

pour, and *spill*, and between *drench*, *douse*, *soak*, and *saturate* (Pinker 1989; see also Davis 2001; Hale & Keyser 1993; Mohanan & Wee 1999; Rappaport Hovav & Levin 1998).

Jackendoff cites Pinker's (1989) analysis of verb-based constructions approvingly, but he is apparently skeptical of the GRSH. In *Foundations* he states that the hypothesized independent level for grammatically relevant meaning "exhibits no interesting semantic constraints beyond its coarseness relative to lexical distinctions" (p. 290), and he offers the following alternative proposal: "The subset of semantic features relevant to grammar is just the subset that is (or can be) mentioned in phrasal interface rules" the part of conceptualization that is "visible" to these rules? (p. 291).

Now, if grammatically relevant and irrelevant components of meaning are segregated, as the GRSH maintains, then they are probably subserved by at least partially distinct neural structures. Therefore, it should be possible for them to be impaired independently of each other by brain damage. I have been conducting a series of studies with aphasic subjects to test this prediction, and have obtained results that are consistent with it. The first study focused on the locative alternation and revealed the following double dissociation (Kemmerer 2000a). One subject failed a verb-picture matching test that evaluated her ability to discriminate between grammatically irrelevant aspects of verb meanings (e.g., *drip-pour-spill*) but passed a grammaticality judgment test that evaluated her knowledge of the grammatically relevant semantic features that determine which constructions the very same verbs can occur in (e.g., *Sam spilled beer on his pants* vs. **Sam spilled his pants with beer*). In contrast, two other subjects manifested the opposite pattern: They passed the matching test but failed the judgment test. Moreover, their errors on the judgment test were most likely due to grammatical-semantic rather than purely syntactic deficits, because they performed well on a separate test that addressed simple clausal syntax. Three subsequent studies focusing on various constructions found robust one-way dissociations involving subjects who passed tests of grammatically irrelevant meaning but failed tests of grammatical relevant meaning (Kemmerer 2000b; 2003; Kemmerer & Wright 2002; see Breedin & Saffran 1999; Marshall et al. 1996, for additional reports of the reverse type of dissociation; see Druks & Masterson 2003; Shapiro & Caramazza 2002, for other pertinent studies).

Although this research has just begun, the initial findings support the GRSH and challenge Jackendoff's view. It is possible, however, that the two competing positions could eventually be reconciled in the following way. The neural structures that implement grammatical semantics might not be genetically programmed for this function; instead, through as yet unknown mechanisms of self-organization (perhaps like those simulated by Kohonen networks), these structures might become functionally specialized over the course of language development as the child formulates increasingly abstract semantic generalizations over verb classes that are associated with certain morphosyntactic frames. This kind of approach could accommodate not only the neuropsychological data, but also recent typological data on extensive crosslinguistic variation in grammatical semantics (Croft 2001; Haspelmath 2003; Slobin 1997; Zhang 1998), as well as recent psycholinguistic data on the acquisition of grammatical constructions (Tomasello 2003).

Finally, and on a more positive note for Jackendoff, neuroscientific studies strongly support his proposal (p. 350) that certain semantic features of action verbs are not algebraic but rather motoric and visuospatial in character (e.g., Breedin & Saffran 1994; Kable et al. 2002; Kemmerer & Tranel 2003; Pulvermüller et al. 2001; Rizzolatti et al. 2001; Stamenov & Gallese 2002; Tranel et al. 2003).

Interestingly, these semantic features tend to be grammatically irrelevant, a point that Jackendoff recognizes and that deserves closer attention from scholars in both linguistics and cognitive neuroscience.

A mixed treatment of categoricity and regularity: Solutions that don't do justice to a well-exposed complexity

René Joseph Lavie

UMR 7114 Modèles, Dynamiques, Corpus (MODYCO), Université Paris 10 et CNRS, 92000 Nanterre, France. rlavie@waika9.com

Abstract: Jackendoff's position with respect to categories (for lexical items and larger constituents) is unclear. Positing categories is (1) implausible in several respects; (2) it makes the binding problem in language seem more massive than it actually is; and (3) it makes it difficult to explain language acquisition. Waiting for connectionism to fulfill its promise, a different track is sketched which is residually symbolic, exemplarist, and analogy-based.

This commentary bears only on Jackendoff's position on categories in *Foundations of Language* (Jackendoff 2002), although there would be much to say on other subjects. (For example, how is the simplest metonymy to be accounted for with the overly simplistic vision of semantics that is advocated?) I will understand "category" – following conventional usage in linguistics – as lexical categories, grammatical categories (including rules), and functional categories.

While several authors today are giving up categories – or making efforts to that end – *Foundations* takes a position on categories which is not entirely clear to me. On p. 24, speaking about "the theoretical claims" that "words belong to syntactic categories" and that "words group hierarchically into larger constituents that also belong to syntactic categories," Jackendoff reminds us that many different notations (trees, bracketed expressions, boxes) may be used. A possible reading of the passage is that Jackendoff is endorsing the claim itself (besides the variety of notations, there would be, unarguably, a categorical structure). But, in many other places in the book, it is clear that the author takes the necessary distance with respect to categories. However, in Chapter 5 "The parallel architecture," which is central to the definition of Jackendoff's proposal, lexical categories are pervasive in the text; there isn't an explicit statement that they are rejected by this theory, nor is there an explicit statement showing how linguistic phenomenology is to be accounted for *without* categories. In general, the author's statement of the "massiveness of the binding problem" (addressed below in this commentary) can be understood only under the assumption of categories. In short, the book ultimately seems to me to be ambiguous as to whether it endorses lexical categories (then, how would that be compatible with the difficulties that Jackendoff himself raises?), or whether it rejects them (in which case, I am not sure I perceive what theoretical devices are called for, for a precise account of linguistic phenomenology).

In any case, there is a theoretical obstacle to positing categories: that of implausibility, recognized by Jackendoff himself. "It is obvious that speakers don't have a direct counterpart of the symbol NP in their heads" (p. 24).

There is also the obstacle of coping with the linguistic facts. The evidence is abundant, for example, in the decades of work done by Maurice Gross at the University of Paris 7, which showed that in French there are no two verbs with exactly the same distributional behaviour (Gross 1975, p. 214). It may be the case, however, that attaching lexical items to several categories, with multiple inheritance – as proposed in *Foundations* – makes it possible to address the variety of distributional behaviours, but this remains to be shown through detailed work on extensive linguistic data. Still, there would remain problems with plausibility, learnability, and language change.

Constructions, as proposed in *Foundations*, are categorical in the sense that they are abstract, and based on the lexical categories. However, the proposed theory seemingly accepts – as does Goldberg (1995) – as many constructions as wanted, and organizes them into an inheritance lattice (pp. 183–87). This reduces

the categoriality of the theory without nullifying it. No doubt it provides the model with enough flexibility for a faithful synchronic account of a language: Make as many constructions as needed, with as many inheritance links as needed. But the prediction is that it will resist explaining language change and acquisition because the process of modifying a lattice of constructions – to say nothing of just establishing it – can only be a complicated one. The prediction turns out to be true: In pages 189–90, Jackendoff addresses learnability issues; he makes a fair summary of the data on acquisition which is available and concludes that “the gap is still not yet bridged” and that he has not “provided a thorough account of language acquisition.” I come back to this point below.

The issue of reducing categoriality is also at stake, in a way, with the proposition “to blur the distinction between lexical items and what have traditionally been regarded as rules of grammar” (p. 180). I have not evaluated to what degree this is workable, but it may well be, and if it is, it certainly reduces categoriality in an interesting way: It is a valuable step in the direction of the much-wanted reconciliation of *data* (the static face of linguistic knowledge) and *processes* (its dynamic face).

To view the matter simplistically, connectionist modelling is where an alternative to categoric accounts is most likely to obtain, ultimately. Yet, Markus (2001) showed that connectionist models have not yet provided three base mechanisms which are mandatory to account for cognition in general, and language in particular – this point is very well recalled in *Foundations*, pp. 62–64. So, today, it is not possible to simply abandon symbolic accounts for connectionist accounts.

In my doctoral dissertation, *Le Locuteur Analogique (The Analogical Speaker)*, Lavie 2003), I provide a ruleless and category-free account of language productivity. It is residually symbolic, and willingly so. It greatly alleviates the problem of binding as stated in *Foundations*, pages 58–60. In effect, among the several causes generating a need for binding, *Foundations* includes the need to bind instances and types (i.e., categories) together. Jackendoff identifies this as the main cause of “the massiveness of the binding problem.” The model I propose posits no categories (and, as a corollary, no rules); all the computation takes place among exemplars and occurrences. This alone suppresses the need to bind instances to types. Therefore, there is still a certain amount of binding required, but it ceases to be as *massive* as deemed by Jackendoff. Reducing the want for binding in this way makes a step toward plausibility.

On page 186, Jackendoff writes:

I am [*sic*] must admit to being uneasy with claiming that the pressure on lexical items from regular l-rules plus historical contingency are together enough to account for the overwhelming syntactic regularity of idioms. Historical contingencies surely are responsible for some irregular idioms . . . evidence from lexical memory can now be brought to bear on the properties of general categorization. I take such potential unification to be a reason for optimism, as it draws a principled connection between two pre-existing robust lines of research.

I think that there is not that much about which to be uneasy. If rules and categories are excluded from the explanation, and contingency (historical and otherwise) is re-acknowledged as underlying all language dynamics, then it becomes possible to see lexical items, far from undergoing “pressure from regular l-rules,” rather, as actively participating in productive processes that are mixed in the sense that they will produce outcomes that sometimes exhibit regularities and sometimes irregularities (as perceived from a given analytical standpoint). The way to achieve this is perhaps through recognition of inheritance, but not by installing inheritance hierarchies explicitly in the theory (Jackendoff himself claims [pp. 185–86] that “there are no overt inheritance hierarchies in the brain”). On the contrary, lexical contingency and the empowerment of the lexicon are achieved by obtaining inheritance effects (along with categorization effects, regularization effects, etc.) and by founding the base inscriptions (I do not write “representations”) and base dynamics on something antecedent:

analogy. The latter has to be backed by contingent, exemplarist paradigmatic links, exerting exemplarist co-positionings of terms, and by abductive movements, the combination of which produces the overall language effects we are seeking. Doing so does indeed “draw principled connections between pre-existing robust lines of research,” one of them being analogy, a respectable, bimillenary theme in linguistics (e.g., studied by Varro, Paul, Brugmann, Sausure, Bloomfield, etc.; cf. also Householder [1971]; Itkonen & Haukioja [1997]), which has been despised and unfortunately ruled out by other influential theoreticians of linguistics through most of the twentieth century. It also connects interestingly with more recent work in neighboring fields (cf. Gentner et al. 2001; Choe 2003; for the latter, one important function of the thalamus is to process analogies).

A theory based on exemplarist inscriptions (and therefore, rejecting rules, templates, constructions, etc.) has another important benefit. Above I quoted Jackendoff refraining from pretending to have filled the gap of language acquisition. As he summarizes acquisition data (pp. 189–90), he rightly mentions results, notably Tomasello’s, which show that the emergence of a new construction happens one word at a time instead of “popping into place.” This constitutes a strong push to dismiss rules and abstract constructions, favoring instead mechanisms based on exemplars, such as the ones I propose. Doing so also provides a straightforward explanation of the sigmoid curve (or logistic curve), which governs the appearance, spreading, and generalization of a new “structure” in the observed productions of young speakers.

The good news with *Foundations* is that, except for a timid “perhaps” (p. 57), it makes no claim that probabilities would play an explanatory role in linguistic theory – *contra* a number of authors who called on probabilities over the last decade, in a desperate effort to cope with variety and variation after realizing that categoric theories fall short on this count.

Finally, if I have sounded negative in my critique, this is because I chose to concentrate solely on categoricity. This must not hide a global esteem for *Foundations*. In particular, the idea (after Selkirk [1984], van Vallin [2001], and Sadock [1991]) that linguistic structure is multidimensional – that is, that it is made up of several complementary, simple hierarchical structures – is certainly a very sound and important one. It deserves being fleshed out in a noncategorical manner.

“Parallel architecture” as a variety of stratificationalism

David G. Lockwood

Department of Linguistics and Germanic, Slavic, Asian, and African Languages, Michigan State University, East Lansing, MI 48824.
lockwood8@msu.edu

Abstract: The model of parallel architecture for language presented by Jackendoff is a kind of stratificational model in the spirit of Sydney Lamb. It differs from the more usual stratificationalism most importantly in its clear commitment to nativism, though the variety of nativism is greatly modified from what is more usual among Chomskians. The revised model presents a potential for fruitful discussion with proponents of stratificationalism, and the potential for enrichment via a relational implementation.

The striking thing about Jackendoff’s *Foundations of Language: Brain, Meaning, Grammar, Evolution* (2002), from my viewpoint, is its similarity to the work of Sydney Lamb – to such a point that dialogue between supporters of the respective views becomes much more feasible than in the past. It can honestly be said that the “parallel architecture” model that Jackendoff proposes amounts to a variety of stratificational theory.

The only citation of Lamb’s work in the book, however, is Lamb