paraphrased or diagrammed, environmental antecedents should be incorporated into those forms.

But commentary would be only of historical interest if it were just that Jackendoff has developed a system whose features Skinner had anticipated. It is more important that the behavioral stance has since expanded to new topics that must be taken into account. For example, Skinner hinted at how multiple causation can yield productivity: "We turn now to a different type of multiple control, in which functional relations, established separately, combine possibly for the first time upon a given occasion" (Skinner 1957, p. 229). But he did not go far enough. Experimental studies have since addressed the spontaneous coming together of responses learned separately, in the phenomenon called adduction (e.g., Catania et al. 2000; Esper 1973; Johnson & Layng 1992). Shaping is another source of novel behavior, and variability itself can be selected (Neuringer 2002; Pryor et al. 1969). Higher order classes provide still another source (Catania 1995a; 1996a), illustrated by generalized imitation, as when a child imitates an action never before seen or imitated (Baer et al. 1967; Gewirtz & Stingle 1968; Poulson & Kymissis 1988; Poulson et al. 1991). Other higher order examples are those of equivalence classes, in which new behavior emerges from reflexivity, symmetry, and transitivity relations among the members of stimulus sets (Catania et al. 1989; D'Amato et al. 1985; Dube et al. 1993). These relations cannot be derived from stimulus properties, and so can only be dealt with in terms of the environmental contingencies that created them (Catania 1996b; Vaughan 1988). They are of particular relevance for interpreting relations among words and other events (in other words, meanings), and provide an easy bridge to many hierarchical structures discussed by Jackendoff.

Other extensions grounded in experimental findings are to the roles of echoic behavior and of responses to pointing in the development of naming in children (Horne & Lowe 1996), functional effects of naming (Wright et al. 1990), developmental transitions from nonverbal to nonverbal behavior (Bentall & Lowe 1987; Bentall et al. 1985; Moerk 1992), the shaping of verbal behavior and correlated changes in subsequent nonverbal responding in verbal governance (Catania, 2003; Catania et al. 1982; 1990; Chadwick et al. 1994; Greenspoon 1955; Lovaas 1964; Rosenfarb et al. 1992; Shimoff & Catania 1998; Skinner 1969), and ways in which verbal governance depends on differential attention to different kinds of verbal stimuli, as when the bringer of bad news is poorly received (Dinsmoor 1983).

Jackendoff has offered "an open-mindedness to insights from whatever quarter" (p. xiii) and has asked for "all the help we can get from every possible quarter" (p. 429), so my hope is that the news offered here in return will not be poorly received. The behavioral bathwater is gone but the baby has thrived and is ready to rejoin the company of linguists to help them with their work.

NOTE

1. Unless otherwise noted, pages refer to Jackendoff (2002).

"Grammar box" in the brain

Valéria Csépe

Department of Psychophysiology, Group of Developmental Psychophysiology Research Institute for Psychology of the Hungarian Academy of Sciences, Budapest, H-1068 Hungary. **csepe@cogpsyphy.hu**
http://humlab.cogpsyphy.hu/humlabe.htm

Abstract: Brain activity data prove the existence of qualitatively different structures in the brain. However, the question is whether the human brain acts as linguists assume in their models. The modular architecture of grammar that has been claimed by many linguists raises some empirical questions. One of the main questions is whether the threefold abstract partition of language (into syntactic, phonological, and semantic domains) has distinct neural correlates.

There is a growing number of data-giving evidence on brain specialization for language, although many language processes, in spite of their distinct function in the architecture, cannot be localized to just one particular area of the brain. However, as we know from brain measures and especially from brain-imaging data, one particular area or part of the network is involved in different tasks, and there is a spatial and temporal overlapping of the processes. Brain-activity data seem to prove the existence of qualitatively different structures in the brain processing phonological, syntactic, and semantic information. However, the question is whether the human brain acts as linguists assume it does in their models.

Jackendoff has many well-elaborated questions about the nervous system serving language functions, eight of them listed in his concluding remarks (pp. 422–23). His questions will attract the attention of neuroscientists, as Chomsky's concept of Universal Grammar has given place to discussions and studies on relating abstract entities with physiological correlates. According to Jackendoff's statement, Universal Grammar is a limited set of "attractor" structures that guides language acquisition through inheritance. However, the question is what do we mean with inheritance, innateness, and wiring, when referring to the biological relevance of Jackendoff's reconfigured generative grammar.

New findings in genetics further strengthen the belief that language is specified by biological factors. The recent discovery of the FOXP2 gene (Lai et al. 2001) supports the assumption of linguists that the development of language is set by innate factors. As revealed by the data of Cecilia Lai and her coworkers, a mutant version of the FOXP2 within chromosome 7 provokes Specific Language Impairment (SLI). However, the FOXP2 data may irritate some linguists rather than satisfy them, because SLI is a heterogeneous class of verbal disturbances and does not correspond to a single domain of rule applications. Therefore, I think, Jackendoff is correct when he refers to a language toolkit, and assumes innate capacities instead of a language system lodged in the brain.

The modular architecture of grammar claimed by many linguists raises some empirical questions. One of the main questions is whether the threefold abstract partition of language (into syntactic, phonological, and semantic domains) has distinct neural correlates. There are experimental data that prove semantic information has a distinct representation in the brain. Another fundamental question is whether syntactic processing is associated with dedicated neural networks. Syntactic processing during sentence reading has been investigated in several functional neuroimaging studies and showed consistent activation of the *pars opercularis* of Broca's area (Caplan et al. 1998; Just et al. 1996). However, sentences presented in the auditory modality (Caplan et al. 1999) lead to activation of the *pars triangularis.* Moreover, in visual tasks the anterior *cingulate gyrus* and the right *medial frontal gyrus* were activated. This finding was interpreted as a correlate of phonological encoding and subvocal rehearsal. A current study by Newman et al. (2003) adds further empirical evidence to partly distinct networks specialized for syntactic and semantic processing. Their fMRI data suggest that separable subregions of the Broca's area contribute to thematic and syntactic processing. In their study, the *pars triangularis* was more involved in thematic processing and the *pars opercularis* in syntactic processing.

Dapretto and Bookheimer (1999) tried to separate the syntactic and lexicosemantic processing in an fMRI experiment. In the semantic condition single words, in the syntactic condition full sequences, were changed. The authors used passive constructions for syntactic change; and, I am sure Jackendoff would argue, passive constructions do not necessarily preserve the semantic content of their active counterpart. In spite of the assumed semantic change in the passive construction, Dapretto and Bookheimer (1999) found activation in the Broca's *pars opercularis.* In a recent study, Moro et al. (2001) applied syntactic, morphosyntactic, and phonotactic tasks for "pseudosentences" and found activation in the Broca's area *pars opercularis* and in the right inferior frontal region during syntactic and morphosyntactic processing. A local network shared by morphological and syntactic computations

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proves that syntax and morphosyntax are closely related in the brain, as it is assumed in the model of modern architecture of language proposed by Jackendoff (p. 261). However, this does not mean that syntactic capacities are implemented in a single area.

The lack of complete overlap of brain areas involved in syntactic and morphosyntactic processing is in agreement with most of the linguistic models. It must be underlined, however, that the role of working memory in syntactic processing is more or less ignored by the linguistic models. It seems to be "understandable" if we take into account the complexity of the relationship of working memory and sentence comprehension. Working memory may play a different role in assigning the syntactic structure of a sentence, and in using this structure to determine the meaning of it. The complex relationship of syntactic complexity and working memory load is proven by patients' data. Pickett et al. (1998) report on a patient with mild Parkinsonism who showed perseverations in rule applications, impaired comprehension in sentence meaning conveyed by syntax, and intact verbal and visual short-term memory. The striking dissociation shown by the patient was that her sentence-comprehension performance increased proportionally with syntactic complexity. We may assume that the most probable areas playing a crucial role in such a memory-syntax interface are frontal regions of the cortex.

Jackendoff mentions the possible role of working memory (WM) in language processes several times in his book and his most elaborate remarks are related to the distinction between Baddeley's WM model and his own linguistic working-memory concept. I agree working memory is not just a "shelf where the brain stores material" (p. 207), but also a workbench that has a complex relationship with constructing verbal structures. From this point of view, Baddeley's model has a limited capacity in explaining the relationship between WM and the integrative and interface processes.

However, a different model of working memory from Just and Carpenter (1992) may fit better with Jackendoff's parallel grammar model. In the Just and Carpenter model of functional working memory, henceforth referred to as f-WM, storage is defined as temporal retention of verbal information already processed, while processing is defined as computations generating various types of linguistic representations (lexical, morphological, grammatical). In one of the f-WM studies by Montgomery (2000), the relation of WM and immediate processing of simple sentence structures was investigated in SLI children and two control groups, age matched and receptive syntax-matched controls. The SLI group showed deficits in all f-WM tasks and was very slow as compared to the control groups. However, immediate processing of simple sentences does not rely heavily on f-WM resources, so the problem may be more related to integrating the resources associated with different subsystems of the linguistic working memory.

Given the distinctions between Baddeley's WM model and the f-WM model we may assume that the f-WM model is closer to Jackendoff's assumption on linguistic working memory than to Baddeley's previous or recent models (Baddeley 2003). The Just and Carpenter model assumes that items activated in the working memory are integrated into larger chunks. The model is not far from that of Jackendoff's idea on the linguistic working memory included in the parallel grammar that heavily relies on item integration. The task of neuroscience would be to shed light on possible neural functions related to the subsystems assumed. If Jackendoff is right about the integrative function of linguistic working memory as an inherent part of the three linguistic structures, brain activity correlates should be associated with it. It is really mysterious how the items retrieved from long-term memory undergo transient processing in working memory and how they are related to brain mechanisms. However, I do think that the problem is that we haven't yet found the right experimental paradigms for investigating these processes.

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Beyond beanbag semantics

Daniel C. Dennett
Center for Cognitive Studies, Tufts University, Medford, MA 02155.
ddennett@tufts.edu **http://ase.tufts.edu/cogstud/**

Abstract: Jackendoff's "mentalistic" semantics looks more radical than it is. It can best be understood as a necessary corrective to the traditional oversimplification that holds that psychological variation "cancels out" on the path from word to world. This reform parallels the "evo-devo" reform in evolutionary biology.

Mendel's genes were a brilliant simplification that permitted many of the fundamental principles and constraints of inheritance to be clearly described and tested. But if you took them too literally, imagining them to have exact counterparts lined up like simple beads strung on the chromosomes, you got "beanbag genetics," as Ernst Mayr once dismissively called it. The working parts of the DNA inheritance machinery encountered in contemporary molecular genetics are so much more subtle and active than Mendelian genes, that some would declare that genes – the genes Mendel introduced to us – do not exist at all! *Eliminative materialism* regarding genes in the Age of Genes? An unlikely terminological reform. We don't throw the Mendelian ladder away; we continue to use it, with due circumspection and allowances (Crow 2001; Haldane 1964).

Jackendoff's masterpiece *Foundations of Language* (Jackendoff 2002) poses a counterpart question: Isn't it time to trade in Chomsky's pathfinding syntactocentric vision for something more complex in some ways and more natural in others? In the syntactocentric picture, a word is a simple, inert sort of thing, *a sound* plus *a meaning* sitting in its pigeonhole in the lexicon waiting to be attached to a twig on a syntactic tree. In Jackendoff's alternative vision, words are active: "little interface rules" (target article, sect. 9.3, para. 6) with lots of attachment prospects, links, constraints, affinities, and so on, carrying many of their combinatorial powers with them. Jackendoff's proposed parallel architecture, with its three simultaneous and semi-autonomous generative processes, is biologically plausible, both neuroscientifically and evolutionarily. It opens up a space for theory modeling in which hypotheses about opponent processes, recurrence, and other sorts of mutual interaction, can be formulated and tested. The Universal Grammar (UG) doesn't need to be written down as rules to be consulted. It is partly embodied in the architecture, and partly fixed by culturally evolved attractors homed-in on by individual learning. The epicycles of syntactocentric theories largely evaporate, as the division of labor between syntax, semantics, and phonology gets re-allotted.

Any revolution is apt to look more outrageous in prospect than it turns out to be in retrospect. I would like to propose a friendly amendment, softening the blow of Jackendoff's "mentalistic" semantics. Semantics, as traditionally conceived by logicians, philosophers, and linguists, is where the rubber meets the road, where language gets all the way to the world and words refer to the things and events therein. The winding path by which a word "gets to" the world, when it does, surely lies in the mind (or brain) of a language user, but tradition has it that this messy intermediary can and should be largely ignored. There are several influential bad arguments as to why this should be so, but here's one that can stand for them all:

"My uncle is suing his stockbroker." When you hear that sentence, and understand it, you perhaps engage in some imagery, picturing an adult male (in a suit?) with some papers in his hand, confronting, somehow, some other man (why a man?), and so on. There would no doubt be wide variation in the imagery in the minds of different hearers, and some might claim that they engaged in no imaging at all and yet still understood the sentence just fine. Moreover, such imagery as people did indulge in would be unable on its own to fix the meaning of the sentence (there is nothing an uncle looks like that distinguishes him from a father or

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