> tities in consciousness, they can serve as the focus of attention, which permits higher-power-processing, anchoring, and, perhaps most important, retrievable storage of these otherwise nonperceptible elements. (Jackendoff 1996a, p. 27)

This way of language *helping* thought seems to be compatible with phenomenology:

> In particular, in speaking, one's choice of words at the beginning of a sentence may by feedback refine the formulation of subsequent parts of the thought; one's choice of a syntactic structure for realizing the words affects the order in which the rest of the thought must be refined . . . As the expression of the thought reaches conscious form (in my theory, phonological structure), one can "hear" it as "inner language" in advance of uttering it, and quickly re-evaluate it, revise it, or repair it before producing it publicly. This is experienced as "finding out what one is thinking by trying to say it." It is also possible at this point for one to discover that an utterance is "not exactly what one meant." (Jackendoff 1996b, p. 204)[3]

On the other hand, Jackendoff's framework liberates narrow syntax from the burden of having to account for the richness of thought. All semantic distinctions that are reflected in grammar (morphology, syntax, and phonology) are carried out by *mappings* between different levels (which may vary between languages). We believe such an architecture is highly adaptable to future evidence on how language might affect thought. It is also compatible with the idea that learning vocabulary and grammar (i.e., mappings between phonology, syntax, and meaning) might shape the "inner conceptual landscape" in a manner that differs substantially from cognitive systems that lack such devices. As Spelke put it:

> Natural languages have a magical property. Once a speaker has learned the terms of a language and the rules by which those terms combine, she can represent the meanings of all grammatical combinations of those terms *without further learning. The compositional semantics of natural languages* allows speakers to know the meanings of new wholes from the meanings of their parts. (Spelke, 2003, p. 306, emphasis added)

Jackendoff's ideas seem to run along these lines, with the exception (I believe) that what Spelke calls the compositional semantics of *natural language* would be called the compositional or combinatorial character of *thought* in Jackendoff's framework, and the achievements mentioned are made not *by* language but *with the help of* language, that is, with the *help* that a lexicon – and the possibility of mapping conceptual structures onto syntactic and phonological (conscious) structures – provides in terms of *anchoring, manipulation, and explicitness.*

Finally, some remarks on Jackendoff's methodology. Although one may not agree with everything he says, his manner of theorizing has one undeniably rare quality: The reader will always understand what is being said. His concepts are well defined and troublesome issues are left open rather than being artificially "solved." I believe that explicitness and clarity are an important part of what we call science. Nature is full of patterns, the mind/brain is a sort of pattern processing device, and thus, when humans begin to speak *clearly* about something, suddenly, voilà! – you have the makings of science. Besides its original ideas on language and cognition, and its impressive integrative power, I see *Foundations* as a tremendous lesson on scientific discourse.

NOTES

1. Carruthers's proposals are at least problematic: How does an account based solely on domain-specific modules and LFs deal with the complexities of "language production," for which it has been necessary to postulate non-verbal processes such as "macroplanning" and "microplanning"? (Molina 2002). On the other hand, how does this account deal with the fact that we can have bare or wordless concepts (i.e., concepts that do not have a word associated with them), such as "the pathetic strands of hair that some men drape carefully but ineffectively over their bald spots" (Dennett 1998, p. 286) or "the moist residue left on a window after a dog presses its nose to it" (Murphy 2003, p. 389)?

2. For Jackendoff's concept of consciousness see Jackendoff (1987; 1997, Ch. 8).

3. I am, however, somewhat uncomfortable with the idea that in language production, "feedback and attention [are] not possible until there is a conscious phonological structure available" (Jackendoff 1996b, p. 205). This is because it is stated that in language production, besides being capable of monitoring the phonology, syntax, and semantics of the sentences that *reach* our inner speech, it also appears to be possible to monitor *the construction of the preverbal message,* for which no overt conscious clues are still available. In other words, it appears that the speaker can directly monitor the preverbal messages he is preparing to express, and he may reject a message *before* its formulation has started. As Levelt puts it:

> The speaker no doubt also monitors messages *before* they are sent into the formulator, considering whether they will have the intended effect in view of the present state of the discourse and the knowledge shared with the interlocutors . . . The main work is done by the Conceptualizer, which can *attend* to *internally generated messages* and to the output of the speech-Comprehension System." (Levelt 1989, p. 14, emphasis added)

What kind of "unconscious" monitoring would this be? Would it be part of what could be called the "dynamic of thought"?

Grammar and brain

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Abstract: Jackendoff's account of relating linguistic structure and brain structure is too restricted in concentrating on formal features of computational requirements, neglecting the achievements of various types of neuroscientific modelling. My own approaches to neuronal models of syntactic organization show how these requirements could be met. The book's lack of discussion of a sound philosophy of the relation is briefly mentioned.

I agree with Jackendoff (2002) on the main principles outlined in *Foundations of Language: Brain, Meaning, Grammar, Evolution:* The discussion of the foundations of language should be based on considerations of the brain's neural circuitry along with its functional properties, on a "two way street" (p. 22); strict reductionism and autonomous functionalism are inappropriate. The challenges to cognitive neuroscience presented in Chapter 3, section 3.5, and the list of basic questions on pp. 422–23, are well selected.

I disagree on the following points: (1) It is not true that *only* structures built of (formal symbolic) discrete combinatorial units (p. 423) can explain the productivity of language (pp. 38–39). (2) The competing design feature of "brain style modelling" is inappropriately characterized by mere reference to a few models (p. 23). (3) It is not correct that "we don't even have an *idea* of how a single speech sound such as /p/ – much less a category like NP – is instantiated in neural firings or synaptic connections" (see below). (4) A book on the foundations of language should find some place for basic philosophical and methodological discussion, and not merely presuppose standards (Cartesianism, the formal view of axiomatization). (For a contrasting Leibnizean view, see Schnelle [1991a; 1991b, Part III; 1996b], and, for an intuitionistic computational foundation, Schnelle [1988].)

My specific critique will elaborate point 3:

1. The possibility of an analysis based on active and interactive feature representation units in terms of neuronal groups, columns, and modules is briefly mentioned in the author's reference to Hubel and Wiesel 1968 (see *Foundations,* p. 23). However, the author disregards its important role for the representation of actively interactive features in the Jakobsonian sense (Schnelle 1997) and their fruitful analyses by Szentagothai, Mountcastle, Arbib, and others (cf. Arbib & Erdi 2000, Schnelle 1980; 1981), as well as the related computational Theory of Neuronal Group Selection of Edelman (1987, and his subsequent books).

2. The author also completely neglects neuroanatomic and

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neuropsychological approaches relevant for language. (For an overview, see Schnelle 1996a.)

3. Jackendoff concentrates on computational problems. His list of eight critical questions about how language is "lodged in the brain" (p. 422) and his four challenges for cognitive neuroscience (p. 58) are very much to the point. His main question is whether and how essential properties of computational combinatoric theories of grammar could be instantiated in active unit interaction networks, that is, in "brain-style modelling." He deplores that to his knowledge these questions and challenges "have not been widely recognized in the cognitive neuroscience community" (p. 58). Unfortunately, the author doesn't seem to have investigated this with care. Among others, my own approaches since 1979, and those of my young colleagues, have addressed precisely these questions in many publications, including two books. Let me briefly explain.

4. The first question is basic: "How are discrete structural units (such as phonological features and syntactic categories) instantiated in the nervous system?" My answer, following the basic idea of Jakobson (cf. Schnelle 1997), is: Each feature and each category is represented by a group of (hundreds of) neurons placed in a genetically prestructured internal network (like a Szentagothai column) and connected by inhibitory and excitatory connections to other group networks (i.e., representations of other features or categories). (For the computational technicalities, see my articles in *Theoretical Linguistics* during the eighties, but mainly Schnelle & Doust 1992, Wilkens & Schnelle 1990, and the computationally elaborate book of Wilkens 1997.) As a result of this external interaction with other groups, each group participates at each time in a distributed network of currently interactive "features." In the simplest cases (those of nonsyntactic patterns) the binding procedures often discussed by neuroscientists are sufficient in building a synchronous activity pattern.

5. In syntactic processes, the situation is different. The computational details can be studied in my publications. Here I emphasize their neurobiological organization. Each group or column representing a syntactic feature contains a subset of neurons functioning as a component of the distributed working memory. Their current activity states – stable for a time span of a few seconds – mark the activation status of the feature use as being either inchoate, in process, or successfully terminated. At the same time, other neurons of the same group indicate how often a given syntactic process for a sentence has made use of the same syntactic category represented by the group. The interdependencies of these working memory neurons also mark the temporal sequence of these uses. In other words, the collection of the working memory neurons of the syntactic category group gives the group the power of an activity unit functioning like a pushdown store with storage capacity in the range of seconds. In theory, the limitations to seconds and finite sets of neurons could be neglected. This would give the system the capacity of a Turing machine.

6. Because this organization represents a syntactic category (by connected working memory marks of complex activation states), a category occurring several times in a syntactic structure is not stored as many times, but rather, is marked by a storage pattern of working memory neurons, a subset of the group. Groups of this marked type occur only in certain areas of the brain, for example, in sulci of frontal areas (such as Broca's) and of the superior temporal sulcus. Both are involved in syntactic and lexical language processing.

7. In this way, hierarchical structures are not represented as composed from passive units but as distributions of syntactic categories' modules marked by their sequence of activation. Thus, working memory does not store but rather, distributively marks, acts of category involvement.

8. The binding problem and the problem of "brain-style modelling" are easily solved by the appropriate connectivity of the syntactic category representation groups.

9. The solution to the problem of variables is solved by the fact that distant areas in the brain are connected by bundles of axons where each axon activates many distant groups (i.e., a class of representations for lexeme units, each being represented like a convergence unit, à la Damasio). Only those that have an appropriate state of (lexemic) pre-activation combine the "request" signal with pre-activation to generate actual activation.

Thus, I believe I have provided technically and empirically possible answers to the critical questions and challenges listed in the book.

Rescuing generative linguistics: Too little, too late?

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Abstract: Jackendoff's *Foundations of Language: Brain, Meaning, Grammar, Evolution* attempts to reconnect generative linguistics to the rest of cognitive science. However, by minimally acknowledging decades of work in cognitive linguistics, treating dynamical systems approaches somewhat dismissively, and clinging to certain fundamental dogma while revising others, he clearly risks satisfying no one by *almost* pleasing everyone.

Jackendoff (2002) promises integration. He vows "open-mindedness to insights from whatever quarter, a willingness to recognize tensions among apparently competing insights, and a joint commitment to fight fair in the interests of deeper understanding" (p. xiii). Yet the most long-standing opposition to generative linguistics, the cognitive linguistics paradigm, and its key insight that linguistic structure is not separable from meaning, receives scant recognition in *Foundations.* In fact, quite a few "tensions and competing insights" are actually given little more than lip service (frequently relegated to footnotes) throughout the book. Jackendoff regularly acknowledges that generative linguistics has made a great many problems for itself, both ideologically and empirically, but insists on maintaining several of its core principles, adding a great many software patches, as it were, and keeping the name. With this book, Jackendoff has, perhaps despite his best intentions, allied himself more than ever before with a program whose rhetorical high-ground and untouchability have faded (indeed, he recounts much of that fading process himself), and whose time may be running out. Indeed, it is somewhat surprising that Jackendoff, a frequent challenger of certain aspects of the mainstream doctrine, would be the one to organize a rescue attempt of the generative linguistics expedition (which has arguably been stranded at its own "Donner Pass" for some time now).

Early on, Jackendoff reduces the competence/performance distinction to a "soft methodological" separation (which, with his addition of "neural instantiation," begins to resemble Marr's [1982] tripartite division of levels of analysis, with crosstalk encouraged). With this subtle revision, he manages to reify the notion of linguistic competence at the same time that he takes away a valuable and convenient defense mechanism that generative linguistics has relied on for decades. He also lists numerous criticisms of the "software versus hardware" analogy for "mind versus brain" (which has so frequently been used as an excuse to ignore neurophysiology), but somehow still manages to refer to it as "a robust analogy." These, and many similar instances, are clearly the wafflings of a torn scientist who senses the future but cannot let go of the past.

For example, Jackendoff's approach to morphological productivity, the "remembering" of idioms such as "he kicked the bucket" and their morphological architecture (Ch. 6) would be a perfect place for a merger between cognitive linguistics and generative linguistics. However, what he instead presents are syntactic problematizations of the issues. He argues to his chagrin that there must be two kinds of rules at play, those that are fully productive

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