarative active sentences leave traces, which are deleted by Broca's aphasics, with the consequence that the subject has no theta-role. However, as Grodzinsky states: "Such sentences pose no comprehension problems to the aphasics, however. Trace deletion, then, is an insufficient account of the data. Something must be done to remedy this." (sect. 2.2). I argue that the solution of this problem must be sought in an adaptation of linguistic theory, rather than in an adjustment of the TDH. The fact that Broca's aphasics display problems with respect to object traces and theta-roles, but not to subject traces and theta-roles in active sentences, might tell us that the VP-internal Subject Hypothesis is not valid. Additional evidence against the VP-internal Subject Hypothesis comes from recent work in the field of First Language Acquisition. Stromswold (1996) shows that several well-known child language phenomena, which have been explained by means of the VP-internal Subject Hypothesis, and are therefore used as support for this Hypothesis, can in fact be better analyzed in alternative ways. For example, Pierce (1989) and Deprez and Pierce (1993) claim that sentence-external negation in child language, such as *no I see truck* ("I do not see a truck"), result from the lack of subject-raising out of the VP (to spec AgrP), as is schematized in (1):

(1) $[_{CP} [_{Agrp} [_{NegP}$ no $[_{TP} [_{VP}$ I see truck]]]]]

However, Stromswold shows that Deprez and Pierce's percentages of early negation-initial sentences drastically decrease and stay constant throughout acquisition if negative sentences with null subjects are eliminated. The rationale for excluding null subject negatives is the fact that in these cases one cannot tell whether negation is to the left or to the right of the subject. This suggests that sentence-external negation in early grammar might not be a robust phenomenon at all. Thus, findings in the field of first language acquisition, as well as of Broca's aphasia, suggest that the VP-internal Subject Hypothesis should be adjusted or perhaps eliminated from linguistic theory altogether.

My second point concerns objects, their traces, and their theta-roles in Broca's aphasia. As Grodzinsky discusses, Japanese Broca's aphasics perform at chance on sentences in which the object has scrambled over the subject. This is in line with the strategy that Broca's aphasics assign a default Agent theta-role to the first argument in the sentence if this argument does not have a theta-role caused by trace-deletion. As a result, both the object and the subject bear an Agent theta-role and the patients guess which one is the Agent. How would this work regarding object scrambling in Dutch? In Dutch, the object can be scrambled over an adverb, or over negation, but crucially, not over the subject. Thus, object scrambling in Dutch renders the order subject-object-adverb/negation, as illustrated in (2):

(2) dat	Jan	het meisje$_i$	voorzichtig	t_i	kust
that	Jan	the girl	carefully		kisses
	S	O	adverb		V
	agent	???			

As the structure in (2) shows, the subject *Jan* is in its canonical position, receiving an Agent theta-role without any problems (modulo the problem regarding subjects discussed above). However, the object *het meisje* has been scrambled over the adverb *voorzichtig*, thus leaving a trace. This implies that Broca's aphasics are incapable of grammatically assigning a (Theme) theta role to *het meisje.* The question is whether, and if so, how this object receives a theta role. Grodzinsky's analysis does not explain such constructions. However, linguistic theory offers some guidance in the sense that it requires every argument to have a theta role (Theta-Criterion; Chomsky 1981). Two hypotheses come to mind. First, analogous to the strategy of Agent theta role assignment to the first argument (if it does not have a theta role yet), Broca's aphasics might employ a strategy that assigns a default Theme theta role to the second argument in the sentence if it has not received a theta role by grammatical means. If this is true, we predict that Broca's aphasics should not make any errors in the interpretation of Dutch sentences with scrambled objects. Alternatively, we could make a slight change in Grodzinsky's original formulation of the strategy and hypothesize that the first theta role-less argument in the sentence receives an Agent role. This predicts that Broca's aphasics will perform at chance on sentences such as in (2), because there will be two arguments with an Agent theta-role. We can test these hypotheses by showing the patient two pictures: one in which John is kissing a girl carefully and one in which a girl is kissing John carefully. Above-chance performance would provide support for our first hypothesis; chance performance would support the second one. The question raised above shows that an analysis such as proposed by Grodzinsky for the assignment of theta roles in Broca's aphasics is not complete (yet), but that linguistic theory can guide us in formulating testable hypotheses.

Sentence comprehension and the left inferior frontal gyrus: Storage, not computation

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Abstract: Neuroimaging evidence suggests that the left inferior frontal gyrus (LIFG) supports temporary storage of linguistic material during linguistic tasks rather than computing a syntactic representation. The LIFG is not activated by simple sentences but by complex sentences and maintenance of word lists. Under this hypothesis, agrammatism should only disturb comprehension for constructions in which storage is essential.

The neurolinguistic model of language developed in the 1970s suggests that syntactic processing occurs in the left inferior frontal gyrus (LIFG), whereas semantic processing occurs in the left posterior temporal lobe. Recent neuroimaging evidence suggests that this model is incorrect. First, processing simple sentences does not activate the LIFG relative to a passive fixation condition; activation is only seen for complex sentences (Stowe et al. 1994). This is very odd if syntactic processing occurs in the LIFG. Evidence from aphasia supports this conclusion, as well. Agrammatic aphasics recognize ungrammaticality and typically produce phrases that are locally grammatical, although they do not form complete sentences (Bastiaanse & van Zonneveld 1998). Agrammatics' syntactic deficits are thus more limited than would be expected if the "syntax" area had been significantly damaged.

Nevertheless, the LIFG is clearly active in sentence comprehension, because PET and fMRI studies have found activation here during the processing of more complex sentences containing incomplete structures (Caplan et al. 1998; 1999; Just et al. 1996; Stowe et al. 1994; 1998; Stromswold et al. 1996). The question is what function it carries out. Grodzinsky points out that the constructions that lead to the greatest difficulty for agrammatic aphasics contain syntactic dependencies between a moved XP and a trace (e.g., wh-questions, relative clauses, and passives). Grodzinsky hypothesizes that agrammatic aphasics have a deficit in a specific type of syntactic computation, the establishment of an XP/trace dependency.

However, Grodzinsky's reinterpretation of the LIFG's function does not mesh with other neuroimaging results. It predicts that the LIFG should be activated only by XP/trace dependencies. Stowe et al. (1998) showed that blood flow was lowest for simple sentences, increased for complex sentences, and was highest for syntactically ambiguous sentences that contained only as many XP/trace dependencies as the simple condition. Thus the LIFG does not appear to be limited to establishing XP/trace dependencies.

Another problem for both the standard view, that syntactic computation occurs in the frontal lobe, and Grodzinsky's reinterpretation is that reading word lists activates the LIFG more than simple sentences (Mazoyer et al. 1993; Stowe et al. 1998), although word lists do not invoke syntactic computation or contain XP/trace dependencies. The LIFT is also activated:

(1) when subjects memorize a list during the scan (e.g., Grasby et al. 1994);

(2) when subjects maintain a short list presented before the scan (e.g., Fiez et al. 1996);

(3) when subjects continuously update a short list for comparison with input (n-back task; e.g., Awh et al. 1996; Smith et al. 1996);

(4) when subjects recall or recognize words out of a short study list presented before the scan (e.g., Awh et al. 1996; Buckner et al. 1996; Paulesu et al. 1993; Smith et al. 1996).

In the set of studies just cited, the mean location of the maximal voxel within the activation is $x = -41$, $y = 14$, $z = 17$ in a stereotactic coordinate system (Talairach & Tournoux 1988; coordinates represent distance in mm. from the anterior commissure). This is comparable to the mean location for the syntactic complexity studies cited above ($x = -40$, $y = 18$, $z = 14$).

The overlap between activations for verbal storage and for sentential complexity seems unlikely to be accidental; it suggests that the LIFG also supports storage during sentence comprehension. However, the most important sort of information that needs to be stored during sentence comprehension concerns incomplete structure (Gibson 1998) and a purely lexical store is not likely to be useful. Stowe et al. (1998) discussed several hypotheses about the forms of information that may be stored in the LIFG. Our argument was that if phrasal information is stored as well as lexical information, blood flow in the LIFG should be predicted by the combination of phrasal memory load and lexical load associated with a condition. It appears that words are maintained until a phrase is created. Thus word lists have a low phrasal load and a high lexical load and should exhibit more blood flow than simple sentences (which have low loads for both), but less than complex sentences (which are high in both phrasal load and lexical load because of long, incomplete phrases). The LIFG exhibited this pattern. On the other hand, if only lexical items are temporarily stored in the LIFG, word lists would typically be associated with a higher load than even the most complex sentences. This pattern was not found.

Another possibility is that two functionally separate networks (lexical memory and phrasal memory or computation) are located in the same anatomical structure. Any hypothesis postulating separate networks predicts that syntactic complexity and an independent verbal memory load should be relatively independent. The single storage function hypothesis predicts an interaction: As storage demands increase during sentence processing, the amount of resources available for a verbal memory task should decrease. A PET study that investigated this prediction was reported by Stowe et al. (in press). We asked subjects to read one- and two-clause sentences while monitoring for words out of a list containing one or five words. We found a highly significant interaction between the two variables in the LIFG. Such a result is difficult for any hypothesis postulating separate networks to explain.

Taken together, these neuroimaging experiments support the hypothesis that a single cognitive function in the LIFG supports temporary storage of verbal information, including structural information, during sentence processing (Kaan & Stowe, forthcoming). This contradicts the hypothesis that the LIFG carries out (aspects of) syntactic computation, as proposed by both the classic model and Grodzinsky's reinterpretation of it. The alternative Storage Hypothesis is capable of explaining the agrammatic data presented by Grodzinsky, as well. The mechanism used for storage in comprehension is clearly related to that used for storing longer term production plans. Under the Storage Hypothesis, it is also not coincidental that XP/trace dependencies, the paradigmatic case of storage of unintegrated structural information, are problematic for these patients. The syntactic computation hypotheses, on the other hand, cannot readily explain the neuroimaging data summarized here.

Temporal perception: A key to understanding language

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Abstract: Although Grodzinsky's target article has merit, it neglects the importance of neural mechanisms underlying language functions. We present results from our clinical studies on different levels of temporal information processing in aphasic patients and briefly review the existing data on neurobiology of language to cast new light on the main thesis of the target article.

Grodzinsky's target article provides a fresh approach to the cerebral representation of language. He argues that most human linguistic abilities are not located in the Broca's area, which for more than a century was believed to be a major centre associated with lenguage production and syntactic processing. His hypothesis originates from a linguistic perspective only, however, and neglects neuronal processes. An important question arising from this article is: What neuronal mechanisms underlie the described functions?

A growing body of evidence suggests that temporal information processing controls many aspects of human behaviour, including language. Experimental studies using a variety of techniques and subject populations (e.g., Fitch et al. 1997) have consistently demonstrated that the superiority of the left hemisphere for the processing of verbal information may reflect a more primary specialisation for processing temporal cues, of which human speech is one example. Moreover, some language disorders in children and adults are associated with timing impairments.

On the basis of a hierarchical model of time perception (Pöppel 1994; 1997) we reinterpret the merits of this target article. Three different temporal ranges seem to be crucial for language, namely, about 2–3 sec, 200–300 msec, and 30–40 msec, corresponding respectively to the duration of phrases, syllables, and phonemes in fluent speech. In our clinical studies we found that temporal perception in aphasics is selectively affected at these three levels, depending on the localisation of lesion and existing disfluency patterns (van Steinbüchel et al. 1999).

The level of approximately 3 seconds was assessed by measuring temporal integration (TI). Experimental evidence (Pöppel 1978) suggests that sequences of events are automatically linked together into a perceptual gestalt. This binding process is presemantic (thus, independent of concrete events) and defines a "working platform" for mental activity. Using the subjective accentuation paradigm (Szelag 1997), we tested the extent of such TI in patients with precentral or postcentral lesions, either to the left hemisphere (resulting in nonfluent or fluent aphasia), or to the right hemisphere (without aphasia). While listening to metronome beats, patients were asked to accentuate mentally every x-th beat and create an individual rhythmic pattern. The extent of temporal integration was defined as the duration of the perceptual units comprised of such subjectively grouped beats. Broca's aphasics behaved differently from all other patient groups and acquired a new strategy because of the lesion, that is, they relied less on automatic TI and more on mental counting (Szelag et al. 1997). They had deficits in the binding operations that probably underlie not only the ability to construct full-fledged tree structures in production (i.e., effortful, nonfluent speech) but also their antecedents in comprehension, which need to integrate and hold the information for up to a few seconds. This hypothesis is supported by section 4 of the target article, which implies that some highly structured syntactic abilities are located in the anterior language area.

Grodzinsky also reports prominent failures on structures containing transformational operations and the deletion of all traces of movement from syntactic representation in Broca's aphasics (sects. 2.1 and 2.2). It also seems that timing disorders in the domain of about 200–300 msec, corresponding to syllable processing, may be crucial for these comprehensional deficits. We observed these disorders in self-paced (personal) finger tapping tasks. Patients with left hemisphere injury and Broca's or Wernicke's aphasia had significantly slower tapping fluency then other brain-damaged patient groups (von Steinbüchel et al. 1999).

On the other hand, a level of approximately 30 msec was assessed by measuring the auditory order threshold (OT), defined as the minimum time interval required to identify the temporal order of two successively presented clicks. This temporal range is associated with the perception of succession and phonemic hearing; and has been demonstrated to be basic in reaction time tasks and other high-speed temporal demands (Pöppel 1970; 1997). Patients with left hemisphere postcentral lesions, suffering from Wernicke's aphasia, showed prolonged OT, demonstrating important deficits in temporal processing at this high-frequency level, with impaired detection of single phonemes and lexicon in comprehension; Broca's aphasics were unaffected. These relationships are in agreement with Grodzinsky's thesis that semantic abilities are unaffected following lesions to Broca's area because phonemic hearing is preserved (sects. 1.1 and 1.2).

These observations support the conclusion that specific left hemisphere lesions selectively damage temporal mechanisms critical to the processing of both verbal and nonverbal information within a time frame of approximately 2 to 3 sec, 300 msec or 30 to 40 msec. Moreover, some areas of the left hemisphere play a more important role in temporal processing than others. We postulate that a disruption of timing mechanisms leads to the phonological and/or syntactic disorders commonly observed in aphasic patients. From the evidence briefly reviewed here, it can be seen that the linguistic abilities considered in Grodzinsky's article are governed by the central timing processor. With this in mind, we think that the target article's focus on the patients' linguistic skill in isolation, without any analysis of its neural substrate, cannot give a complete image of language organisation in the brain. Timing is essential to language use and different "neural clocks" underlie the machinery of comprehension and production.

What is special about Broca's area?

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Abstract: We discuss problematic theoretical and empirical issues and consider alternative explanations for Grodzinsky's hypotheses regarding receptive and expressive syntactic mechanisms in agrammatic aphasia. We also explore his claims pertaining to domain-specificity and neuroanatomical localization.

Grodzinsky has presented an impressive range of evidence from aphasia in support of the view that Broca's area and surrounding structures (hereafter referred to as "Broca's region") underlie receptive and expressive syntactic mechanisms. His endeavor to ground his hypotheses in linguistic theory is particularly valuable. Here we discuss a number of problematic theoretical and empirical issues related to his claims.

A syntactic role for Broca's region? Receptive mechanisms. First we address theoretical issues. In the syntactic framework assumed by Grodzinsky, certain constraints apply to all traces (the Empty Category Principle), whereas others distinguish not only between X^0- and XP-traces, but also between two types of XP traces (Chomsky 1981; 1986; Rizzi 1990). Grodzinsky's theoretical motivation for implicating Broca's region in the former distinction, but not the latter, is unclear. One principled difference is that XP but not X^0 traces are assigned thematic roles. However, Grodzinsky rejects the view that the receptive impairment concerns the mediating function of traces in thematic-role assignment (also see Grodzinsky & Finkel 1998). More generally, syntactic theory has shifted away from the concept of traces as syntactic objects in their own right or even as notational devices (Chomsky 1995a), further undermining the theoretical basis of positing a neurological deficit specific to traces.

Second, we turn to empirical issues. The arguments that Grodzinsky presents in support of the Trace Deletion Hypothesis (TDH) rely crucially on a three-way distinction between aphasics' performance at, below, or above chance. However, in a number of cases the level at which aphasics perform is not the one predicted by the TDH. English object-gap relative clauses and Japanese object scrambling are two examples. If subjects are assigned a thematic role through the mediation of a trace (the VP-Internal Subject hypothesis assumed by Grodzinsky), the grammatical assignment of an Agent role to the subject should be precluded in such structures. Thus the subject should not enter into thematic competition with the object (which should get the Agent role by the default strategy), resulting in below-chance performance and not the reported chance performance. Chinese subject-gap relatives are another example. Here, the object gets the grammatically assigned role of Theme. The subject should not compete for this role (unless the default strategy is modified so that non-first NPs get the Theme role), so the observed chance performance would be unexpected. If thematic-role assignment to objects is also trace-mediated (Chomsky 1995a), the object could not be assigned a thematic role grammatically, and should be assigned an Agent role by default, given its linear position as the first NP. This would result in below-chance performance in Chinese object-relatives. Additional sentence types problematic for the TDH are discussed by Beretta et al. (1999), Berndt and Caramazza (1999), and others.

Third, there may be alternative explanations. Grodzinsky discusses only briefly working memory and speed of processing deficits, both of which have been proposed to explain receptive agrammatism (see Kolk 1998). Both explanations warrant further examination: Broca's area has been linked strongly to working memory (Fiez et al. 1996b; Smith & Jonides 1997), and also to fast temporal processing (Fiez et al. 1995). Importantly, reports of dissociations between receptive syntax and working memory (Caplan & Waters 1999) are consistent with the view that different frontal regions may subserve different types of working memory (Smith & Jonides 1997).

Expressive mechanisms. We address theoretical issues first. Unlike the dichotomies between lexical versus functional, or Comp(lementizer)-related versus Infl(ection)-related projections, there is no clear theoretical basis to Grodzinsky's proposed categorical distinction between Tense and Agreement. Moreover, it has been argued that the relative order of Tense and Agreement is crosslinguistically parameterized (e.g., Ouhalla 1991); the order in English is posited to be opposite to that which Grodzinsky adopts for Hebrew, with AgrS (the projection licensing subject-verb agreement) higher than Tense (Chomsky 1993). Thus, impaired Tense and intact Agreement would not be expected in both English and Hebrew, contrary to Grodzinsky's claims.

Empirical issues are also problematic. The data are not consistent with a Tense/Agreement categorical distinction. First, Tense itself can be spared in agrammatism, whereas higher projections are impaired (see Hagiwara 1995). Second, agrammatics can show a graded impairment, with increasingly worse performance at higher projections. For example, Ullman et al. (in press) report decreasing production rates of verbal inflection at increasingly higher levels in the syntactic hierarchy (see also data presented in Hagiwara 1995).

Finally, there appear to be alternative explanations. Hagiwara (1995) has proposed that agrammatics' grammar allows convergence (i.e., successful computation) at lower functional projections, because such structures are less costly from a global economy perspective (i.e., comparing different syntactic derivations; Chomsky 1993). Ullman et al. (in press) argue that graded impairments of functional projections can be explained by deficits of concatenation and/or movement. Because functional categories are assumed to be concatenated and to trigger verb movement stepwise into hierarchical structures, from lower to higher categories (Chomsky 1993), such deficits should yield a greater likelihood of successful computation of lower than higher categories.

Relation between receptive and expressive mechanisms. We have two concerns with the receptive and expressive deficits posited to underlie agrammatism: the lack of an independent factor, linguistic or neuropsychological, unifying the two, and the highly specific nature of the deficits. Impaired computation could arise from deficits of linguistic knowledge (competence) or processing (performance). Although linguistic knowledge is often thought of as highly modular (Chomsky 1981; 1995a), it is generally thought to underlie the computation of structures in both the receptive and expressive modalities (e.g., Crain & Fodor 1989). Thus if linguistic *knowledge* is affected, the deficit should similarly affect both modalities, contrary to Grodzinsky's claims. Indeed, greater deficits in higher than lower functional categories are found in *receptive* as well as expressive agrammatism (Hagiwara 1995). In contrast, although different *processing* mechanisms may be posited for receptive and expressive modalities, they do not normally employ highly specific components, such as a module whose only function is to construct solely those parts of the syntactic tree at and above Tense.

Is Broca's region domain-specific? It is not clear whether Grodzinsky is suggesting that all of Broca's region is dedicated to language, or whether, within this region, there exist specific structures dedicated to language. The first case is clearly false: Evidence suggests that Broca's area underlies motor functions (see Rizzolatti & Arbib 1998). The second case is also problematic. To demonstrate domain-specificity, one must show that *no* nonlanguage functions are subserved by the neural material or cognitive component in question. At the very least it should be demonstrated that those nonlanguage functions most likely to explain a set of linguistic impairments do not co-occur with those impairments. It is therefore puzzling that Grodzinsky concentrates on mathematical combinatorial skills, given that he explicitly posits that Broca's region does not subserve the "basic combinatorial capacities necessary for language processing" (Abstract).

Grodzinsky also claims that Broca's region plays a restricted role within language, subserving only the two hypothesized syntactic functions. However, Broca's aphasics are more impaired at producing, reading, and even judging regularly inflected than irregularly inflected forms (Badecker & Caramazza 1987; Marin et al. 1976; Ullman et al. 1997; in press). This morphological affixation deficit in both expression and reception cannot be explained by Grodzinsky's hypothesized syntactic dysfunctions. Finally, there is also substantial evidence that Broca's area plays a role in phonology (see Demonet et al. 1996) and in lexical search or retrieval (see Buckner & Tulving 1995).

Anatomical localization. Grodzinsky's effort to implicate Broca's region alone in the hypothesized syntactic functions is hampered by problems of patient selection. Conclusions regarding the function of Broca's region would be less problematic if patients were selected *solely* on the basis of their lesions, which should be limited to those structures. However, many of the Broca's aphasics on which Grodzinsky bases his claims also have lesions outside Broca's region, or, even worse, have no reported lesions to this region at all (e.g., Friedmann & Grodzinsky 1997; Grodzinsky 1989; Grodzinsky & Finkel 1998. For additional discussion on patient selection, see Berndt & Caramazza 1999; Grodzinsky et al. 1999.) More generally, chronic Broca's aphasia is also associated with damage to left parietal regions (Alexander 1997). Grodzinsky points out that Wernicke's aphasics' failures in syntactic comprehension are inconsistent and varied. Perhaps this variability of impairments correlates with Wernicke's aphasics' variability in damage to inferior parietal regions (Alexander 1997). Indeed, conduction aphasia is associated with both left inferior parietal damage (Alexander 1997) and syntactic processing deficits (see Caramazza et al. 1981). It may be that left inferior parietal regions, in concert with left frontal structures, underlie grammatical processing, in a working memory role (Smith & Jonides 1997), or perhaps as a repository of grammatical knowledge.

Conclusion. We have argued that a number of Grodzinsky's specific claims are problematic, and should therefore be weakened or modified. Nevertheless, we strongly support his program relating language deficits to linguistic theory, and believe that such an approach will prove crucial to our understanding of both the neurobiology and structure of language.

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The need to consider additional variables when summarizing agrammatism research

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Abstract: Throughout the history of aphasiology, researchers have identified important premorbid and stroke-related predictors of linguistic performance. Although Grodzinsky discusses some of these variables, exclusion of other variables could lead to unnecessary experimental error and erroneous conclusions. Aspects to consider include sources of experimental bias, premorbid differences, nonlinguistic roles of the frontal regions, and comparison of normal and aphasic performance.

Grodzinsky identifies common factors of linguistic performance across languages and tasks, reviewing data from various studies. He considers the variables of diagnostic label and lesion source when collecting results of studies, and his analysis of spoken output takes into account differences among languages' canonical sentence order and morphosyntax. Important aspects of how the studies' original data were obtained have not been taken into account, however. These aspects can be categorized as sources of nonsampling and sampling bias, premorbid differences, nonlinguistic roles of the frontal cortical regions, and comparative data about neurologically normal performance. A brief review of these variables not considered in Grodzinsky's article suggests that combining the results of agrammatism studies could lead to erroneous conclusions.

In applied social research methods, there are three components that comprise experimental error, or the difference between the results of a sample and the truth about a population. Nonsampling bias includes errors in defining the population of interest. Sampling bias consists of inequalities caused by unequal or disproportionate sampling from subgroups of the population under consideration. Sampling error is the expected set of differences between the sample and the population, creating the need for inferential statistical methods (Henry 1998). In agrammatism research, only sampling error has been addressed fully (Bates et al. 1991).

Nonsampling bias, specifically errors in defining the population, can result from problems with subject classification (Henry 1998). It has been noted that classification systems within standard aphasia batteries have imperfect classification and diagnostic abilities, leading to overlap among categories (Clark et al. 1979; Rao 1994). Also, different aphasia batteries can give different labels for the same subject's performance (Caplan 1987b). As a result, selecting subjects only according to a diagnosis of Broca's aphasia from various batteries could lead to possible differences in subject selection criteria across studies (Bates et al. 1991).

An unknown degree of sampling bias could result from three problems with subject selection methods used in agrammatism research. First, we know little about the nature and prevalence of documented subgroups of Broca's aphasia, described by either lesion site or performance (Love & Webb 1992; Sundet & Engvik 1985). Second, we do not know how subjects were selected out of the available pool of Broca's aphasics for each study (Bates et al. 1991). Third, we do not know much about the relationship of lesion site and diagnostic label to the naturally occurring category or population of impairment underlying diagnoses of Broca's aphasia, anterior aphasia, nonfluent aphasia, or agrammatism, making it difficult currently to sample equally or proportionately (Anderson 1991; Bates et al. 1991; Menn et al. 1995).

In addition, premorbid differences may contribute in unknown ways to differences in patient performance. Interpersonal sociological factors such as age, educational level, number of languages spoken, and socioeconomic background, in addition to their interactions and relationships with performance variables, are considered increasingly important to modern aphasia research (Bates et al. 1991; Coffey et al. 1998; Menn et al. 1995). Also, various unknown or inadequately researched cohort effects are thought to result from generational or historical changes within the population of interest (Glenn 1977). These changes may be significantly associated with subtle but important changes in the epidemiology of stroke and its clinical profile in the population. Furthermore, aspects of hemispheric specialization and aspects of memory are two areas of research that have contributed greatly to understanding individual, normal neurolinguistic, and psycholinguistic differences (Dean 1985; Engle 1996).

Also, focusing almost exclusively on grammatical performance and comprehension may lead to a disregard of interactions with nonlinguistic roles of the frontal lobe. In addition to the syntactic-semantic aspects of frontal lesions, deficits in oral-motor abilities and working memory also result from lesions to frontal regions (Brookshire 1997; Damasio & Anderson 1993; Darley et al. 1975). Likewise, direct effects of brain damage may be confounded by the concurrent use of compensatory abilities and strategies (Blackwell & Bates 1995; Menn et al. 1995).

Furthermore, Grodzinsky's approach has de-emphasized comparative information on how neurologically normal control subjects perform on similar tasks. Various studies using neurologically normal speakers of a language have demonstrated the existence of occasional discrepancies between grammatical competence and performance and the existence of normal performance errors on neurolinguistic measures (Cook & Newson 1996; Lezak 1995). Consequently, there is no assurance that the tasks given to subjects with aphasia are ones that all neurologically normal, competent speakers would always perform "perfectly" or ideally. As a result, overall experimental error may result from comparing aphasics' performance with ideal syntactic-semantic output, rather than comparing normal and aphasic performance to determine their similarities and differences.

In summary, the type of impairment variously labeled and diagnosed as Broca's aphasia, anterior aphasia, nonfluent aphasia, or agrammatism is apparently a natural category of impairment with unique qualities, but the multidisciplinary field of aphasiology is far from discovering its nature. Grodzinsky's research and that of others have been invaluable in determining aspects of the nature of this type of aphasia and the differences between its syntactic-semantic performance pattern and ideal performance. However, the inclusion of additional, previously identified variables and results of other lines of research would improve the knowledge about this category of aphasia.

Author's Response

The Trace Deletion Hypothesis and the Tree-Pruning Hypothesis: Still valid characterizations of Broca's aphasia

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Abstract: I begin with a characterization of neurolinguistic theories, trying to pinpoint some general properties that an account of brain/language relations should have. I then address specific criticisms made in the commentaries regarding the syntactic theory assumed in the target article, properties of the Trace Deletion Hypothesis (TDH) and the Tree-Pruning Hyothesis (TPH), other experimental results from aphasia, and findings from functional neuroimaging. Despite the criticism, the picture of the limited role of Broca's area remains unchanged.

R1. Working in the syntax/neuroscience interface is hard

What do neurolinguists do? What makes their intellectual life interesting? A serious investigation of the functional neuroanatomy of language currently involves a rich and complex array of data, both behavioral and anatomical. A learned discussion of these, in turn, presupposes deep knowledge of brain research techniques and formal linguistics, as well as sophisticated methods of experimental design and data analysis. Language is a complex mental function, and studying it is hard.

The process of discovery and understanding in this domain is complex and consists of a series of steps that the neurolinguist must take. First, a question of interest Q must be formulated, dictating the focus of inquiry. This must be followed by the introduction of the theoretical apparatus T in both the behavioral and the neural domains. Next, one must establish an explicit mapping M from the theory onto the measured behavior. The choice of T, Q, and M then dictates a set of experiments E, which, if carried out properly, produce reliable results R. Finally, an interpretation I brings R to bear on T modulo M.

Articles published in *Behavioral and Brain Sciences* often provide a unique opportunity for a passer-by to get a glimpse of a scientific field. Sometimes, they also allow an observation into the sociology and intellectual norms of a research area, as multiple backgrounds, positions, and points of view are rolled into one treatment. In this context it is interesting to note that although my critics made many important points regarding the choice of T, criticized certain members of E, added scores into the pool R, and disagreed with me on I, not one commentary challenged Q or M. Is this because of a very broad consensus on these issues, or a collective oversight?

In my response, I try to address this and other questions, to consider alternatives, and to propose solutions to other problems and issues that were raised in the commentaries. As explicitly as possible I lay out the assumptions underlying the approach espoused in the target article. First, I discuss a host of conceptual issues that arise regarding theoretical choices, mapping and method. Then, I consider certain empirical problems. I address as many criticisms as I can, and try to organize the response so as to make the reader's life easier. Issues are thus addressed from general to specific, making it possible to focus on things one cares about, and skip the rest.

The target article ended with a glance into the past – a tribute to Paul Broca, a founder of my field. The response will conclude with an attempt to look forward: I will consider what a final theory of brain/language relations will eventually look like.

R2. Properties of the best theory

An adequate theory of the functional neuroanatomy of language must have certain properties; these are laid out below with examples that come from the commentaries.

R2.1. Generality and exclusiveness

All and only the relevant data must be accommodated. There are three different aspects to this property in the present context: cross-structural (spanning data from all the relevant syntactic constructions), cross-linguistic (encompassing data from different languages, especially where languages diverge), and cross-task and method (accounting for principled variation across experiments). The Trace Deletion Hypothesis (TDH) with its two parts (trace-deletion and default strategy) was designed to satisfy this property. Consider, by contrast, one family of theories that purport to offer an alternative to the TDH, and attribute the syntactic deficit in Broca's aphasia to a disability with structures that deviate from canonicity (**Kay, Piñango**). On this view, Broca's aphasics are said to do well on canonical structures yet to be unable to represent, and hence to guess at, "noncanonical" ones. Canonicity is determined by the order of arguments in lexical representation. This account fails to be general in all three respects.

R2.1.1. Cross-structural failure. The patients' performance pattern reveals surprising comprehension asymmetries between sentences that differ in certain respects, but not in the order of arguments. These are agentive versus experiencer predicates in passives (1a-b), and referential versus nonreferential subjects in questions (2a-b) and passive (3a-b). Critically, in each comparison the positions of arguments relative to the predicate (which determine canonicity) remain fixed:

(1)	a. The woman was pushed by the man	***chance***
	b. The woman was loved by the man	***below chance***
(2)	a. Which elephant did the giraffe sniff?	***chance***
	b. Who did the giraffe sniff?	***above chance***
(3)	a. The woman was pushed by the man	***chance***
	b. Every woman was pushed by a man	***above chance***

Such contrasts, which the TDH accommodates (as shown in the target article), cannot in principle be couched in a theory that is based exclusively on the relationship between canonical and actual positions on NPs in a sentence.

R2.1.2. Cross-linguistic differences. These accounts cannot accommodate the striking cross-linguistic contrast in which there is complete reversal in performance patterns on relative clauses in Chinese and English, both SVO languages with identical canonical structure (and identical ϑ-structure). This contrast, which follows directly from the TDH, is reiterated in (4)–(5):

(4) *Chinese relative clauses*
a. *Subject:* [t_i zhuei gou] **de**$_i$ **mau**$_i$ hen da — ***chance***
chase dog COMP **cat** very big
the cat that chased a dog was very big
b. *Object:* [mau zhuei t_i] **de**$_i$ **gou**$_i$ hen xiao — ***above chance***
cat chase COMP **dog** very small
the dog that the cat chased was very small

(5) *English relative clauses*
a. *Subject:* **[The man]**$_i$ **who**$_i$ [t_i pushed the woman] was tall — ***above chance***
b. *Object:* **[The man]**$_i$ **who**$_i$ [the woman pushed t_i] was tall — ***chance***

R2.1.3. Tasks. Accounts claiming that aphasics have trouble comprehending sentences with deviations from canonicity have little to say about other tasks. In particular, they have no prediction regarding the patients' selective failure in judging the acceptability of certain sequences, or in their failure to prime normally in a position of a gap. The TDH, by contrast, is designed to explain such results.

R2.2. Deductive structure

The mapping M must be explicit, leading to clear predictions regarding (past and future) experimental results. This is perhaps the most salient and important property of the TDH. Its bipartite structure (trace-deletion, strategy) provides two premises that are taken as part of the mapping M from normal to deficient linguistic ability. These two premises, once applied to a sentence with a given structure and a given task, provide the machinery from which results can be deduced directly.

R2.3. Transparency

Performance levels for each structure in each task must follow directly and independently from the theory. This requirement is strict: The TDH makes no statements of the form "performance on A is better (or worse) than on B." That is, it has a prediction for each data point, not only in terms of its relation to others (e.g., passive vs. active), but also in terms of its own value relative to chance. The latter type of measure is extremely important for the interpretation of performance levels in experiments where the dependent variable is discrete, namely those with binary-choice decisn (as is in most comprehension tests). So, in syntactic constructions like the passive, trace-deletion blocks ϑ-transmission to the moved subject, which in turn is assigned an agent role by the strategy. The result is a representation with two agents, and chance performance is derived deductively, independent of the performance on other structures. Performances are very carefully measured, and detailed (group and individual) statistics are reported in each of the experimental papers on which the analysis relies (contrary to **Bickerton**'s claim on this matter). As performance is examined in terms of its relation to chance, a distinction between types of erroneous performances (chance level vs. below-chance) emerges. This distinction corresponds to the contrast between the Broca's performances on passives with psychological and agentive predicates, as shown in the target article. I know of no other account that has this property. Take complexity-based accounts (**Kolk & Hartsuiker, Pickering, Stowe,** and others), for example. Here, the idea is to establish a nonarbitrary metric for complexity, one that makes reference to structure. These metrics are rarely spelled out explicitly or motivated theoretically. To take one example, Pickering expects nested dependencies to be more complex than consecutive ones, yet gives no reason for this expectation. More important, complexity-based predictions are inherently weaker than the TDH. This is so because performance on any construction can be deduced from the TDH directly (i.e., structure S yields performance level L), yet complexity-based accounts can only make comparative statements (i.e., structure S is easier/harder than T). So, even if a complexity metric in syntax can be established, specifying an order of difficulty among structures, additional assumptions are necessary to turn it into a theory about the "order of breakdown" in aphasia. By contrast, the TDH provides a stronger and more precise prediction for each stimulus type.

R2.4. Falsifiability

It is common, especially in the social sciences, to say that a theory must be accompanied by a clear procedure for falsification. Somehow, as physicist Daniel Amit (1996, p. 653) pointed out so eloquently in this journal, this requirement is overemphasized and misconstrued in biology (and the social sciences). It is important to note that a theory is at best "refuted" not by data, as some commentators (**Beretta, Berndt, Dick & Bates, Dronkers, Müller**) erroneously contend, but, rather, by an alternative proposal. Thus, a finding that runs contrary to the predictions of the theory may indeed be worrisome and should be considered very seriously, but is itself hardly sufficient for refutation.

The proposals made in the target article satisfy these properties. Alternatives should be considered according to the principle that a theory cannot be replaced by another that is more vague or has narrower data coverage. I will argue that the proposals made in the commentaries are not viable competitors.

R3. The mapping – structure and task specificity

A claim regarding the functional neuroanatomy of language (or any other piece of cognition) usually relates some putative underlying mechanism(s) to a brain part. The goal is to break represented knowledge and processes that underlie language behavior into their component parts, and relate them to brain regions. Sources for experimental results divide into two groups: measures of cerebral activity (ERP, fMRI, SPECT, PET, MEG, etc.), and behavioral measures (RT and error rates). Our discussion in this section focuses on behavioral measures. Although knowledge recruited for language use presumably comes from a single source, different tasks potentially tap different combinations of processes. Thus each experimental method may require its own function M that maps knowledge + processes onto measured behavior. It is thus possible, in principle, for stimuli bearing the same linguistic structure to generate very different results, because of different task characteristics (cf. Grodzinsky, 1988, for a demonstration that although Broca's aphasics comprehend actives at near-normal levels, and passives at chance, their performance is reversed when they are required to detect violations of grammaticality in these structures, which involve substitution of a preposition). Conversely, different structures may lead to similar results if appropriate tasks are selected. This makes cross-task comparisons difficult, but it also makes theoretical generalizations that are upheld across tasks more compelling than arguments that are based on findings from one task only.

It is perhaps a curiosity that no issue concerning M (Mapping of Theory onto Behavior) was noted in the commentaries, although many critics were rather detailed in pointing out potential empirical problems for the TDH. Moreover, although many commentators criticized this or that piece of comprehension data, or brought additional comprehension results to bear on the debate, little attention was paid to the fact that the TDH successfully accounts for data ranging from error analysis in comprehension to aberrations in grammaticality judgment and real-time contrasts between normal and aphasic sentence processing. Of the alternatives sketched in the commentary, none can be said to have data coverage that cuts across tasks and method. Some examples will make this point clear. Consider complexity-based accounts first (e.g., **Pickering, Stowe,** and others). Broca's aphasics are expected to do well on "simple," hence "easy" structures, but fail on "complex" ones. Coherent complexity metrics – a must for such accounts to get off the ground – are hard to come by. Still, on most psycholinguistic views, subject-gap relative clauses are less complex than object-gaps because only the latter deviates from canonicity (e.g., Just et al. 1996). If complexity acted as the source of difficulty in Broca's aphasia, we would expect it to manifest equally across tasks. Yet the data tell us a different, more intricate and interesting story. When tested in Cross-Modal priming tasks, Broca's aphasics cannot prime for antecedent NPs in their respective gaps (i.e., point *3* in 6a and point *2* in 6b), failing in both subject- and object-gap center-embedded relatives (despite putative differences in complexity between them). However, their comprehension performance differentiates between the two constructions (7):

(6) a. The priest enjoyed **the drink**^{1} that [the caterer was ^{2} serving $\boldsymbol{t}^3$ to the guests]
b. The professor liked **the waitress**^{1} who [$\boldsymbol{t}^2$ was serving drinks to the guests]

(7) a. *Subject:* **[The man]**$_i$ **who**$_i$ [$\boldsymbol{t}_i$ pushed the woman] was tall — ***above chance***
b. *Object:* **[The man]**$_i$ **who**$_i$ [the woman pushed $\boldsymbol{t}_i$] was tall — ***chance***

Moreover, when tested for comprehension on right-branching structures (8), Broca's aphasics give the same pattern of results as they do for the center-embedded ones in (7), indicating that they suffer a movement, rather than complexity, failure:

(8) a. *Subject:* Show me **[The man]**$_i$ **who**$_i$ [$\boldsymbol{t}_i$ pushed the woman] — ***above chance***
b. *Object:* Show me **[The man]**$_i$ **who**$_i$ [the woman pushed $\boldsymbol{t}_i$] — ***chance***

It is hard to imagine how this intricate array of results can be accommodated with a complexity-based account, yet it follows directly from the TDH, as I showed in the target article (see Grodzinsky, 1989, for further discussion of complexity in the context of relative clauses). The data presented in **Friedmann**'s commentary likewise resist a generalization that is not dependent on syntactic movement.

Moving on to results from grammaticality judgment, we note that the violations in (9) fall under one linguistic generalization (Relativized Minimality, Rizzi 1990), yet the performance impairment is selective precisely in a way that the TDH is designed to accommodate. It is difficult to see how a complexity-based deficit analysis could account for this pattern:

(9) a. *John seems that it is likely to win — ***high error rate***
b. *I don't know what who saw — ***high error rate***
c. *Have they could leave town? — ***Low error rate***

Next, we take a look at proposals that build on canonical ordering of constituents (**Kay, Piñango**). Recall that the claim is that Broca's aphasics fail whenever a structure contains an overt ordering that deviates from canonicity (as determined by lexical representation). Yet, because no mapping function is provided for any task, it is hard to make predictions about tasks other than comprehension. But even if such a mapping were to be devised, it would not account for the data in (6), in which both canonical and noncanonical orderings of constituents produced performance aberrations. Moreover, when the data in (7) is juxtaposed to (6), the irrelevance of canonical ordering becomes even more apparent. As for the deficit in (9), I am aware of no reason to believe that it is related to canonicity.

Finally, consider accounts that attribute the deficit to a general failure in cognitive ability, whether caused by timing in general cognitive representations (**Szelag & Pöppel**), sequencing, (**Dominey & Lelekov, Murray**), computation (**O'Grady**) or memory failures (**Previc**). Some of these are genuinely sophisticated proposals, that should be explored seriously, through wide-ranging research programs. I therefore feel that I am giving them an unjustifiable short shrift. Yet, in the present context I cannot go much beyond noting my own limitations. I find it hard to imagine how general failures of these types could lead to task- and structure-specificity of the types presented in the target article. The complex array of data that is available calls for a specific account, which then leads to the conclusion that Broca's area is highly specialized. Still, some authors (Swinney & Zurif 1995) have attempted to argue for a timing failure, yet one that is apparent only in the context of tasks that force the construction of specific linguistic representations (as some commentators – **Hickok, Piñango, Ullman & Izvorski** – have also proposed). I have little to say about this issue. A timing failure that underlies the structural deficit in Broca's aphasia is a logical possibility, but compelling data are yet to be presented.

R4. Syntax

Any attempt to state a generalization over a wide range of linguistic data that purports to be both precise and general must make theoretical choices of various sorts. The single most important intuition underlying the TDH/TPH is that a lesion in Broca's aphasia affects only part of syntax, leading to the conclusion that the lesioned cerebral area is the locus of certain subsystems of syntax. In formulating these hypotheses such choices were made, generating critical commentary. In this section I will address these points, which I divide into two parts: (1) Criticisms of choice of the general descriptive framework, and (2) more specific linguistic problems that arise in the context of the TDH. These are addressed from general to specific.

R4.1. Movement and traces

R4.1.1. Minimalist issues. The TDH/TPH were formulated in the descriptive language of generative grammar, which raised a number of objections and queries, including whether the formulation is generally compatible with recent versions, specifically, with the Minimalist Program (MP, **Bánréti, Bickerton, Edwards & Lightfoot** and **Frisch et al.**) Positions vary. Edwards & Lightfoot correctly note that in the MP, the process that merges categories constructs a tree while taking movement into account "on the fly" as it were, that is, during the derivation itself. Yet, this observation does not lead to their conclusion that this model cannot accommodate the distinction between operations that are structure-building and those that establish dependencies among constituents. It is questionable whether a rule like Merge should be taken as a concrete claim about sentence processing, as Edwards & Lightfoot would have it. But even so, viewing it as they do obfuscates important issues: Changes in theoretical conception do not change the facts. The formulation of the MP has not dispensed with the need for certain independent constraints on movement (like Relativized Minimality), distinguishing it from other types of syntactic relations. Similarly, whether movement is formulated over features (Frisch et al.), or copies (Bickerton), the need for such a relation is there, and it is this relation that serves as the basis for the TDH.

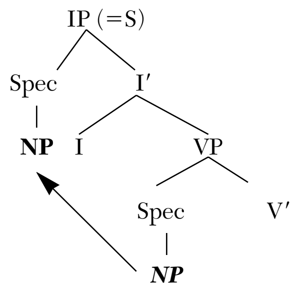
R4.1.2. Movement and case. In the MP, **Bánréti** points out, the interpretation of noun phrases (DPs) is dependent on case assignment, which requires feature checking, and hence movement into an appropriate position. As a consequence, both subject and object NPs move. Bánréti is thus concerned that the MP posits more traces than the TDH can handle. Again, this is a position that adheres too literally to the details of one formulation. First, Bánréti's presentation of the theoretical mechanism itself shows that the feature-checking model has no consequences for the TDH coverage of data in comprehension, because the assignment of ϑ-roles takes place prior to movement to check features. The critical cases for comprehension, then, are those DPs that depend on the trace for ϑ-role assignment. The TDH picks them out correctly, regardless of other movements. Note that even if movement to a feature-checking position leaves a trace, its deletion would not result in an inability to detect case violations, because this detection does not depend on the trace. It is interesting that this distinction receives empirical support from an unexpected direction. As I pointed out in the target article, Broca's aphasics have virtually intact abilities in the domain of case, as evidenced by experiments in languages with overt case marking, for example, Serbo-Croat (Crain et al. 1989; Lukatela et al. 1988) and also in English (Linebarger et al. 1983). Their performance, then, distinguishes between checking-motivated, and thematic, "real" movement; their syntactic capacity, therefore, is intact when a dependency relation does not involve a trace. Further evidence comes from Japanese. As **Hillert** and **Frisch et al.** point out, Japanese patients are unable to make use of overt case marking. However, this observation leads to a conclusion that is opposite to theirs: The patients' inability to use case argues for a dissociation between case and movement, rather than against the TDH. Therefore, the distinctions made by this hypothesis are fine, but allow for very wide data coverage.

R4.1.3. LF-movement. Is the TDH not too strong? **Cappa et al.** wonder. They, too, point out that movement operations feature in other domains of syntax, mainly in cases where they are covert (LF movement). In such instances, constituents appear *in situ,* although their interpretation can be shown to require syntactic movement. Cappa et al. are right: The status of these cases in Broca's aphasia is unclear. The appropriate experiments have not yet been done, and whether or not LF-movement is implicated in the deficit is an open empirical question that calls for sophisticated experimentation.

R4.1.4. Traces. In a different vein, some commentators express doubts regarding the validity of the arguments for representations that contain traces (**Ben Shalom, Hillert, Kay, Pickering**). This issue is independent of the TDH. One could imagine the TDH stated in a theory that has no traces. All critics agree that the existence of traces is an entirely empirical matter. Indeed, some have questioned the validity of the experimental evidence from normal real-time processing. In this context it is important to note that arguments for or against traces do not necessarily come from psycholinguistics. There has been a long debate about it in the linguistic literature (ignored in the commentary), and neurolinguistic evidence from aphasia strongly supports the need for traces (or at least of generalizations over different movement types) in the theory of grammar (Grodzinsky et al. 1991). Moreover, no commentary offered an account for the selective impairment Broca's aphasics have in grammaticality judgment in violations that depend on traces (whose deletion, incidentally, does not imply deletion of their structural positions, contrary to Pickering's suggestion). One can safely conclude that the TDH is compatible with the general linguistic principles on which it is based.

R4.2. Compatibility of theory with results

R4.2.1. Traces in [Spec,VP] and the TDH. Many commentators (**Friedmann, Frisch et al., Hickok, Otsu, Schaeffer, Ullmann & Izvorski**) worry about empirical problems that may arise if the TDH is coupled with a syntactic theory that assumes subjects start out in the Specifier of VP position (the VP-Internal Subject Hypothesis, henceforth ISH, Kitagawa 1986; Koopman & Sportiche 1988; Kuroda 1986). This concern may, in fact, be a specific instance of a more general problem raised by **Bánréti** regarding the movement of subjects. A serious examination of this problem presupposes a precise characterization of potentially problematic places in the comprehension data. The TDH accounts for deficient performance on structures containing two NPs, in which only one is moved. The standard case for chance performance is one in which the object moves and crosses the verb, and the subject stays in its base position. Recall that in such a case, the (unmoved) subject receives the external (mostly agent) ϑ-role, whereas the moved NP receives a role by strategy (mostly agent, as well). That is how chance performance is derived. But what if both NPs move? Adopting the ISH creates such a case, as it assumes that subjects move up from the Specifier position of VP, to the "traditional" subject position [Spec,IP]:



If a result postulated by the TDH crucially depends on this NP's staying *in situ,* problems may arise. Example cases are scrambled actives in Japanese and English object-gap relative clauses, presented here according to their ISH-compatible analysis, and with their TDH-assigned roles – grammatically (G) and by strategy (S):

(11) a. **Hanako$_i$-o Taro$_j$**-ga [t_j t_i nagutta] ***chance***
Agent (S) **_Agent_** (G)
"Taro hit Hanako"
b. **[The man]$_i$ who$_i$** [**the woman** [t_i pushed t_i]]was tall
Agent (S) **_Agent_** (G) ***chance***

(Note that the assignment in [11b] is done directly. **Newmeyer** points out that the assignee of the ϑ-role should not be the head of the relative, but rather the [overt or silent] relativizer (*who*). This may be true, although some analyses consider direct ϑ-role assignment from the predicate of the relative to the head. Be it as it may, both views have identical consequences. The concern in these cases is that because the trace of movement from VP to IP is deleted, the grammatically assigned ϑ-role cannot reach its destination. This leaves the subject in both (11a) and (11b) ϑ-less, and the analysis falls apart.

This problem is potentially serious, but there are some solutions. One creative solution is to take this presumed incompatibility between linguistic analysis and aphasia results and turn it on its head, namely, to *take it as evidence against the ISH.* This is what **Schaeffer** proposes. She cites results from language acquisition that presumably point in the same direction, and concludes that the ISH may be invalidated on psycholinguistic grounds.

Schaeffer's is an interesting proposal, but it leaves the linguistic data that originally motivated the ISH unexplained. It is therefore wise to look for ways to reconcile this hypothesis with the TDH. Several ideas come to mind. One is to assume that if a ϑ-role cannot be assigned to a position because a trace is absent, an adjacent position is capable of inheriting this role. This idea is very much in the spirit of ϑ-theory, which in general operates under conditions of sisterhood and adjacency. Alternatively, we can reformulate the strategy so that it can apply in a more restricted manner. If two moved NPs in the same string do not have ϑ-roles, both would presumably be under the scope of the strategy. Such cases arise in sentences that have more than one clause. Thus, if the strategy applies once per clause, it could assign the role of agent twice by default: once to a first NP in the main clause, and another to the first NP in the subordinate clause. It is important to note that imaginable solutions exist, and that each of these has different empirical consequences that can be formulated precisely and then tested. It is hoped that more relevant test results will become available as research proceeds, but currently, the interaction between the TDH and ISH does not lead to inconsistencies or problems with the data.

Another potential concern regrading ϑ-role assignment in Broca's aphasia is pointed out by **Newmeyer,** who wonders why patients allow for thematic representations that run against their thematic knowledge, which I claimed to be intact. Specifically, he wonders how the patients allow for ϑ-representations with two agents, and how agentless predicates may end up with an agent (as is the case in the TDH account of passives with psychological verbs). Incompatibilities between the normal ϑ-representation of a sentence and that of a Broca's aphasic may indeed arise. Yet reflect for a moment on the nature of the strategically assigned ϑ-role. It is, by definition, nonlinguistic. As I have argued elsewhere (Grodzinsky 1990, Ch. 5) there are good reasons to believe that the strategy snaps into action after thematic representations become output. Only after the ϑ-assignment is finished can incomplete thematic representations be detected and augmented by the strategy. Therefore, incompatibilities of the sort Newmeyer highlights indeed occur, yet at a stage where the ϑ-criterion, as well as other linguistic principles, are no longer operative. The above-cited discussion of this issue, in fact, takes these considerations as an argument for strict modularity of the sentence processing device.

R4.2.2. Is AgrP higher than TP? The production aspect of the linguistic description of Broca's aphasia (hence of the function of the associated neural tissue) makes use of a grammatical distinction between Tense and Agreement features (Tree-Pruning Hypothesis-TPH). The main point is not just the new observation that such a distinction exists in the aphasia data, but more importantly, that grammatical properties that form a natural class with tense (placement of negation, copulas, nominative case, complementizers, and verb movement to C) are all deficient, as opposed to those clustering with agreement, which are not. The available evidence comes from an impressive array of languages (see Friedmann 1998). Commentators have taken issue with this claim on various grounds. Many criticize the TPH on the grounds that it gets things backwards (**Bánréti, Bickerton, Newmeyer, Ullman & Izvorski**). For us, Tns is higher than AGR, yet the MP has the Tense node higher in the tree than AGR (i.e., TP contains AGRP). I find this criticism strange for three reasons:

(1) The split inflection hypothesis has provided good reasons to believe that there is more than one inflectional node. However, the number, as well as the internal ordering of the nodes, is far from being a closed matter. Some authors have proposed other inflectional categories (e.g., Koizumi 1995; Siloni & Friedemann 1994). Heavy linguistic arguments in favor of AGR $\gg$ Tns would indeed give rise to concerns. As things stand, the issue is unresolved, and thus the conclusion is opposite to the one suggested by the critics. Rather than being questionable, the TPH analysis of the production findings from aphasia provides a powerful, neurologically based argument for Tns $\gg$ AGR (cf. Friedmann, 1998, for further discussion).

(2) It is interesting to note that although critics hasten to dismiss the TPH on these grounds, the interesting clustering of aphasic production phenomena has not been taken seriously in any of the commentaries, nor has an alternative been proposed.

(3) In production, there is clearly an issue of severity as **Kolk & Hartsuiker, Penke,** and **Ullman & Izvorski** correctly point out. Disagreement begins when one considers possible accounts. Everyone notes that the production deficit is graded. The question is whether this gradation is principled, and in particular, whether it goes along syntactic lines.

This is an issue considered in detail in Friedmann and Grodzinsky (1997). We analyzed a wide range of data from English, Italian, French, and Hebrew (further extended in Friedmann [1998] to additional languages, as well as to larger numbers of patients and large size corpora, and supported in Bastiaanse & van Zonneveld's [1998] Dutch study). Against this context, counterexamples such as those of Penke, who found a set of cases with no such production deficit, are indeed puzzling. Still, the weight of the evidence (including French and Italian for which there is plenty of data that attest to selectivity, contrary to Penke's claim) points to the selectivity-based view.

We observed that when a functional category is impaired it affects not only its phrasal projection, but also anything above the functional category in the tree that is pruned. We further noted a qualitative gradation of the deficit: The most severe cases are those in which all functional categories are impaired; less severe cases are those that distinguish Tns from AGR; and finally, there are cases in which only CP is pruned (Hagiwara, 1995, for example, which, contrary to **Newmeyer**'s and **Ullman & Izvorski**'s claim, fits well into our account). We thus observed a hierarchy that can be depicted graphically as shown in Figure R1.

On the basis of this typology, we proposed the first formal severity metric for the production deficit in agrammatic Broca's aphasia:

(12) *Severity metric for agrammatism* (Friedmann & Grodzinsky 1997)

For P_1, $P_2 \ldots P_n$ different variants of the syndrome, P_i are more severe than P_{i-1} if and only if N_i, the node impaired in P_i, is contained in the c-command domain of N_{i-1}, the node impaired in P_{i-1}.

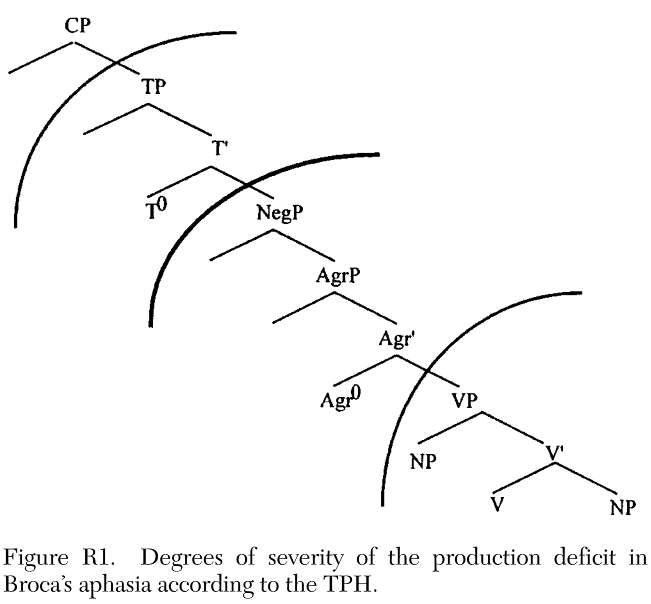


Figure R1. Degrees of severity of the production deficit in Broca's aphasia according to the TPH.

R4.2.3. Comprehension-production parallelism. Some commentators have questioned the relation between language production and comprehension mechanisms as they emerge from the TDH/TPH. Their different descriptions force the conclusion that the mechanisms supporting the two modalities are different, at least to some degree. **Bickerton, Kempen,** and **Ullman & Izvorski** see it as a flaw. I fail to see why, for two reasons. First, it is clear that normal mechanisms for the planning and analysis of sentences are not exactly two sides of the very same coin. Second, whether or not the deficit descriptions of both modalities are the same is a matter of contingent truth, not of principle. The only acceptable proposal regarding parallelism is a unified account that derives both the TDH and the TPH, which is what **Luzzatti & Guasti** have attempted to devise. They propose that the TDH in fact follows from the TPH. Modifying their proposal a bit, the idea is that subject positions are above the pruned Tense Phrase (by the TPH), and thus movement to subject (which is true of all the cases that have been tested for comprehension) leads to the comprehension deficit, as characterized by the TDH. This proposal is appealing, but I believe that it fails at least on empirical grounds. The comprehension asymmetries that the TDH accounts for (agentive vs. psychological passives; quantified vs. referential subjects in passive; *who*- vs. *which*-questions) do not follow from it in any obvious way. The same is true for the grammaticality judgment results (see Grodzinsky 2000, for a reappraisal of Zurif's "overarching agrammatism" hypothesis). Still, Luzzatti & Guasti's thinking is in just the right direction (see Friedmann, 1998, for discussion along the same lines).

R5. Broca's area and Broca's aphasia

R5.1. Understanding versus documenting natural phenomena

Working in the linguistics/neuroscience interface puts one in double jeopardy. In this response, I have thus far dealt with linguistic problems. It is now time to address neurologically motivated criticisms. There are important differences between the two approaches. Linguists are mostly concerned with understanding, which means that they attempt to construct a coherent picture of the data. It is universally accepted that the right way to go about research is to seek reason, commonality, and precision, that is, theoretically based, motivated generalizations. The discussion thus centers around the nature of the picture of the language faculty, not on the need to construct it, nor on how to go about it.

By contrast, more than a few of the neuropsychologists among the commentators focus on documenting "effects," such as demonstrating differences among patients, "falsifying" theoretical claims, and pointing to data points that would put creases on an otherwise elegant picture. Thus, with few exceptions, the commentaries are mostly concerned with debunking the TDH/TPH approach, and offer few, if any alternatives. This activity is very useful, as it keeps a theoretician honest. It may also be disruptive, however, in that on occasion, apparent differences divert attention from true similarities. Section R5.2 will give two illustrations of this contingency, one from speech production, the other from comprehension.

I will discuss three types of criticisms: methodology, neuroimaging, and some other, more minor, residual problems.

R5.2. Methodology – intrasyndromic variation

The centrality of individual differences in aphasia concerns many commentators (**Berndt, Caplan, Cappa et al., Dick & Bates, Edwards & Lightfoot, Hickok, Luzzatti & Guasti, Young & Hutchinson**). Some of these commentators even see irreconcilable differences among patients, to a point where they resist generalization. This issue has featured rather centrally in the recent debate on Broca's aphasia and is recurring here as well.

Consider first an argument made by **Cappa et al.** who bring up Miceli et al.'s (1989) study as a demonstration of variation in error rates among agrammatic Broca's aphasics. Seeking "to consider the range of production deficits involving the omission or substitution of grammatical morphemes in patients clinically classified as agrammatic" (p. 449), Miceli et al. analyzed speech samples of 20 Broca's aphasics along 5 dimensions, and found apparently vast variation. On free-standing morphemes the overall percent of errors ranged from 9.1%–52.0%, omissions ranged from 5.3%–50.0%, and substitutions ranged from 0.6%–19.8%. On bound morphemes substitutions ranged from 0.8%–24.2%. Similar variation was observed in other analytic categories established by Miceli et al., who conclude that "the patterns of variation are so large that it is difficult to imagine what could be gained by considering the patients included in the sample as all having a common functional lesion at some level of language processing" (p. 471).

I tend to think that Miceli's (and **Cappa et al.**'s) conclusions are quite hasty. As pointed out elsewhere (Grodzinsky 1991), the variation on the measures taken leads to one of two possible conclusions: either the group was not homogeneous or the scores that were compared were unrelated to the principles that group the patients. Miceli et al. choose the first possibility, and ignore the second. Not a single sentence in their article discusses the relevance of error rates to the issue of patient classification (nor is there justification for the choice of analytic categories). Yet to be convinced that the group is indeed heterogeneous, the diagnostic and theoretical relevance of the measures used must be demonstrated. This is where the argument falls apart. In all probability, numerical values of proportions of errors in speech production reflect variation in severity of the disorder, but in no way bear on the description of agrammatism in Broca's aphasia – whether clinical or theory-based. Moreover, there are very good reasons to think that the group in Miceli et al.'s study *is* homogeneous. All the patients made errors along the same grammatical dimensions, varying only in rates. These striking similarities thus lead to the opposite conclusion: the group is homogeneous; the syndrome is uniform, although it manifests at different degrees of severity.

A careful analysis of Miceli et al.'s data yields another important nugget. As was later found, their paper shows that the focus on differences among the patients diverted the authors' attention from an additional important commonality; their patients made many errors in tense, but hardly any in agreement, which was one of the cross-linguistic clues that motivated the TPH (Friedmann 1994; Friedmann & Grodzinsky 1997). So **Young & Hutchinson** are right: we should approach a patient as a whole and not ignore individual differences. This should certainly be the case in the context of potential therapeutic measures. A scientific theory of brain/language relations, however, tries to state precise, yet abstract generalizations over a broad range of carefully selected results.

Criticism of a somewhat similar nature comes from **Berndt** and **Caplan.** In this case, variation in comprehension is the issue. Sadly, the focus is on the active/passive contrast. This is far from being the first imaginable contrast one should study, yet the history of this field almost forces us to dwell on it, as there are more data on it than on any other structural contrast. This has enabled Berndt and her colleagues to carry out a retrospective survey in which differences were found.

Berndt et al.'s (1996) survey was not based on a careful selection of studies; included among others, was Goodglass et al.'s (1993) study, in which all seven Broca's aphasics are at 100% level on passives. This study is also cited by several other commentators here (**Caplan, Hickok,** and others), as a counterexample to the TDH. A more careful reading of this study undertaken to get at the source of variation, reveals the reason for this remarkable result. This sentence-to-picture matching test asked the patients to select one of two pictures per sentence. Yet, unlike most studies of this type, in which the "foil," or mismatch, picture reverses the ϑ-roles, Goodglass et al. presented a set of foils that was not related to ϑ-roles at all. Not surprisingly, then, the task did not interact with the patients' deficit, and perfect performance followed. As Grodzinsky et al. (1999) show, and as I repeat in the target article, once the studies are more carefully selected, some of these differences disappear. Other differences are understood once the results are reanalyzed. Performance on actives – as gleaned through an examination of 42 patients – is well above chance, whereas the passives yield a binomial distribution with a mean around 50%, to match the expectation of chance performance precisely.

This is not the end of the story, however. We are also accused of doctoring the data by preselecting just those patients who fit our pattern. **Berndt**'s commentary reiterates the points made in Berndt and Caramazza (1999), who argue that there is no regularity in the data, and that the regularities Grodzinsky et al. (1999) discovered are the result of having a selection of patients that was tailor-made to obtain our results. Berndt and Caramazza's critique of our diagnostic methods is shown to be unwarranted by Zurif and Piñango (1999). Yet, because Berndt and Caramazza claim (offering no empirical demonstration) that statistical regularity does not exist in "real," "un-doctored" data sets, Drai and Grodzinsky (1999) took their claim seriously and examined the data set that they themselves relied on, namely, that presented in Berndt et al. (1996). What was found should come as no big surprise: The original data set gives rise to the same distinctions that the cleaned-up data set shows, as can be seen through a comparison between the two graphs that contrast actives and passives (Fig. R2).

Moreover, the data in both graphs have several regularities that can be expressed in familiar statistical language – convexity of passive curve versus concave active; different means for the two sentence types; vastly different likelihood of giving a correct answer (2:1 for actives in Berndt et al., 4:1 in ours); reversed likelihood of being at chance in active versus passive (2:1 in Berndt et al., 3:1 in ours, see Drai & Grodzinsky, 1999, for detailed analyses).

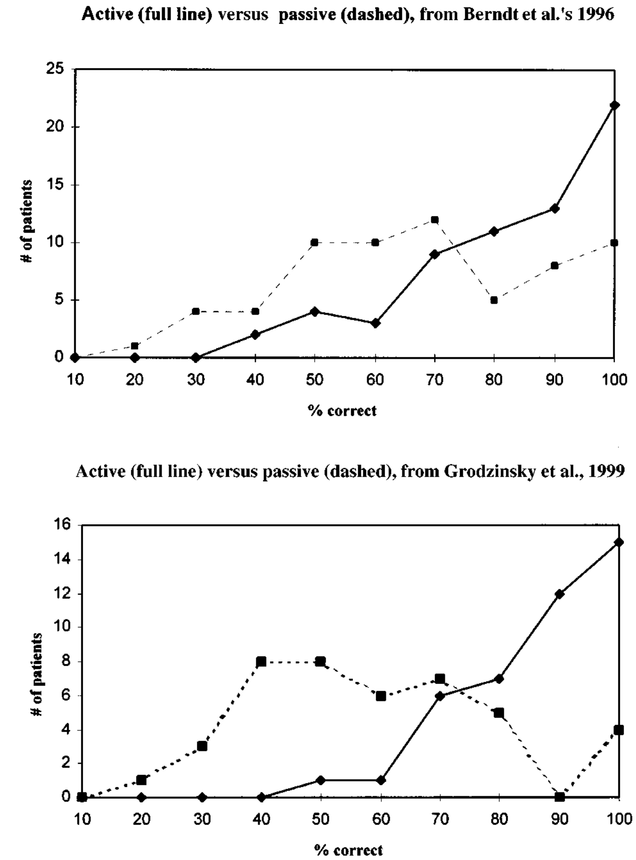


Figure R2. Comparison between two graphs that contrast actives and passives.

Careful scrutiny of the two data sets again reveals a striking similarity that seems to have eluded Berndt's examination. If your main focus is differences, you are very likely to let important similarities go unnoticed, and to miss important generalizations. These are apparent in both data sets. Yet Grodzinsky et al.'s (1999) analysis is based on more rigorous clinical diagnostic procedures, whose lesion localizing value is higher. It presents a more focused picture, indicating once more that properly diagnosed Broca's aphasia correlates strongly with a syntactically selective comprehension deficit. Even an inaccurate selection and arrangement of data, such as Berndt et al.'s, has most of the relevant properties, however, because it contains enough Broca's aphasics exhibiting a reasonably clear picture; Berndt and her colleagues deny this without providing empirical support. Thus there is no more unexplained individual variation in aphasia than in other fields of neuroscience (cf. Friston, 1999, for an interesting discussion of variation in fMRI results, and of the way this variation should be treated).

R5.3. More on localization: Aphasia and neuroimaging

Several commentators point out that Broca's area is involved in more than syntax, with empirical arguments based mostly on functional neuroimaging. Here, evidence must be examined most carefully. As Stefano Cappa (personal communication) has pointed out, we must distinguish between *critical involvement* and *mere participation* of a cerebral region in the processing of information types. A localizing claim therefore needs more than just an observation that an area is activated during a task, as many investigators realize (e.g., Thompson-Schill et al. 1998) and contrary to **Levelt**'s optimistic opinion that only functional imaging will provide a "critical test." By contrast, error analysis on the basis of lesion studies, despite all is problems allows (if done carefully) for localizing claims (gross as these may be) to be made more readily, because it identifies areas whose involvement is a precondition to normal functioning.

Some commentators suggest that Broca's area is involved not only in syntax, but also in phonological processing (**Cappa et al., Friederici & von Cramon, Ullman & Izvorski**). It is likely that many areas take part in phonological analysis, yet when the tasks used in these studies are examined, it appears likely that they probed working memory, rather than phonological representations (as some have acknowledged, cf. Demonet et al. 1996). If true, this can be made consistent with claims regarding working memory and the left frontal lobe (cf. Smith & Jonides, 1997, for a recent review). Again, a lot remains to be shown, but if working memory indeed depends critically on the same parts of the left frontal lobe that are involved in syntactic transformations, this region might have more than one role. But as tempting as a single generalization over the phonological, syntactic, and memory-related findings might be, it is hard to imagine how the processing of intrasentential dependencies (and the constraints involved) could be derived from working memory alone. Syntax, then, remains the sole combinatorial process housed in Broca's area and its vicinity.

Some commentaries (**Cappa et al., Dick & Bates, Dronkers, Müller**) mention the lexicon as another cognitive function related to Broca's area. This is correct, yet here, too, a localizing claim is hard to establish solely on the basis of functional imaging in nonbrain-damaged populations. A combined look at lesion studies and normal functional imaging may provide a larger body of evidence for detailed scrutiny. Commentaries cite Fiez et al. (1996b), Martin et al. (1995), Perani et al. (1999), and others for lexical representation in this region. Cappa et al. are careful, claiming only that BA 45 (pars triangularis) "seems to be related to semantic access both from words and pictures." Others rush to ascribe many functions – linguistic as well as motor – to this area. The data, however, present an interestingly complicated picture, one that eludes simple generalizations. The scope of this response makes a complete review impossible, but one example will make my point clear.

Consider the lexical domain, whose underlying mechanisms are claimed by some commentators to be housed in Broca's area. Findings from neuroimaging (e.g., Fiez et al. 1996b; Perani et al. 1999) indicate left frontal involvement in lexically related tasks (although even this statement is a vast oversimplification). In aphasia, however, the picture is reversed: Posterior lesions affect lexical abilities much more than anterior ones (cf. Shapiro et al. 1993 and Shapiro & Levine 1990, for evidence regarding the intactness of the lexicon in Broca's aphasia). In naming and identification tasks, as well, Broca's aphasics are superior to Wernicke's and Anomic aphasics. Once the findings from both domains are juxtaposed, the complexity of the data becomes apparent, and calls for delicate handling.

Finally, there has been a recent surge in neuroimaging studies of sentences, which were not available at the time the target article was written, and were cited by some (e.g., **Friederici & von Cramon, Hickok, Stowe**). My personal involvement with fMRI has forced me to grapple with new questions regarding the design and interpretation of experiments carried out with this technology. Specifically, I have been looking into the most recent studies of language, which divide into two groups: unstructured and structured analyses of sentence processing. The former involve investigations of stories or "connected prose" with gross contrasts, whereas the latter contrast syntactic properties. The two types of studies indeed diverge, when areas of activation are studied: The picture that emerges from imaging during exposure to stories is vague, as different regions are activated in different studies. In structured studies of sentence processing, however, the frontal operculum is centrally involved in every study, despite varied methodological and design problems from which each may suffer. The difference between the two types of study is not surprising. Consider the story-type paradigm (e.g., Nakai et al. 1999; Schlosser et al. 1998). In these experiments, short texts that contrast known with unknown languages are presented to the subjects. The localizing information in these studies comes from subtractions of activations during exposure to stimuli in a known language (e.g., English) from those in an unknown one (e.g., Hungarian). Because the unit of analysis is so gross (i.e., native vs. unknown language), potential interpretations of such contrasts are not easily made. Indeed, a perspective that analyzes sentences into their component parts finds such designs uninterpretable. As for the sentence-level experiments, **Levelt** mentions two recent studies, which in his opinion speak against the TDH. He cites Caplan et al. (1998) as one of these, yet this study used the same materials as the Stromswold et al. (1996) study, and is hence subject to the same critique (cf. sect. 2.5 in the target article). Another study Levelt cites (Meyer et al. 1999) contrasted actives and passives that contained nonsense words. It is not clear how this study can be evaluated, especially because structure building in this case depends so strongly on lexical properties. Some other new work (e.g., Caplan et al. 1999) has compared activation during listening to sentences of different types. The contrasts in this case are similar to the Stromswold study cited in the target article. The authors (as well as **Müller** who echoes the same point) have viewed their stimuli as ordered by complexity, for which no metric is given. A perusal of the stimuli (for this plausibility-judgment experiment) shows that they are subject- and object-cleft sentences:

(13) a. It was the child that enjoyed the juice
b. It was the child that the juice enjoyed

These sentences do not fall within any known complexity metric, but they do provide a structural contrast similar to that found in subject- and object-relative clauses, for which data from aphasia are available, consistent with the TDH. As I already noted, a clear mapping is necessary for this (and any other) hypothesis to make a precise prediction for neuroimaging, but I cannot imagine how these findings would be inconsistent with the TDH.

Concluding, then, it is true that many aspects of language (and other cognitive abilities, like memory) may occasionally involve the frontal language areas. It is important to note, though, that only studies of syntactic processing give a stable and consistent picture in which the central area to light up during the processing of syntactic movement is Broca's area. In other tasks (phonological, lexical, semantic) this area may be involved, but the lack of consistency and stability within studies, and the fact that this region is one among several to be activated, provide further support for the claim that the critical role of Broca's area and its vicinity is the processing of movement.

R5.4. Some residual issues

Commentators have raised some additional points, mostly regarding additional experimental results. I discuss these briefly.

R5.4.1. The R-strategy. In an experiment conducted by **Beretta,** patients presumably did not follow what he takes to be a prediction of the TDH: As passives are said to have two agents, he expected patients to point to pictures that corresponded to such an interpretation, rather than to the standard set. Patients failed to meet this expectation, leading Beretta to conclude that the TDH is false. This could be true, with three reservations: (1) The test materials were flawed – there was always a third actor in the pictures; indeed, scores for actives in this condition, expected to be normal, were unusually low. (2) The truth of Beretta's prediction is contingent on the nature of M, the mapping function, which Beretta failed to provide. Therefore, an evaluation of his claims awaits a specific claim. (3) Even if the requirement in (2) is satisfied, a falsification of the TDH is still far off, pending an alternative interpretation of the available data set (which includes, among many other things, Beretta et al.'s [1996] data from Spanish, which support the TDH).

R5.4.2. Japanese adversative passive. The experiment in Japanese that tested adversative ("indirect") passive is questioned by **Otsu,** who suspects that they were "giveaways," because the materials were not chosen correctly. An examination of this issue was made, indicating that Otsu is right (Hiroko Hagiwara, personal communication), and the materials for this condition were chosen incorrectly. A new, improved experiment is currently under way. This small error does not, however, undermine the overall picture of the Japanese data. Most important, the contrast between scrambled and unscrambled activities is still valid.

R5.4.3. Failures in main clauses. Broca's aphasics fail to compute the proper relationship between a predicate adjective and its subject. **Kolk & Hartsuiker** and **Hickok** take this as evidence against the TDH. Linguistic considerations point precisely to the opposite conclusion, however: If the ISH is correct, then subjects move out of VPs to receive an agent ϑ-role by the strategy. Sentences with predicate adjectives, however, can never take agents. The discrepancy leads to chance performance.

R5.4.4. The best control. Conduction aphasia is the right control case for Broca's aphasia, **Berndt** proposes, rather than the standardly used Wernicke's. She may be right, and if so, it would once again be a case where our current scientific practice falls victim to our own history. However, a claim like that needs to be demonstrated, not merely asserted.

R6. Fantasies on the final theory

For years now the study of brain/language relations has witnessed a split between neuroscientists, neurologists, and neuropsychologists who engage in brain talk, and linguists who do language talk. Sadly, there has been insufficient cross-talk. Future neurolinguistic theory will close the gap, and will transcend the current distinction between neuroscience and cognition. It will keep generalizations that linguistics offers, but specify how these are implemented in neural tissue. Mutual constraints will be formulated, to regulate the complex relations between language knowledge and the working brain. Currently tenuous connections will become strong, explicit, and clear, as will our understanding of what it means to be a living language user.

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Letters "a" and "r" before authors' initials refer to target article and response, respectively.

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