

Précis of *Foundations of Language: Brain, Meaning, Grammar, Evolution*

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Abstract: The goal of this study is to reintegrate the theory of generative grammar into the cognitive sciences. Generative grammar was right to focus on the child's acquisition of language as its central problem, leading to the hypothesis of an innate Universal Grammar. However, generative grammar was mistaken in assuming that the syntactic component is the sole course of combinatoriality, and that everything else is "interpretive." The proper approach is a parallel architecture, in which phonology, syntax, and semantics are autonomous generative systems linked by interface components. The parallel architecture leads to an integration within linguistics, and to a far better integration with the rest of cognitive neuroscience. It fits naturally into the larger architecture of the mind/brain and permits a properly mentalistic theory of semantics. It results in a view of linguistic performance in which the rules of grammar are directly involved in processing. Finally, it leads to a natural account of the incremental evolution of the language capacity.

Keywords: evolution of language; generative grammar; parallel architecture; semantics, syntax

1. Introduction

In the 1960s, when I became a graduate student in linguistics, generative grammar was *the* hot new topic. Everyone from philosophers to psychologists to anthropologists to educators to literary theorists was reading about transformational grammar. But by the late 1970s, the bloom was off the rose, although most linguists didn't realize it; and by the 1990s, linguistics was arguably far on the periphery of the action in cognitive science. To some extent, of course, such a decline in fortune was simply a matter of fashion and the arrival of new methodologies such as connectionism and brain imaging. However, there are deeper reasons for linguistics' loss of prestige, some historical and some scientific. The basic questions I want to take up here, then, are:

What was *right* about generative grammar in the 1960s, that it held out such promise?

What was *wrong* about it, such that it didn't fulfill its promise?

How can we fix it, so as to restore its value to the other cognitive sciences?

The goal is to integrate linguistics with the other cognitive sciences, not to eliminate the insights achieved by any of them. To understand language and the brain, we need all the tools we can get. But everyone will have to give a little in order for the pieces to fit together properly.

The position developed in *Foundations of Language* (Jackendoff 2002) is that the overall program of generative grammar was correct, as was the way this program was intended to fit in with psychology and biology. However, a basic technical mistake at the heart of the formal implementation, concerning the overall role of syntax in the grammar, led to the theory being unable to make the proper connections both within linguistic theory and with neighboring fields. *Foundations of Language* develops an alternative, the *parallel architecture*, which offers far richer opportuni-

ties for integration of the field. To understand the motivation for the parallel architecture, it is necessary to go through some history.

2. Three founding themes of generative grammar

The remarkable first chapter of Noam Chomsky's *Aspects of the Theory of Syntax* (1965, henceforth *Aspects*) set the agenda for everything that has happened in generative linguistics since. Three theoretical pillars support the enterprise: *mentalism*, *combinatoriality*, and *acquisition*.

2.1. Mentalism

Before *Aspects*, the predominant view among linguists – if it was even discussed – was that language is something that exists either as an abstraction, in texts, or in some sense "in the community" (the latter being the influential view of de Saussure [1915], for example). Chomsky urged the view that the appropriate object of study is the linguistic system

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in the mind/brain of the individual speaker. According to this stance, a community has a common language by virtue of all speakers in the community having essentially the same linguistic system in their minds/brains.¹

The term most often used for this linguistic system is “knowledge,” perhaps an unfortunate choice. However, within the theoretical discourse of the time, the alternative was thinking of language as an ability, a “knowing how” in the sense of Ryle (1949), which carried overtones of behaviorism and stimulus-response learning, a sense from which Chomsky with good reason wished to distance himself. It must be stressed, though, that whatever term is used, the linguistic system in a speaker’s mind/brain is deeply unconscious and largely unavailable to introspection, in the same way that our processing of visual signals is deeply unconscious. Thus, language is a kind of mind/brain property hard to associate with the term “knowledge,” which commonly implies accessibility to introspection. *Foundations of Language* (hereafter *Foundations*) compromises with tradition by systematically using the term *f-knowledge* (functional knowledge) to describe whatever is in speakers’ heads that enables them to speak and understand their native language(s).

There still are linguists, especially those edging off toward semiotics and hermeneutics, who reject the mentalist stance and assert that the only sensible way to study language is in terms of the communication between individuals (a random example is Dufva & Lähteenmäki 1996). But, on the whole, the mentalistic outlook of generative grammar has continued to be hugely influential throughout linguistics and cognitive neuroscience.

More controversial has been an important distinction made in *Aspects* between the study of *competence* – a speaker’s *f-knowledge* of language – and *performance*, the actual processes (viewed computationally or neurally) taking place in the mind/brain that put this *f-knowledge* to use in speaking and understanding sentences. I think the original impulse behind the distinction was methodological convenience. A competence theory permits linguists to do what they have always done, namely, study phenomena like Polish case marking and Turkish vowel harmony, without worrying too much about how the brain actually processes them. Unfortunately, in response to criticism from many different quarters (especially in response to the collapse of the derivational theory of complexity as detailed in Fodor et al. 1974), linguists tended to harden the distinction into a firewall: competence theories were taken to be immune to evidence from performance. So began a gulf between linguistics and the rest of cognitive science that has persisted until the present.

Foundations does not abandon the competence-performance distinction, but does return it to its original status as a methodological rather than ideological distinction. Although the innovations in *Foundations* are largely in the realm of competence theory, one of their important consequences is that there is a far closer connection to theories of processing, as well as the possibility of a two-way dialogue between competence and performance theories. We return to this issue in section 9.3.

2.2. Combinatoriality

The earliest published work in generative grammar, Chomsky’s *Syntactic Structures* (1957), began with the observa-

tion that a language contains an arbitrarily large number of sentences. Therefore, in addition to the finite list of words, a characterization of a language must contain a set of *rules* (or *grammar*) that collectively describe or “generate” the sentences of the language. *Syntactic Structures* showed that the rules of natural language could not be characterized in terms of a finite-state Markov process, nor in terms of a context-free phrase structure grammar. Chomsky proposed that the appropriate form for the rules of a natural language is a context-free phrase structure grammar supplemented by transformational rules. Not all subsequent traditions of generative grammar (e.g., Head-Driven Phrase Structure Grammar, Pollard & Sag 1994; Lexical-Functional Grammar, Bresnan 1982; 2001) have maintained the device of transformational rules; but they all contain machinery designed to overcome the shortcomings of context-free grammars pointed out in Chomsky 1957.²

Transferred into the mentalistic framework of Chomsky 1965, the consequence of combinatoriality is that speakers of the language must have rules of language (or mental grammars) in their heads as part of their *f-knowledge*. Again there is a certain amount of controversy arising from the term “rules.” Rules of grammar in the sense of generative grammar are not like any of the sorts of rules or laws in ordinary life: rules of etiquette, rules of chess, traffic laws, or laws of physics. They are unconscious principles that play a role in the production and understanding of sentences. Again, to ward off improper analogies, *Foundations* uses the term *f-rules* for whatever the combinatorial principles in the head may be. Generative linguistics leaves open how directly the *f-rules* are involved in processing, but, as suggested above, the unfortunate tendency among linguists has been not to care. The theory in *Foundations*, though, makes it possible to regard the rules as playing a direct role in processing (again, see sect. 9.3).

An important reason for the spectacular reception of early generative grammar was that it went beyond merely claiming that language needs rules. It offered rigorous formal techniques for characterizing the rules, based on approaches to the foundations of mathematics and computability developed earlier in the century. The technology suddenly made it possible to say lots of interesting things about language and ask lots of interesting questions. For the first time ever it was possible to provide detailed descriptions of the syntax of natural languages (not only English but German, French, Turkish, Mohawk, Hidatsa, and Japanese were studied early on). In addition, generative phonology took off rapidly, adapting elements of Prague School phonology of the 1930s to the new techniques. With Chomsky and Halle’s 1968 *Sound Pattern of English* as its flagship, generative phonology quickly supplanted the phonological theory of the American structuralist tradition.

2.3. Acquisition

Mentalism and combinatoriality together lead to the crucial question: How do children get the *f-rules* into their heads? Given that the *f-rules* are unconscious, parents and peers cannot verbalize them; and even if they could, children would not understand, because they don’t know language yet. The best the environment can do for a language learner is provide examples of the language in a context. From there on it is up to the language learner to construct the principles on his or her own – unconsciously, of course.

Chomsky (1965) asked the prescient question: What does the child have to “[f-]know in advance” in order to accomplish this feat? He phrased the problem in terms of the “poverty of the stimulus”: Many different generalizations are consistent with the data presented to the child, but the child somehow comes up with the “right” one, that is, the one that puts him or her in tune with the generalizations of the language community. I like to put the problem a bit more starkly: The whole community of linguists, working together for decades with all sorts of crosslinguistic and psycholinguistic data unavailable to children, has still been unable to come up with a complete characterization of the grammar of a single natural language. Yet every child does it by the age of ten or so. Children don’t have to make the choices we do. They don’t have to decide whether the “right” choice of grammar is in the style of transformational grammar, the Minimalist Program, Optimality Theory, Role and Reference Grammar, Tree-Adjoining Grammar, Cognitive Grammar, connectionist networks, or some as yet unarticulated alternative. They already f-know it in advance.

One of the goals of linguistic theory, then, is to solve this “Paradox of Language Acquisition” by discovering what aspects of linguistic f-knowledge are *not* learned, but rather *form the basis* for the child’s learning. The standard term for the unlearned component is *Universal Grammar* or *UG*, a term that again perhaps carries too much unwanted baggage. In particular, UG should not be confused with universals of language; it is rather what shapes the acquisition of language. I prefer to think of it as a toolkit for constructing language, out of which the child (or better, the child’s brain) f-selects tools appropriate to the job at hand. If the language in the environment happens to have a case system (like German), UG will help shape the child’s acquisition of case; if it has a tone system (like Mandarin), UG will help shape the child’s acquisition of tone. But if the language in the environment happens to be English, which lacks case and tone, these parts of UG will simply be silent.

What then is the source of language universals? Some of them will indeed be determined by UG, for example, the overall “architecture” of the grammatical system: the parts of the mental grammar and their relations (of which much more below). Other universals, especially what are often called “statistical” or “implicational” universals, may be the result of biases imposed by UG. For example, UG may say that if a language has a case system, the simplest such systems are thus-and-so; these will be widespread systems crosslinguistically; they will be acquired earlier by children than other systems; and they will create pressures for historical change. Other universals may be a consequence of the functional properties of any relatively efficient communication system: for instance, the most frequently used signals tend to be short. UG doesn’t have to say anything about these universals at all; they will come about through the dynamics of language use in the community (a process which of course is not very well understood).

If UG is not learned, how does the child acquire it? The only alternative is through the structure of the brain, which is determined through a combination of genetic inheritance and the biological processes resulting from expression of the genes, the latter in turn determined by some combination of inherent structure and environmental input. Here contemporary science is pretty much at an impasse. We know little about how genes determine brain structure and

nothing about how the details of brain structure determine anything about language structure, even aspects of language as simple as speech sounds. Filling out this part of the picture is a long-term challenge for cognitive neuroscience. It is premature to reject the hypothesis of Universal Grammar, as some have (e.g., Deacon 1997; Elman et al. 1996), arguing that we don’t know how genes could code for language acquisition. After all, we don’t know how genes code for birdsong or sexual behavior or sneezing either, but we don’t deny that there is a genetic basis behind these.

There next arises the question of how much of UG is a human cognitive specialization for language and how much is a consequence of more general capacities. The question has often been oversimplified to a binary decision between language being entirely special or entirely general, with a strong bias toward the former inside generative linguistics and toward the latter outside the field. The truth of the matter undoubtedly lies somewhere in between. To be sure, many people (including myself) would find it satisfying if a substantial part of language acquisition were a consequence of general human cognitive factors; but the possibility of some specialization overlaying the general factors must not be discounted. My view is that we cannot determine what is general and what is special until we have comparable theories of other cognitive capacities, including other *learned* cognitive capacities. To claim that language is parasitic on, say, motor control, perhaps because both have hierarchical and temporal structures (this seems to be the essence of Corballis’s [1991] position) – but without stating a theory of the f-knowledge involved in motor control – is to coarsen the fabric of linguistic theory to the point of unrecognizability. The closest approach to a comparable theory is the music theory of Lerdahl and Jackendoff (1983), which displays some striking parallels and some striking differences with language (Pinker and Jackendoff, to appear).

Of course, if UG – the ability to learn language – is in part a human cognitive specialization, it must be determined by some specifically human genes, which in turn had to have come into existence sometime since the hominid line separated from the other great apes. One would therefore like to be able to tell some reasonable story about how UG could be shaped by natural selection or other evolutionary processes. We return to this issue in section 9.4.

This approach to the acquisition of language has given rise to a flourishing tradition of developmental research (references far too numerous to mention) and a small but persistent tradition in learnability theory (e.g., Baker & McCarthy 1981; Wexler & Culicover 1980). And certainly, even if the jury is still out on the degree to which language acquisition is a cognitive specialization, there have been all manner of phenomena investigated that bear on the issue, for example:

(1) The sensitive period for language learning and the consequences for first and second language acquisition at a later age (Curtiss 1977; Flynn & O’Neill 1988; Klein & Perdue 1997; Lenneberg 1967; Newport 1990).

(2) The limited ability of apes to acquire even rudimentary versions of human language, even with extensive training (Premack 1976; Savage-Rumbaugh et al. 1998; Seidenberg & Petitto 1978; Terrace 1979).

(3) The characteristic brain localization of language functions, resulting in characteristic aphasias (Zurif 1990).

(4) The grammatical parallels between spoken and signed languages and the parallels in acquisition and apha-

sia (Bellugi et al. 1989; Fischer & Siple 1990; Klima & Bellugi 1979).

(5) The existence of characteristic language deficits associated with various genetic conditions (Bellugi et al. 1994; Clahsen & Almazan 1998; Gopnik 1999).

(6) The creation of creole languages by communities of pidgin-speaking children (Bickerton 1981; DeGraff 1999).

(7) Most strikingly, the creation of a signed language *de novo* by a newly assembled community of deaf children in Nicaragua (Kegl et al. 1999).

My impression is that, although there are questions about all of these cases, en masse they offer an overwhelming case for some degree of genetic specialization for language learning in humans.

These three foundational issues of generative grammar – mentalism, combinatoriality, and acquisition – have stood the test of time; if anything, they have become even more important through the years in the rest of cognitive science. It is these three issues that connect linguistics intimately with psychology, brain science, and genetics. Much of the promise of generative linguistics arose from this new and exciting potential for scientific unification.

3. The broken promise: Deep Structure would be the key to the mind

A fourth major point of *Aspects*, and the one that seeped most deeply into the awareness of the wider public, concerned the notion of Deep Structure. A basic claim of the Chomsky 1965 version of generative grammar was that in addition to the surface form of sentences, that is, the form we hear, there is another level of syntactic structure, called Deep Structure, which expresses underlying syntactic regularities of sentences. For example, a passive sentence like (1a) has a Deep Structure in which the noun phrases are in the order of the corresponding active (1b).

(1a) The bear was chased by the lion.

(1b) The lion chased the bear.

Similarly, a question such as (2a) has a Deep Structure closely resembling that of the corresponding declarative (2b).

(2a) Which martini did Harry drink?

(2b) Harry drank that martini.

In the years preceding *Aspects*, the question arose of how syntactic structure is connected to meaning. Following a hypothesis first proposed by Katz and Postal (1964), *Aspects* made the striking claim that the relevant level of syntax for determining meaning is Deep Structure.

In its weakest version, this claim was only that regularities of meaning are most directly encoded in Deep Structure, and this can be seen in examples (1) and (2). However, the claim was sometimes taken to imply much more – that Deep Structure *is* meaning, an interpretation that Chomsky did not at first discourage.³ This was the part of generative linguistics that got everyone really excited. For if the techniques of transformational grammar lead us to meaning, we can uncover the nature of human thought. Moreover, if Deep Structure is innate – being dictated by Universal Grammar – then linguistic theory gives us unparalleled access to the essence of human nature. No wonder everyone wanted to learn linguistics.

What happened next was that a group of generative linguists, notably George Lakoff, John Robert Ross, James McCawley, and Paul Postal, pushed very hard on the idea

that Deep Structure should directly encode meaning. The outcome, the theory of Generative Semantics (e.g., Lakoff 1971; McCawley 1968; Postal 1970), increased the “abstractness” and complexity of Deep Structure, to the point that the example *Floyd broke the glass* was famously posited to have eight underlying clauses, each corresponding to some feature of the semantics. All the people who admired *Aspects* for what it said about meaning loved Generative Semantics, and it swept the country. But Chomsky himself reacted negatively, and with the aid of his then-current students (full disclosure: present author included), argued vigorously against Generative Semantics. When the dust of the ensuing “Linguistics Wars” cleared around 1973 (cf. Harris 1993; Huck & Goldsmith 1995; Newmeyer 1980), Chomsky had won – but with a twist: he no longer claimed that Deep Structure was the sole level that determines meaning (Chomsky 1972). Then, having won the battle, he turned his attention not to meaning, but to relatively technical constraints on movement transformations (e.g., Chomsky 1973; 1977).

The reaction in the larger community was shock: for one thing, at the fact that the linguists had behaved so badly; but more substantively, at the sense that there had been a “bait and switch.” Chomsky had promised Meaning with a capital M and then had withdrawn the offer. Many researchers, both inside and outside linguistics, turned away from generative grammar with disgust, rejecting not only Deep Structure but also mentalism, innateness, and sometimes even combinatoriality. And when, later in the 1970s, Chomsky started talking about meaning again, in terms of a syntactic level of Logical Form (e.g., Chomsky 1981), it was too late – the damage had been done. From this point on, the increasingly abstract technical apparatus of generative grammar was of no interest to more than a tiny minority of cognitive scientists, much less to the general public.

Meanwhile, various non-Chomskyan traditions of generative grammar developed, most notably Relational Grammar (Perlmutter 1983), Head-Driven Phrase Structure Grammar (Pollard & Sag 1987; 1994), Lexical-Functional Grammar (Bresnan 1982; 2001), Formal Semantics (Chierchia & McConnell-Ginet 1990, Heim & Kratzer 1998; Pardee 1976), Optimality Theory (Prince & Smolensky 1993), Construction Grammar (Fillmore et al. 1988; Goldberg 1995), and Cognitive Grammar (Lakoff 1987; Langacker 1987; Talmy 2000). On the whole, these approaches to linguistics (with the possible exception of Cognitive Grammar) have made even less contact with philosophy, psychology, and neuroscience than the recent Chomskyan tradition. My impression is that many linguists have simply returned to the traditional concerns of the field: describing languages, with as little theoretical and cognitive baggage as possible. Although this is perfectly fine – issues of innateness don’t play too big a role when you’re trying to record an endangered language before its speakers all die – the sense of excitement that comes from participating in the integration of fields has become attenuated.

4. The scientific mistake: Syntactocentrism

So much for pure intellectual history. We now turn to what I think was an important mistake at the core of generative grammar, one that in retrospect lies behind much of the alienation of linguistic theory from the cognitive sciences. Chomsky did demonstrate that language requires a gener-

ative system that makes possible an infinite variety of sentences. However, he explicitly assumed, without argument (Chomsky 1965), that generativity is localized in the syntactic component of the grammar – the construction of phrases from words – and that phonology (the organization of speech sounds) and semantics (the organization of meaning) are purely “interpretive,” that is, that their combinatorial properties are derived strictly from the combinatoriality of syntax.

In 1965 this was a perfectly reasonable view. The important issue at that time was to show that *something* in language is generative. Generative syntax had provided powerful new tools, which were yielding copious and striking results. At the time, it looked as though phonology could be treated as a sort of low-level derivative of syntax: The syntax gets the words in the right order, then phonology massages their pronunciation to adjust them to their local environment. As for semantics, virtually nothing was known: The only things on the table were the rudimentary proposals of Katz and Fodor (1963) and some promising work by people such as Bierwisch (1967; 1969) and Weinreich (1966). So the state of the theory offered no reason to question the assumption that all combinatorial complexity arises from syntax.

Subsequent shifts in mainstream generative linguistics stressed major differences in outlook. But one thing that remained unchanged was the assumption that syntax is the sole source of combinatoriality. Figure 1 diagrams the architecture of components in three major stages of Chomskyan syntactic theory: the *Aspects* theory, Principles and Parameters (or Government-Binding) Theory (Chomsky 1981), and the Minimalist Program (Chomsky 1995). The arrows denote direction of derivation.

These theoretical shifts alter the components of syntax and their relation to sound and meaning. What remains constant throughout, though, is that (a) there is an initial stage of derivation in which words or morphemes are combined into syntactic structures; (b) these structures are then massaged by various syntactic operations; and (c) certain syntactic structures are shipped off to phonology/phonetics to be pronounced and other syntactic structures are shipped off to “semantic interpretation” to be understood. In short, syntax is the source of all linguistic organization.

I believe that this assumption of “syntactocentrism” – which, I repeat, was never explicitly grounded – was an important mistake at the heart of the field.⁴ The correct approach is to regard linguistic structure as the product of a number of parallel but interacting generative capacities – at the very least, one each for phonology, syntax, and semantics. As we will see, elements of such a “parallel architecture” have been implicit in practice in the field for years. What is novel in the present work is bringing these practices out into the open, stating them as a foundational principle of linguistic organization, and exploring the large-scale consequences.

5. Phonology as an exemplar of the parallel architecture

An unnoticed crack in the assumption of syntactocentrism appeared in the middle to late 1970s, when the theory of phonology underwent a major sea change. Before then, the sound system of language had been regarded essentially as a sequence of speech sounds. Any further structure, such

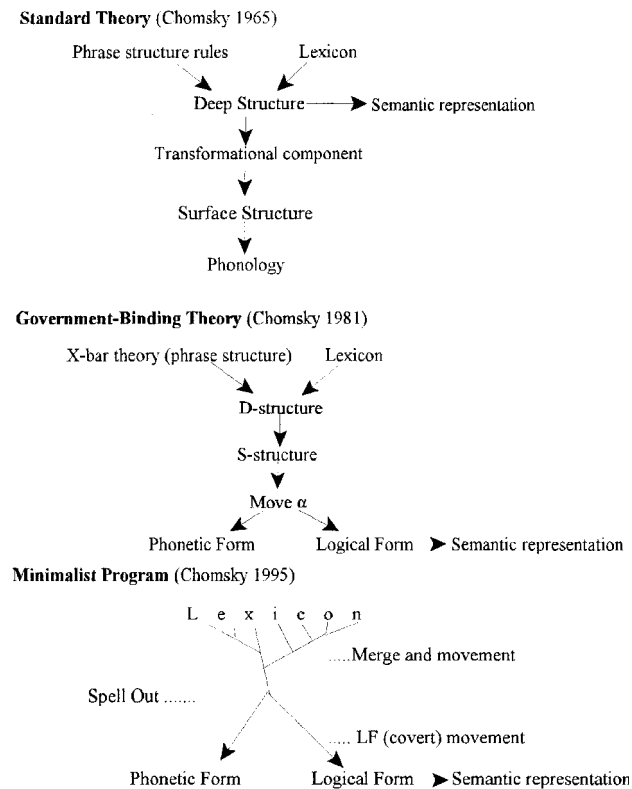


Figure 1. Architecture of Chomsky's theories through the years.

as the division into words, was thought of as simply inherited from syntax. However, beginning with work such as Goldsmith (1979) and Liberman and Prince (1977), phonology rapidly came to be thought of as having its own autonomous structure, in fact multiple structures or *tiers*. Figure 2 provides a sample, the structure of the phrase “the big apple.” The phonological segments appear at the bottom, as terminal elements of the syllabic tree.

There are several innovations here. First, syllabic structure is seen as hierarchically organized. At the center of the syllable (notated as σ) is a syllabic nucleus (notated N), which is usually a vowel but sometimes a syllabic consonant such as the l in “apple.” The material following the nucleus is the syllabic coda (notated C); this groups with the nucleus to form the rhyme (notated R), the part involved in rhyming. In turn, the rhyme groups with the syllabic onset (notated O) to form the entire syllable. Syllables are grouped together into larger units such as feet and phonological words (the bracketing subscripted Wd). Notice that in Figure 2, the word “the” does not constitute a phonological word on its own; it is attached (or cliticized) to the word “big.” Finally, phonological words group into larger units such as phonological phrases. Languages differ in their repertoire of admissible nuclei, onsets, and codas, but the basic hierarchical organization and the principles by which strings of segments are divided into syllables are universal. (It should also be mentioned that signed languages have parallel syllabic organization, except that the syllables are built out of manual rather than vocal constituents, Fischer & Siple 1990; Klima & Bellugi 1979.)

These hierarchical structures are not built out of syntactic primitives such as nouns, verbs, and determiners; their units are intrinsically phonological. In addition, the structures, though hierarchical, are not recursive.⁵ Therefore,

dently, in the hope they can be matched up by the interfaces. That would be the same sort of mistake as thinking that speakers start with the symbol *S* and generate a syntactic tree, finally putting in words so they know what the sentence is about. At the moment we are not thinking in terms of production; rather we are stating the principles (of “competence”) in terms of which sentences are well-formed. We will get back to how this is related to processing in section 9.3.

Now the main point of this section: This view of phonological structure, developed in the late 1970s and almost immediately adopted as standard, is deeply subversive of the syntactocentric assumption that all linguistic combinatoriality originates in syntax. According to this view, phonological structure is not just a passive hand-me-down derived from low-level syntax – it has its own role in shaping the totality of linguistic structure. But no great commotion was made about this most radical aspect of the new phonology. Phonologists for the most part were happy to get on with exploring this exciting way of doing things, and for them, the consequences for syntax didn’t matter. Syntacticians, for their part, simply found phonology irrelevant to their concerns of constraining movement rules and the like, especially since phonology had now developed its own arcane technical machinery. So neither subdiscipline really took notice; and as the technologies diverged, the relation between syntax and phonology became a no-man’s-land (or perhaps only a very-few-men’s-land). Tellingly, as far as I can determine, in all of Chomsky’s frequent writings on the character of the human language capacity, there is virtually no reference to post-1975 phonology – much less to the challenge that it presents to his overall syntactocentric view of language.

6. The syntax-semantics interface

I have treated the developments in phonology first because they are less controversial. But in fact the same thing happened in semantics. During the course of the 1970s and 1980s, several radically different approaches to semantics developed. Within linguistics, these developments at least included Formal Semantics (growing out of formal logic, Chierchia & McConnell-Ginet 1990, Heim & Kratzer 1998; Partee 1976), Cognitive Grammar (Lakoff 1987; Langacker 1987; Talmy 2000), and Conceptual Semantics (Jackendoff 1983; 1990; Pinker 1989; Pustejovsky 1995), plus approaches within computational linguistics and cognitive psychology. Whatever their differences, all these approaches take meaning to be deeply combinatorial. None of them take the units of semantic structure to be syntactic units such as NPs and VPs; rather, the units are intrinsically semantic entities like objects, events, actions, properties, and quantifiers.⁷ Therefore, whichever semantic theory we choose, it is necessary to grant semantics an independent generative organization, and it is necessary to include in the theory of grammar an interface component that correlates semantic structures with syntactic and phonological structures. In other words, the relation of syntax to semantics is qualitatively parallel to the relation of syntax to phonology. However, apparently no one pointed out the challenge to syntactocentrism – except the Cognitive Grammarians, who mostly went to the other extreme and denied syntax *any* independent role, and who have been steadfastly ignored by mainstream generative linguistics.

The organization of phonological structure into semi-independent tiers finds a parallel in semantics as well. Linguistic meaning can be readily partialled into two independent aspects. On one hand, there is what might be called “propositional structure”: who did what to whom and so on. For example, in *The bear chased the lion*, there is an event of chasing in which the bear is the chaser and the lion is “chasee.” On the other hand, there is also what is now called “information structure”: the partitioning of the message into old versus new information, topic versus comment, presupposition versus focus, and so forth. We can leave the propositional structure of a sentence intact but change its information structure, by using stress (6a–c) or various focusing constructions (6d–f):

- (6) a. The BEAR chased the lion.
- b. The bear chased the LION.
- c. The bear CHASED the lion.
- d. It was the bear that chased the lion.
- e. What the bear did was chase the lion.
- f. What happened to the lion was the bear chased it.

Thus the propositional structure and the information structure are orthogonal dimensions of meaning, and can profitably be regarded as autonomous tiers. (*Foundations* proposes a further split of propositional structure into *descriptive* and *referential* tiers, an issue too complex for the present context.)

Like the interface between syntax and phonology, that between syntax and semantics is not an isomorphism. Some aspects of syntax make no difference in semantics. For instance, the semantic structure of a language is the same whether or not the syntax marks subject-verb agreement, verb-object agreement, or nominative and accusative case. The semantic structure of a language does not care whether the syntax calls for the verb to be after the subject (as in English), at the end of the clause (as in Japanese), or second in a main clause and final in a subordinate clause (as in German). As these aspects of syntax are not correlated with or derivable from semantics, the interface component disregards them.

Similarly, some aspects of semantics have little if any systematic effect in syntax. Here are a few well-known examples:

First, the syntactic form of a question can be used to elicit information (7a), test someone’s knowledge (7b), request an action (7c), or express sarcasm (7d). Therefore, these choices of illocutionary force are not mapped into syntactic structure.

- (7) a. Where is my hat?
- b. (Now, Billy:) What’s the capital of New York?
- c. Would you open the window?
- d. Is the Pope Catholic?

Second, in example (8a), the interpretation is that Jill jumped multiple times. This aspect of interpretation does not arise from any single word in the sentence, nor from the syntactic structure. If we change the verb to *sleep*, as in (8b), we don’t interpret the sentence as implying multiple acts of sleeping. If we change *until* to *when*, as in (8c), only a single jump is entailed.

- (8) a. Jill jumped until the alarm went off.
- b. Jill slept until the alarm went off.
- c. Jill jumped when the alarm went off.

The standard account of this contrast (Jackendoff 1997; Pustejovsky 1995; Talmy 2000; Verkuyl 1993) is that the meaning of *until* sets a temporal bound on an ongoing

process. When the verb phrase already denotes an ongoing process, such as sleeping, all is well. But when the verb phrase denotes an action that has a natural temporal ending, such as jumping, then its interpretation is “coerced” into *repeated* action – a sort of ongoing process – which in turn can have a temporal bound set on it by *until*. For present purposes, the point is that the sense of repetition arises from semantic combination, without any direct syntactic reflex. (On the other hand, there are languages such as American Sign Language that have a grammatical marker of iteration; this will have to be used in the translation of [8a].)

A third case of semantic combinatoriality that is not reflected in syntax appears in (9), where the “understood” subject of the sentence is not the entity normally denoted by the actual subject (Nunberg 1979).

- (9) a. [One waitress says to another]:
The ham sandwich wants another cup of coffee.
[Interpretation: “*the person who ordered/is eating* the ham sandwich . . .”]
b. Chomsky is on the top shelf next to Plato.
[Interpretation: “*the book by* Chomsky . . .”]

Such cases of “reference transfer” contain no syntactic reflex of the italicized parts of the interpretation. One might be tempted to dismiss these phenomena as “mere pragmatics,” hence outside the grammatical system. But this proves impossible, because reference transfer can have indirect grammatical effects. A clear example involves imagining that Richard Nixon went to see the opera *Nixon in China* (yes, a real opera!), and what happened was that:

- (10) Nixon was astonished to see himself sing a foolish duet with Pat.

The singer of the duet, of course, is the *actor playing* Nixon; thus the interpretation of *himself* involves a reference transfer. However, we cannot felicitously say that what happened next was that:

- (11) *(Up on stage), Nixon was astonished to see himself get up and walk out.

That is, a reflexive pronoun referring to the acted character can have the real person as antecedent, but not vice versa (Fauconnier 1985; Jackendoff 1992b). As the use of reflexive pronouns is central to grammar, reference transfer cannot be seen as “extragrammatical.”

A fourth case of syntax-semantics mismatch concerns one of the most persistent issues within generative grammar, the syntactic status of quantifier scope. Consider the two interpretations of (12).

- (12) Everyone in this room knows at least two languages.
a. “John knows English and French; Sue knows Hebrew and Hausa; . . .”
b. “. . . namely, Mandarin and Navajo.”

Should there be two different syntactic structures associated with these two interpretations? Chomsky 1957 said no; Chomsky 1981 said yes; Generative Semantics said yes; I am inclined to say no (Jackendoff 1996c; 2002 [*Foundations*], Ch. 12). The problem with finding two different syntactic structures is that it requires systematic and drastic distortions of the syntactic tree that never show up in the surface syntax of any language. The problem with having only one syntactic structure is that it makes the syntax-semantics interface more complex. The point to be made here is that the scope of quantification may well be a further example of the “dirtiness” of the interface between syntax and semantics. This continues to be an important issue in linguistic theory.

In each of these cases, a syntactocentric theory is forced to derive the semantic distinctions from syntactic distinctions. Hence, it is forced into artificial solutions such as empty syntactic structure and elaborate movement, which have no independent motivation beyond providing grist for the semantics. On the other hand, if the semantics is treated as independent from syntax but correlated with it, it is possible to permit a less than perfect correlation; it is then an empirical issue to determine how close the match is.

If we abandon syntactocentrism, it is logically possible that there are aspects of semantics that have no impact on syntax but *do* have an effect on phonology through a direct phonology-semantics interface. Such a treatment is attractive for the correlation between prosody and information structure. For example, the differences among (6a-c) do not show up in syntax at all – only in the stress and intonation in phonology, and in the focus-presupposition relations in semantics. In a syntactocentric theory, one is forced to generate these sentences with a dummy syntactic element such as [+Focus], which serves only to correlate phonology and meaning and does not affect word order or inflection. (Such was the approach in Jackendoff [1972], for instance.) But this element does no work in syntax per se; it is only present to account for the correlation between phonology and semantics. By introducing a direct phonology-to-semantics interface sensitive to this correlation, we can account for it with minimal extra machinery; but of course this requires us to abandon syntactocentrism.

7. The outcome: Parallel architecture

The argument so far has been that theoretical thinking in both phonology and semantics has proceeded in practice as though their structures are a result of independent generative capacities. What has attracted far less notice among syntacticians, phonologists, and semanticists alike is that such an organization logically requires the grammar to contain interface components that correlate the independent structures. Carrying this observation through the entire architecture of grammar, we arrive at an overall picture like Figure 3. Here the grammar contains multiple sets of formation rules (the “generative” components), each determining its own characteristic type of structure; and the structures are linked or correlated by interface components.

In the syntactocentric architecture, a sentence is well-formed when its initial syntactic tree is well-formed and all the steps of derivation from this to phonology and semantics are well-formed. In the parallel architecture, a sentence is well-formed when all three of its structures – phonological, syntactic, and semantic – are independently well-formed and a well-formed correspondence among them has been established by the interfaces.

One of the primary interface rules between phonology and syntax is that the linear order of units in phonology corresponds to the linear order of the corresponding units in syntax. One of the primary interface rules between syntax and semantics is that a syntactic head (such as a verb, noun, adjective, or preposition) corresponds to a semantic function, and that the syntactic arguments of the head (subject, object, etc.) correspond to the arguments of the semantic

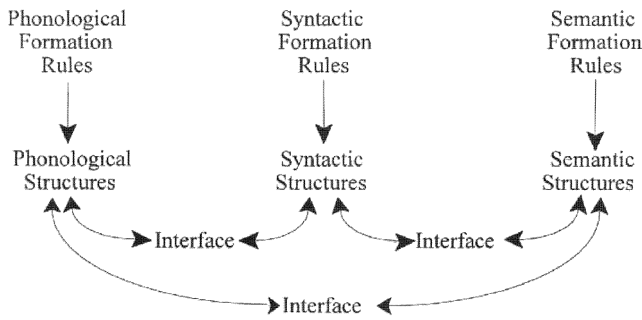


Figure 3. The parallel architecture.

function. The consequence of these two primary interface principles is that for the most part, syntax has the linear order of phonology but the embedding structure of semantics.

An illustration of some of these properties of the parallel architecture appears in Figure 4, the structure of the phrase *the cats*. The three independent structures are displayed side by side;⁸ the subscripting indicates the connections established by the interfaces between the parts of the three structures. For example, the clitic pronounced *ðə* is indexed with the determiner in the syntax and with the definiteness feature in semantics. Notice that the lowest nodes in the syntactic tree are syntactic features, not the customary notation *the cat-s*. The reasons for this are explained in the next section.

The overall architecture laid out in Figure 3 provides a model within which many different theories of grammar can be embedded and compared. For example, Figure 3 does not dictate whether the syntactic formation rules are along the lines of transformational grammar, the Minimalist Program, Head-Driven Phrase Structure Grammar, or many other alternatives. Moreover, the syntactocentric framework is a version of Figure 3 in which the phonological and semantic formation rules are null, so that everything in phonological and semantic structures is determined only by their interfaces with syntax. The framework favored by many in Cognitive Linguistics minimizes or even eliminates the syntactic formation rules, so that syntax is determined entirely by meaning.

The organization into parallel generative components is not new here. In addition to the innovations in phonology discussed in section 5, Lexical-Functional Grammar divides syntax into two tiers, c-structure and f-structure; Autolexical Syntax (Sadock 1991) has a different division into morphosyntax and phrasal syntax; Role and Reference Grammar (Van Valin & LaPolla 1997) has, in addition to a morphosyntax/phrasal syntax division, the propositional/

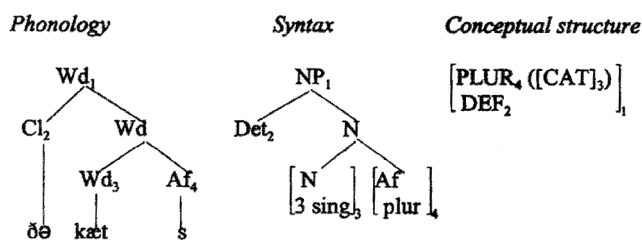


Figure 4. The structure of *the cats* in the parallel architecture.

information tier division in semantics, with interfaces going every which way among the tiers. In other words, various elements of this architecture are widely present in the literature. What is novel here is recognizing that this organization runs through the entire grammar, from phonology through semantics (and further, as we will see in sect. 9).⁹

It might well be argued that the standard syntactocentric framework has served the field well for forty years. Why should anyone want to give it up? A reply might come in five parts. First, no one has ever argued for the syntactocentric model. In *Aspects*, it was explicitly only an assumption, which quickly hardened into dogma and then became part of the unstated background. By contrast, the parallel architecture now *has* been argued for, in part based on well-established results in phonology and semantics, which have never played a role in syntactocentric argumentation.

Second, an advocate might argue that the syntactocentric model is a priori simpler: Why should we admit so many different components into the grammar? The reply would be that the choice among theories must be determined by empirical adequacy as well as a priori simplicity. If the parallel architecture allows a more perspicuous account of, say, intonation contours or the relation of focus to stress, these are arguments in its favor.

A third point concerns the relation of syntax and semantics. As syntax is now not responsible for determining every semantic distinction, it is to some degree liberated from semantics and can therefore be considerably simplified. However, some compensating complexity must be introduced into the syntax-semantics interface, so it doesn't disappear from the grammar entirely. It now becomes an empirical question how to parcel the complexity out, and this question can be addressed; it is not just an issue of opinion or preference (see e.g., Culicover 1999b; Culicover & Jackendoff 1995; 1997; 1999; Culicover & Jackendoff, forthcoming). At the same time, syntax does not go away entirely (as opponents of Chomsky would often like). The syntax of a language still has to say where the verb goes, whether the verb agrees with the subject, how to form relative clauses and questions, and so on. The differences among languages in these respects are not predictable from semantics, and children have to learn them.

A fourth point concerns the nature of Universal Grammar. In the parallel architecture, the issues of acquisition and innateness don't go away, they are exactly the same, namely: How does the child acquire the grammar of its native language on the basis of environmental evidence? However, as just suggested, the sorts of questions that most often arise concern the balance of power among components. We don't find ourselves invariably asking: What do we have to add to syntax to account for such-and-such a phenomenon? Rather, we find ourselves asking: In which component does this phenomenon belong? Is it a fact of syntax, of semantics, or of the interfaces? And to what extent is it realistic to attribute such a bias to the child learning the language?

A final point concerns not linguistic structure itself but its connection to the rest of the theory of the brain/mind. On the face of it (at least in my opinion), one should favor approaches that permit theoretical integration. Section 9 will show four ways that the parallel architecture invites such integration but the syntactocentric theory does not.

8. The lexicon and the words versus rules controversy

Every theory of language has to view a word as a stored complex of phonological, syntactic, and semantic features or structure; commonly the store of words is called the *lexicon*. However, theories differ in the role of the lexicon in the construction of sentences. In all of the syntactocentric architectures shown in Figure 1, words are inserted into syntactic trees at the beginning of a syntactic derivation, at the point when syntactic trees are being built and before they begin to be manipulated and fed to phonology and semantics. Thus, the traditional notation for trees in Figure 5a is actually intended as an abbreviation of Figure 5b, in which the lexical items are spelled out in full. The consequence is that the syntax is carrying around with it all the phonological and semantic features of words, which are totally invisible to syntactic rules and are of use to the grammar only when handed over and “interpreted” by the proper component.

The parallel architecture, by contrast, insists that each kind of feature belongs only in its own structure. In this framework, the traditional syntactic notation in Figure 5 is formally incoherent, because it has phonological and semantic features in a syntactic structure. Thus, it is formally impossible to insert full lexical items into syntactic structure. How then do words get into linguistic structures? The answer is that each of the three structures making up a word is inserted into its proper structure, and each of them carries with it an index that connects it to the others. So, for example, the word *cat* is notated as in Figure 6; its contribution to the larger structure in Figure 4 should be evident.

Thus, a word is best regarded as a type of interface rule that establishes partial correspondences among pieces of phonological, syntactic, and semantic structure (each piece in turn conforming to the formation rules of its own component). In other words, the language does not consist of a lexicon *plus* rules of grammar; rather, lexical items are *among* the rules of grammar – very particular rules to be sure, but rules nonetheless.

This treatment of the lexicon offers an attractive account for a number of previously troublesome phenomena. For example, consider an idiom such as *kick the bucket*. This can be treated as a lexically listed VP that is coindexed with phonology in the normal way, but which lacks indices connecting the individual words to semantics. Instead, the VP as a whole is coindexed with the semantic structure *DIE*. As a consequence, the individual words *kick*, *the*, and *bucket* do not contribute individually to meaning. This is precisely what an idiom is supposed to be: a stored unit in which the words do not have their normal meaning.

A sort of converse is found in irregular morphology. Consider something like the irregular plural *feet*. It has to be



Figure 5a. Traditional notion for *the cat*. 5b. What the traditional notation abbreviates.

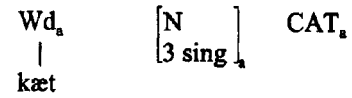


Figure 6. The structure of the word *cat*.

listed syntactically as a plural noun, and the two syntactic parts are coindexed in the normal way to semantics: the word denotes multiple entities of the type *FOOT*. However, the syntactic parts are not connected in normal fashion to phonology; rather the whole syntactic complex is coindexed with the undifferentiated lump *feet* in phonology.

Notice by contrast how the *regular* plural is coded in Figure 4. The regular plural consists of a piece of meaning, namely plurality, plus a piece of syntax, namely an affix attached to nouns, plus a piece of phonology, namely a suffix *s* or *z* or *əz*, the choice determined contextually. That is, the regular plural has all the same parts as a word, and it determines a connection between them. We can notate this as a lexical item along the lines of Figure 7. (The italicized bits denote contextual features that determine how this item is combined with its environment.) The contribution of this item to the overall structure in Figure 4 is entirely parallel to the contribution of the word *cat*.

This view of regular morphology puts a new and unexpected spin on the by now hoary “words versus rules” controversy (e.g., Elman et al. 1996; Pinker 1999; Rumelhart & McClelland 1986). Traditionally, everyone agrees that irregular plural nouns like *feet* have to be listed in the lexicon. The issues are taken to be: (1) Are regular plurals all listed as well, or is there a separate *rule* for the regular cases that says “To form the plural of a noun, add -z?” And, therefore, (2) When children learn to form regular plurals, are they learning something qualitatively different from learning the rough-and-ready generalizations among irregular plurals?

In the present view, words *are* rules – interface rules that help connect phonological, syntactic, and semantic structures. Figure 7, the “rule” for the regular English plural affix, is qualitatively no different. Its contextual features are qualitatively not unlike those of, say, transitive verbs. It combines with nouns the same way a transitive verb combines with its object. Thus, the formation of regular plurals is an instance of ordinary combinatoriality. In this approach, the issues come to be restated like this: (1) Are regular plurals all listed, or is there a separate *lexical item* that encodes the regular affix, which combines with any singular noun to form a plural noun? And, therefore, (2) when children learn to form regular plurals, are they learning this new lexical item by extracting it as a regularity from the contexts in which it appears – in the same way that they extract verbs from the phrasal contexts in which they appear?

I submit that even to the most committed of connectionists, this latter way of framing the question can hardly

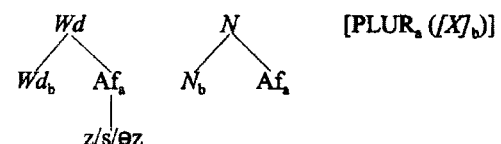


Figure 7. The English regular plural as a lexical item.

be objectionable. Yet the advocates of rules, such as Pinker, have not made the case nearly as strong as it can be. The connectionist argument has been to the effect: We can make a device that learns all English past tenses without making use of a rule, and we can find evidence from acquisition and processing that supports this account. The best version of the anti-connectionist argument has been: Connectionist modeling offers important innovations over standard models of language in dealing with case-by-case learning and analogy for the irregular past tenses. But – you still need rule learning to account for children’s acquisition of regular past tense, and we can find evidence from acquisition and processing that supports *this* account. The problem is that the debate has often been framed as though only the past tense were at issue, while the subtext behind the connectionist position is that if *this* can be learned without rules, then it is a good bet that the rest of language can be too.

But not only the past tense is at stake. To deal with the whole of language, it is necessary to account for the creative formation of things like verb phrases and relative clauses – linguistic entities that cannot be listed in the lexicon. In the present view, the way that the regular past tense affix combines with verbs is exactly like the way nouns combine with relative clauses and the way noun phrases combine with verbs and the way subordinate clauses combine with main clauses – it is just another case of free combinatoriality. In the decade and a half since the original connectionist past tense model, there has been no demonstration that the model scales up to acquisition of the full free combinatoriality of language – the issue that grounds generative linguistics.

At the same time, notice that within the parallel architecture, the terms of the dispute become far less contentious. The regular past tense is no longer a qualitatively different phenomenon from words: Words are a type of rule, and the posited regular past tense morpheme, shown in Figure 7, is in the relevant respects just like a word. It differs only in that it happens to be grammatically smaller and it requires a word as its grammatical host. So the issue is only whether there is such a separate lexical item, not whether there are two wholly different kinds of linguistic animal, namely words and rules. Thus, in the end, the fate of the past tense doesn’t seem like such a big deal.

9. Four ways the parallel architecture helps integrate linguistics with cognitive neuroscience

The parallel architecture may be an intriguing technical alternative to the Chomskyan orthodoxy in linguistics, but is there any reason why it should be of interest to anyone other than linguists? The end of the previous section may have begun to offer some hints. This section will sketch out a little more fully some ways in which the parallel architecture offers opportunities to unify linguistics with the other cognitive sciences.

9.1. The place of the parallel framework in the larger architecture of the mind/brain

To sum up the larger picture: The parallel architecture claims that language is organized into a number of semi-in-

dependent combinatorial systems, each of which has its own organizing principles. These systems are linked by systems of interface principles. Interface principles establish a correlation between pieces of structure in two (or more) of the combinatorial systems. Some interface principles deal with large-scale and general correspondences such as the parallel between linear order in syntax and in phonology. On the other hand, some of them are extremely specialized, for instance individual words, idioms, and regular affixes. The interface principles as a whole do not implement an isomorphism between the structures they connect. Rather, they implement a partial homomorphism, a “dirty” correspondence in which not all parts of the structures in question are correlated, and in which many-to-many mappings are altogether common.

This conception of the interfaces within language is perfectly in tune with the way linguistic structures connect to the rest of the mind. Consider how phonology interacts with the auditory system in speech perception and with the motor system in speech production. As is well known (to the dismay of fifty years of computer scientists working on automated speech recognition), the mapping between a frequency analysis of the speech signal and the phonological structure of an utterance is frighteningly complex. In particular, some aspects of the speech signal play no role in phonological structure and must be factored out, for example, the individual timbre of the speaker’s voice, the speaker’s tone of voice, and the speed of production, not to mention ambient noise. These aspects of the speech signal are put to use for other cognitive purposes, but not for speech. Moreover, having factored all these things out from the acoustic signal, still not every part of the phonological structure is predictable from what is left: Most prominently, word boundaries are not present as pauses in the signal. Thus, the auditory-to-phonological mapping has the same general characteristics as the interfaces inside language: It establishes a “dirty” correspondence between certain aspects of two disparate mental structures.

Speech production has similar properties. Not every aspect of phonological structure corresponds to an aspect of the motor control involved in operating the vocal tract. In particular, word boundaries do not correspond at all consistently to pauses in production. And not every aspect of motor control is controlled by phonological structure. For example, one can talk intelligibly with a pipe in one’s mouth, which hugely distorts the motor commands involved in speech without changing the phonological structure a bit. And of course the same muscles in the vocal tract are used for chewing, swallowing, and so on. Without going into more detail, it should be clear that again the same sort of interface is in play here.

Next, consider the visual system. Beyond the very early levels of vision, there is little detailed theory of the f-knowledge involved in vision – the necessary levels of representation and so on (I take Marr [1982] to have been attempting to lay out such a theory, but the enterprise has been largely abandoned since his death). On the other hand, the neuroscience of vision reveals a qualitatively similar picture: numerous independent brain areas, each specializing in a particular aspect of vision such as shape, motion, color, and spatial relations, each interacting with certain others by dedicated pathways, and no area where “it all comes together” to form a full representation of the visual field. This has precisely the flavor of the parallel architecture in lin-

guistics, where the notion of a “sentence” or “phrase” is distributed among several structures, communicating with each other via dedicated interfaces, as shown in Figure 4.¹⁰

A syntactocentric architecture, by comparison, shows no resemblance to the rest of the mind/brain. A master “computational system” that generates syntactic structures, which in turn determine phonological structures and meanings, simply has no known parallel in the brain. Even the connection of language to speech is markedly different from the connections among the components inside of language.

9.2. The role of semantics

Another important advantage of the parallel architecture is the connection of semantics to the rest of the mind/brain. *Foundations of Language* (Chs. 9 and 10) advocates that if generative grammar is to truly adopt the mentalist stance, this stance must be applied to meaning as well. According to this stance, the basic function of language is to convert thoughts into communicable form; the virtue of human language over other natural communication systems is that it is so broad in the range of messages it can convey. Each of the indefinitely many sentences of a language conveys a different thought. As not all these thoughts can be stored in a single head, it is necessary that thoughts be constructed combinatorially. Therefore, a goal for semantic theory is to uncover the combinatorial system underlying human concepts. Such a goal converges with important trends in psychology and philosophy.

However, another influential strain in semantics (and the predominant one in Anglo-American philosophy, dating back to Frege 1892 and shared by people as different as Jerry Fodor 1987; David Lewis 1972; Hilary Putnam 1975; and John Searle 1980) takes it that semantics is the study of the connection of language to the *world*. On this view, a proper semantics has to be concerned above all with how the noise *kæt* is connected with cats. How language users make that connection is quite a different issue (and to many semanticists, not of interest). There is no room here to begin the critique of this view; Chapters 9 and 10 of *Foundations* take up the argument in detail. My overall conclusion is that even if it is worthwhile undertaking such a “realist” semantics, the enterprise of discovering how language users do it is also worthwhile. I don’t care whether you call the latter enterprise semantics or shmenantics or whatever—it is this enterprise whose central issues intercalate naturally with those of generative linguistics, cognitive psychology, and neuroscience. Just to be clear, I will call this enterprise *conceptualist semantics*.

Conceptualist semantics requires us to rethink the traditional issue of reference, which takes as its starting point the unshakeable intuition that the phrase *my cat* does indeed pick out an individual in the world. In a mentalist linguistic theory, the language user’s linguistic system connects the phonological string /maykæt/ to the concept of a feline animal, and to the concept of this feline animal being possessed by the speaker of the phrase. How then does the language user get from there to the actual individual out there in the world? The answer in brief is that it isn’t just language users who have to connect something in their head to a sense of individuals in the world: Any organism with a visual system approximately like ours (e.g., babies and apes) has precisely the same problem. The environment acting on

the visual system produces some set of activations in the brain, resulting in the organism experiencing real objects out there. In other words, conceptualist semantics allows us to recognize that the problem of reference is not a problem about *language*; it is, at bottom, a problem about *perception* and *cognition* which has to be solved by psychology and neuroscience. By contrast, conventional realist theories of reference divorce reference from the mind and make no contact whatsoever with research on perception.

In order for the system of meaning to be influenced by perception, of course, there has to be an interface between conceptual/semantic structure and the “upper end” of the perceptual systems, where “the world” (i.e., the perceiver’s conceptualization of the physical world) is organized in terms of stable three-dimensional objects that are located in space with respect to the perceiver and each other. This interface too can be shown to have the standard characteristics: It is a partial homomorphism between the quasi-algebraic format in which linguistic meanings are encoded and the quasi-geometric/topological format(s) in which spatial understanding is encoded. Thus, at the semantic end of the language faculty, just as at the phonological end, the relation between language and the rest of the mind is of the same general character as the interfaces within the language faculty itself.

Studying the conceptual system as a combinatorial system leads to the same questions about acquisition as studying syntax. How does the child learning language acquire the meanings of all those thousands of words on the basis of experience, both perceptual and linguistic? What perceptual biases and innate structures does the child bring to the task of interpreting the world? Here conceptualist semantics makes contact with a rich literature on word and concept learning and its innate bases (Baillargeon 1986; Bloom 2000; Carey 1985; Gleitman & Landau 1994; Keil 1989; Macnamara 1982; Spelke et al. 1994, to mention only a few examples). Moreover, because humans doubtless share with monkeys and apes at least the parts of the conceptual system dealing with physical space and perhaps some of the parts dealing with social relations and other minds, conceptualist semantics further makes contact with research on primate cognition (Cheney & Seyfarth 1990; Hauser 2000; Köhler 1927; Povinelli 2002; Premack 1976; Tomasello 2000).

Again, these are issues that conventional realist semantics cannot address. Nor are they particularly accessible to semantics studied in a syntactocentric linguistic theory. For if the combinatorial properties of semantics were completely attributable to the combinatorial properties of syntax, then it would be impossible for nonlinguistic organisms to have combinatorial thoughts. There are of course important strains of philosophy that have embraced this view, identifying the capability for thought with the capability for overt language (Descartes comes to mind, for instance). But I think contemporary cognitive neuroscience has outgrown such a view, and linguistics ought to be able to follow suit gracefully.

9.3. The relation of grammar to processing

A theory of linguistic competence is supposed to simply define the permissible structures in the language, without saying how those structures are produced in real time. However, as pointed out in section 2, a competence theory ought

to lend itself to being embedded in a theory of processing: We ought to be able to say how the f-knowledge that constitutes the competence theory is actually put to use.

There turns out to be an inherent structural reason why competence has to be isolated from performance in the syntactocentric view of language. If we flatten out and simplify all the syntactocentric architectures in Figure 1, they all have a logical directionality proceeding outward from syntax in the middle:

- (13) *Logical directionality of syntactocentric architecture*
 sound ← phonology ← syntax → meaning

What I mean by logical directionality is that the possible phonological structures and meanings cannot be determined without first determining syntactic structures. Syntacticians may insist that they are being “metaphorical” when they talk about things happening “before” and “after” other things in a derivation; but the *logical* dependence is there nevertheless. Now contrast this to the logical directionality of language processing: Language perception goes consistently from left to right, and language production from right to left.

- (14) a. *Logical directionality of language perception*
 sound → phonology → syntax → meaning
 b. *Logical directionality of language production*
 sound ← phonology ← syntax ← meaning

Hence, there is no way that the logical directionality in (13) can serve the purposes of both perception and production. Going from syntax to phonology in (13) seems inherently like production – but only *part* of production; going from syntax to semantics in (13) seems inherently like perception – but only part of it.

The parallel architecture, by contrast, is inherently *nondirectional*. The “information flow” between sound and meaning is through the sequence of interfaces, each of which is a system of correlations between two structures, not a derivation of one from the other. The correlations can be used in either direction (which is why they are drawn with double arrows in Fig. 3). This makes it possible to think of speech perception as a process where structures are activated first at the auditory end of the chain of structures, “clamped” by the environmental input. The interfaces propagate activation rightward through the chain, each interface principle creating a partial resonance between the structures it connects. Eventually the structured activation reaches semantic structure, at which point it can interact with the hearer’s understanding of the context to produce the understanding of the heard utterance. Similarly, in speech production, the speaker begins with a thought to convey, that is, meaning is “clamped” by the speaker’s communicative intent. Then the interface principles propagate activation leftward through the chain, eventually activating motor control of the vocal tract and producing speech. Crucially, except for the specifically auditory and vocal parts of the chain, the very same structures and the very same interface principles are invoked in perception and production, just in opposite directions.

There is no need in this system for all of one level to be totally processed before activation of the next level sets in. Any activation of a level, no matter how incomplete, if it can be detected by the next interface, will start to propagate to the next level in the chain. Therefore, processing can be thought of as “incremental” or “opportunistic” rather than rigidly regulated. In addition, because the interfaces are trying to achieve “resonance,” that is, optimal mapping be-

tween levels, there is ample room in the processing theory for feedback in processing – semantics affecting syntactic processing in perception, and vice versa in production.

A crucial tenet of this theory, though, is that *the rules of grammar are the only source of information flow in language processing*. For example, knowledge of context cannot directly affect phonological processing, because there are no interface rules that directly relate contextual understanding to phonological structure. On the other hand, context can *indirectly* affect phonological processing – via the interfaces linking them through semantics and syntax. The prediction is that such feedback will take effect some time after constraints directly from phonology, because it has to go up the chain of interfaces and down again. On the whole such a prediction seems consistent with the experimental literature (Cutler & Clifton 1999; Levelt 1989); *Foundations* works out many details, in particular the relation of long-term memory to working memory during language processing.

The role of the lexicon in the processing theory is entirely parallel to that in the competence theory. Recall that words are little interface rules, providing partial routes for mapping between sound and meaning. Now consider the logic of language perception. The auditory system and the interface from audition to phonology produce some string of speech sounds in the hearer’s head, and this activates a call to the lexicon: “Do any of you guys in there sound like *this*?” Then various items “raise their hands,” that is, are activated. At this point the processor has no way of knowing which of these items is semantically appropriate, because no contact has yet been made with semantics. However, each item over time activates a connection to potential syntactic and semantic structures, which can be integrated with previous words and with context to determine which candidate word makes most sense in context. This scenario corresponds precisely to the results in lexical access experiments (Swinney 1979; Tanenhaus et al. 1979), in which at first every possible sense of a given phonological string is activated, later to be pruned down by semantic context.

A parallel story can be told for speech production. The speaker has activated some conceptual structure that s/he wishes to communicate. The first step is to call the lexicon: “Do any of you guys in there mean *this*?” Then various items raise their hands. All the lexical retrieval and speech error literature now comes to bear in showing us the flow of information from this point to actual vocal production; for the most part, it proves to correspond nicely to the options made possible by the components of the parallel framework (Levelt 1989; 1999).

It is significant that the parallel architecture accords words a very active role in determining the structure of sentences, in concurrence with evidence from the psycholinguistic literature. By contrast, the syntactocentric architecture views words as essentially passive: They simply ride around at the bottom of syntactic trees, while the derivational rules of syntax do all the interesting work. Thus again, in the area of the lexicon, the syntactocentric framework makes it hard to connect competence and performance.

The conclusion here is that the parallel architecture permits a far closer relation between competence and performance theories. The rules of the language, including the words, are posited to be precisely what the processing system uses in constructing mappings between sound and meaning. This opens the door for a two-way dialogue be-

tween linguistics and psycholinguistics. Linguistics has always dictated the structures that psycholinguistics should be investigating. But now there is the possibility that psycholinguistic experiments may help determine what component of the grammar is responsible for a particular phenomenon. For example, Piñango et al. (1999) have shown that aspectual coercion (e.g., the sense of repetition in *Jill jumped until the alarm went off*, example [8a]) causes a processing load at a point in time that is appropriate to semantic, not syntactic processing. This result conforms to the theoretical claim that aspectual coercion is a matter of adjusting semantic well-formedness, not a matter of syntactic deletion of an iterative morpheme. In short, the idealization of a competence theory is not a rigid abstraction; rather, it is a convenient methodological move, to be bridged freely when the occasion arises. (More detail on the relation of rules to processing is offered in *Foundations*, Ch. 7.)

9.4. Evolution of language

Let us return to a point from section 2. If Universal Grammar is a human cognitive specialization, it has to be transmitted by genes that have emerged in the course of our evolutionary divergence from the chimpanzees. Of course, the actual evidence for the evolution of the language faculty is practically nonexistent. There is some evidence about the evolution of the human vocal tract (Fitch 2000), but the ability to make speech sounds is only one element of language – and of course there are signed languages, which don't involve speech at all. In addition, it has begun to look like many of the mechanisms for auditory perception are already in place in other mammals (Hauser et al. 2002). But the real issue is: How did the ability to systematically map combinations of concepts into sequences of speech sounds and back again develop in our species, and how did the ability to *learn* such systematic combinatorial mappings develop?

In the absence of evidence, we would like at least to be able to tell a plausible story about the emergence of Universal Grammar, an important aspect of which is the overall architecture of language. In particular, we would not like to have to explain language through miraculous emergence, given that (as argued by Pinker & Bloom 1990) it has the hallmarks of being shaped by natural selection. Pinker and Bloom, however, do not offer any concrete proposals as to how language evolved. As is well known, Chomsky himself has been notably evasive on the issue of the evolution of the language faculty, often seeming to cast aspersions on the theory of natural selection (Newmeyer 1998 collects representative quotes). Chomsky is correct that other factors besides natural selection play a role in evolution, for example, the mechanics of physical bodies and the biochemistry of proteins. Nevertheless, there is nothing in these other factors that provides any helpful hints on what brought about the emergence of language.

The logic of the syntactocentric architecture suggests a reason why such evasion has been necessary. The problem is in providing a route for incremental evolution, such that some primitive version of the faculty could still be useful to the organism. In the syntactocentric architecture, everything depends on syntax. Meaning cannot have evolved before syntax, because its structure is totally dependent on the syntactic structure from which it is derived. For the same reason, phonological structure cannot have evolved before

syntax. Therefore, the complexity of syntax had to evolve before the complexity of the other components. But what would confer an adaptive advantage on a syntactic faculty that just generated meaningless and imperceptible syntactic structures? And what would enable children to acquire such syntactic structure if there were no perceptible output to which they could attach it? We quickly see that, at this very crude level at least, the syntactocentric theory is stuck: There is no logical way to build it incrementally, such that the earlier stages are useful.

The parallel architecture offers a better alternative. The system of concepts that language expresses is an independent generative component in the mind/brain. Given that this system is believed to exist, in some degree at least, in other primates as well, it also could have existed in our ancestors, prior to language. That is, our ancestors had interesting thoughts but lacked any way to say them: Meaning therefore would be the first generative component of language to emerge (a similar view is urged by Hauser 2000).

Most speculation on language evolution goes on to say that the earliest stage would have been the symbolic use of simple vocalization, without grammatical organization. Such a stage is theoretically impossible in the syntactocentric theory, because even single-word utterances have to arise from syntactic structure. But such a stage is quite natural in the parallel architecture: It consists of stored associations of vocalizations and concepts, a “paleo-lexicon.” Lexical items that can serve on their own as utterances still exist in modern language, for example, *hello*, *oops*, *ouch*, and *gadzooks*. The provision for them in language might be viewed as an evolutionary relic of this earliest stage.

Assuming that there would be an adaptive advantage to a larger number of signals, a regimentation of vocalization along the lines of phonological structure would be the next generative component of language to emerge. Phonological organization in effect digitizes vocalizations, making a large vocabulary reliably discriminable and learnable. (Proto-)words at this point would be simply duples of phonological and semantic structure, without syntax.

A next innovation might be the provision of concatenating words into larger utterances. However, when words are concatenated, the issue arises of how the meanings of words in a string are related to each other semantically. In a string like *eat apple Fred*, it is pretty clear on pragmatic grounds that Fred is eating the apple and not the reverse. But pragmatics can only go so far: in *chase lion bear*, who is the chaser? Something as elaborate as English syntax is not entirely necessary to fix this. One can actually get considerable mileage from simple functional principles of linear word order. For example, the principle “Agent First” would tell us that the lion is chasing the bear and not the reverse. Such a principle is a straight phonology-to-semantics mapping, relating linear order to semantic function. And principles like this appear to be widespread in pidgin languages (Bickerton 1981) and the grammars of speakers who have acquired their languages late in life, after the sensitive period (Klein & Perdue 1997).

Finally, principles like Agent First have their limitations, too. One can imagine the capacity for modern syntactic structure evolving *last*, as a way of making more complex semantic relations among the words of utterances more precisely mappable to linear word order in phonology. That is, syntax develops in evolution as a refinement, a “supercharger” of a preexisting interface between phonology and

semantics. This seems exactly appropriate to its function within the parallel architecture.

In short, the parallel architecture cannot tell us exactly how language evolved – I don't think anything can ever tell us that. But it does afford a far more plausible hypothesis than the syntactocentric architecture (*Foundations*, Ch. 8, develops this story in considerably more detail). Therefore, the parallel architecture opens the door for linguistics to participate far more fully in the mainstream of evolutionary psychology, yet another desirable connection.

10. Conclusions

Putting this all together, the parallel architecture makes it possible both to internally integrate linguistic theory, establishing the proper relation between phonology, syntax, semantics, and the lexicon, and also to integrate linguistic theory more comprehensively with the brain and with biology. In addition, by liberating semantics from its syntactic shackles, the parallel architecture makes it possible to develop a fully psychological theory of meaning and its relation to perception. As observed above, these points of connection were precisely what early generative grammar promised but ultimately couldn't bring off. I have tried to show here why syntactocentrism was a major reason behind this disappointment.

Of course, to propose a new architecture only begins the work. It opens major questions about exactly what components the grammar requires and what interfaces connect them. Vast numbers of phenomena have been studied in the context of the traditional architecture; to what extent can the analyses proposed there be duplicated or even improved upon? In particular, a thorough overhaul of syntactic theory is necessary to overcome decades of accretions motivated solely by syntactocentric assumptions (Culicover & Jackendoff [forthcoming] begin to undertake this task). Perhaps the hardest part of all this will be maintaining a sense of global integration, keeping the subdomains of the field in closer touch than they have recently been.

But linguistics alone cannot sustain the weight of the inquiry. We need all the help we can get from every possible quarter. And in return, one would hope that linguistic theory might be a more fruitful source of evidence and puzzles for other fields. Above all, my aspiration for *Foundations* is that it can help encourage the necessary culture of collaboration.

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NOTES

1. “Essentially the same” is a matter of perspective. When we are talking about “English speakers” as a whole we can treat them all as essentially the same. But if we're talking about dialect differences, dialect contact, or language change, we can just as easily switch to treating different speakers as having (slightly) different linguistic systems in their heads. And of course when we're talking about language acquisition, we take it for granted that the young child has a different system than the adults.

2. To some extent Chomsky's point has been lost on the larger

cognitive neuroscience community. For instance, the widely cited connectionist parser of Elman (1990) is a variation of a finite-state Markov device, and is subject to some of the same objections raised by Chomsky in 1957. See Marcus (2001) and Pinker (1999) for extensive discussion.

3. For example: “The deep structure that expresses the meaning is common to all languages, so it is claimed [by the Port-Royal grammarians – who of course did not use the term “deep structure”], being a simple reflection of the forms of thought” (Chomsky 1966).

4. Some opponents of Chomskyan generative grammar (for instance some Cognitive Grammarians) have rightly objected to syntactocentrism, but proposed instead that all properties of language are derivable from meaning. I take this to be equally misguided, for reasons that should be evident as we proceed.

5. A standard mark of recursivity is a constituent occurring within another constituent of the same type. For instance, a clause can appear within another clause: *The man who comes from New York is tall*; and a noun phrase can appear within a noun phrase: *the king of the Cannibal Islands*. In phonology this sort of situation does not occur nearly so freely: In particular, a syllable cannot occur within another syllable.

6. Interestingly, Chomsky (1965) brings up an example like (4) and analyzes the prosody as a fact of performance: Speakers don't pronounce the sentence in accordance with its syntactic structure. This is about the only way he can analyze it, given that he does not have independent principles of intonational constituency at his disposal. Contemporary theory allows us to say (correctly, I believe) that (4) is well-formed both syntactically and prosodically, with a well-formed but non-isomorphic correspondence between the two structures.

7. It is important to distinguish two interpretations of “syntactic” here. In the broader sense, every combinatorial system has a syntax: mathematics, computer languages, music, and even phonology and semantics. In the narrower sense of technical linguistics, “syntactic” denotes the organization of units such as NPs, VPs, and prepositions. I am reserving “syntactic” for this narrower sense and using “combinatorial” for the broader sense.

8. For the semantics I have used the Conceptual Structure notation of Jackendoff (1983; 1990); readers invested in other frameworks should feel free to substitute their own notations.

9. Stratificational Grammar (Lamb 1966) also proposed a thoroughgoing organization into independent generative components linked by interfaces.

10. The lexicon, a large collection of learned arbitrary associations between very particular bits of structure, also has parallels in other domains of memory. For instance, it is an arbitrary fact that the sound *kæt* means “feline animal,” and the child must learn it from the environment. Now consider the association between the appearance of foods and their tastes. It is similarly arbitrary (from the point of view of the organism) that something that looks like a cucumber tastes the way it does, and organisms learn probably hundreds or thousands of such associations. (There are even ambiguous looking foods: Think of mashed potatoes and vanilla ice cream.)

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Semantic paralysis

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Abstract: I challenge Jackendoff's claim that semantics should not be paralyzed by a failure to solve Brentano's problem of intentionality. I argue that his account of semantics is in fact paralyzed because it fails to live up to his own standards of naturalization, has no account of falsity, and gives the wrong semantic objects for words and thoughts.

There is no reason to be paralyzed by the absence of a solution for intentionality . . . (Jackendoff 2002, p. 280)

Of late, there are two big ideas at the extremes in cognitive science. One is that the mind itself, not just its referential content, extends beyond the head and into the environment (Clark & Chalmers 1998). The other is that not even the content of thoughts extends into the environment, for that requires solving the problem of intentionality – how thoughts come to be about things and mean things outside the head. Jackendoff defends this second idea in Chapters 9 and 10 of his recent book, *Foundations of Language: Brain, Meaning, Grammar, Evolution* (henceforth Jackendoff 2002). Elsewhere (Adams & Aizawa 2001), I have said why the first idea is a bad one. Here I'll say why the second idea is an unhappy one, as well.

Jackendoff accepts the following:

1. "People find sentences . . . meaningful because of something going on in their brains" (Jackendoff 2002, p. 268).
2. "There is no magic . . . we seek a thoroughly naturalistic explanation [of meaning] that ultimately can be embedded in our understanding of the physical world" (p. 268).
3. "[T]he basic problem [is to] situate the study of meaning in the study of the f-mind"¹ (p. 271).
4. Meaningful f-mental entities in cognition direct attention and make judgments on the world as perceived through the senses (p. 271).
5. Meaningful f-mental entities in cognitive processes connect linguistically conveyed messages with one's physical actions (p. 272).

Jackendoff also signals a departure from Jerry Fodor's views. Fodor (1990) wants syntactic items in the language of thought (LOT, Fodor's version of f-mind) to represent things – entities in the world. The meaningful entities, in this view, are meaningful *because* they represent things, are *about* things in the world. "Naturalized semantics" is all about how purely natural conditions and natural causes can make this happen (make things in the head mean or be about things outside the head). Finding a satisfactory account of the relations between the representing item and the represented is notoriously difficult. Jackendoff finds it so difficult ("one cannot make naturalistic sense of intentionality" [p. 300]) that he is ready to throw in the towel ("there is no physically realizable causal connections between concepts and objects" [p. 300]). He says:

Fodor's problems arise from treating the combinatorial structures that constitute meanings/thoughts as symbols for something, representations of something, information about something. Instead, I am going to try to take them just as pure non-intentional structure . . . with

phonology and syntax. The problem will then be to reconstruct the intuitions that the notion of intentionality is supposed to account for. (p. 279)

Jackendoff thinks that one can simply push "the world" into the head as a conceptual structure or reconstruction, and dispense with the hard problem of naturalizing semantics in terms of causal relations to an external world (p. 303ff). He spends a good deal of Chapters 9 and 10 explaining why his *constructivist semantics* is not guilty of *solipsism*. Nevertheless, I think that he should leap at the chance for solipsism. After all, solipsists may wonder whether there is a world beyond their minds, but at least their terms have perfectly stable semantic contents. Their worry is largely epistemological ("How do I know there is more than just me?"), but the semantics of the terms in which they worry are perfectly ordinary meaningful terms. "Tree" means tree when they wonder whether there really are trees.

What would symbols in the conceptual semantics of Jackendoff mean? He says "A speaker (or thinker) S judges that a term or phrase in the f-mind refers to an entity E in the world conceptualized by S" (p. 304). He is proposing a mapping from terms in the f-mind to other objects in the f-mind E, where the first set of objects are the representational vehicles and the second set *are the meanings*. This way we don't have to worry about counterfactuals or causal chains or what information is: "in a conceptualist theory, reference is taken to be . . . dependent on a language user" (p. 304). Jackendoff retreats to the friendly confines of the head because this will somehow make semantics easier and because the conceptual structures inside the head "do exactly the things meaning is supposed to do" (p. 306). Language is meaningful because it connects to such conceptual structures. As per the numbered list above, we just have to construe "world" and "physical" as referring to conceptual structures of a mental model when we do the semantics of terms in the f-mind.

So why is this not going to work? I have space to state only a few reasons (but there are more). First, even for objects inside the head, Jackendoff has to give the naturalistic conditions under which one object represents another. He gives none. Second, what he does say violates his own principle (2) above. How can the origin of reference depend on a language user, unless there is already language with meaning to be used? *It would be magic* to invoke meaning in the explanation of the origin of a system of language use. Naturalized accounts of meaning must avoid magic.

Third, since everyone is familiar with Searle's (1980) example of the Chinese Room, through reference to it I can register my strongest complaints. Jackendoff admits that "On this picture our thoughts seem to be trapped in our own brains" (p. 305), but things are even worse – as if that weren't bad enough. There is no sense in calling what is trapped in the brain *thoughts*. At most there are structures, perhaps even information-bearing structures delivered by the senses. But there seems little reason to think these are more than semantically uninterpreted squiggles and squoggles (in Searle's terminology) that come in through the sensory oracles. They might as well be Chinese characters to non-Chinese speakers.

Here is an example: I see beer and say "beer, please" because I want a beer. Now in Jackendoff's view there is in the f-mind a syntactic object that I would call my symbol for beer. He can't call it that because it is not a symbol *for beer*. It is a symbol for a perceptual structure that may occur in me in the presence of beer (but also may not). There is no nomic semantic intentional relation between "beer" and beer in his picture. Normally we would say that it was because I wanted beer that I said "beer, please." It was because of the semantic content of my thought (there is beer here) that I intentionally tried to order beer. Thoughts do that. They cause things like behavior because of their contents and they derive their contents, at least in part, from their causal connections to their environments. And they can be falsely tokened – I could mistakenly have thought there was beer.

Now, how can any of these things constitutive of *thoughts* be

true of Jackendoff's conceptual structures? They can't. Take just the last case. If I apply "beer" to the structure conceptualized by me *now present in my head and that normally is tokened in the presence of beer* (but which can be tokened whether or not there is beer actually nearby), how could my thought be false? It can't. There is no mismatch with *my reality* and *no falsity according to me*. So it is not *thoughts* that are trapped in the brain, according to Jackendoff's picture. Thoughts really can *be false* (not just *conceived false*, whatever that comes to in his semantics [p. 329]).

Finally, at the end of the day one often wants a beer. In Jackendoff's proposal, what one actually wants is a beer percept or an as-perceived-beerly-by-me conceptual structure to be tokened. Not for me – I just want a beer.

NOTE

1. Editor's note: "f-mind" stands for "functional mind" (Cf. *Foundations*, p. 21).

On the role of frame-based knowledge in lexical representation

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Abstract: In this commentary I discuss the role of types of knowledge and conceptual structures in lexical representation, revealing the explanatory potential of frame-based knowledge. Although frame-based lexical semantics is not alien to the theoretical model outlined in Jackendoff's conceptual semantics, testing its relevance to the analysis of the lexical evidence presented in his book has been left out of consideration.

Through the years, Jackendoff's approach to describing lexical representation and characterizing the nature of lexical storage and retrieval has been strictly conceptualist. However, in *Foundations of Language* (Jackendoff 2002) he has not addressed several factors of the mental representation of lexical information extensively, and consequently, various important details have remained unexplained or have been overlooked. One of these concerns the relation between the linguistic (i.e., the "dictionary") versus the encyclopedic meaning of lexical items; that is, as Jackendoff refers to them in discussing the views of others, their semantic versus pragmatic potential (Jackendoff 2002, pp. 285–86). I would argue that in discussing this conceptual facet of lexical representation we are not strictly facing meaning. Rather, in my view, we are facing here various types of knowledge and their conceptually based role in lexical representation and the mapping of meaning. As outlined by Clark (1992; 1996) and by Andor (1985; 2003), however, the relation does not only hold between "dictionary" (i.e., lexical) and encyclopedic types of knowledge, but is manifold and can occur as a result of the interaction of multiple types of knowledge, including generic and private or socio-cognitively based communal and expert knowledge during communication. All of these types of knowledge contribute to the common ground shared by speakers of a linguistic community (Andor 1985; 2003; Clark 1992; 1996, p. 92–121). In *Foundations*, Jackendoff does not address in detail the complex issue of the relation between these types of knowledge based on empirical evidence. For instance, how exactly does encyclopedic knowledge, a body of stereotypically-based knowledge, serve as a source for lexically represented knowledge? Conversely, does the latter type of knowledge serve as a source for the saturation of lexical meaning embodied by the lexical items represented in a given language?

Nor is the issue of the role of frame-based, scenic and scriptal knowledge in lexical storage and retrieval, as well as in the representation of lexical and encyclopedic knowledge types, discussed, although Chapters 9, 10, and 11 abound in traces of this domain.

Jackendoff refers to difficulties in separating domains of encyclopedic and lexical semantics, for instance, in clarifying the difference between the lexical meanings of *murder* and *assassinate*. He argues that the "latter implies a political motive on the part of the agent" (Jackendoff 2002, p. 286), but fails to identify the real core of difference: These verbs belong to the lexical networks in the representation of different conceptual scenes and frames, and thus have different scripts of associated performance in their conceptual makeup.

This is an important issue to be taken into account in studying the criteria and borderlines of synonymy. Although words that are members of a given lexical field may fall into different types of synonym sets, some of them may be freely substitutable by another member of the same field and may even show the same patterns of syntactic alternations, and hence be identified as absolute synonyms; others in the same domain may be near or partial synonyms only (Cruse 2000, p. 156–60). Absolute synonymy is known to be quite rare. According to Jackendoff, items are synonymous in case they are mutually subordinate (1983, p. 104). But perhaps the most important issue concerning the set of criteria of synonymy has been overlooked by researchers of the field: Although lexical items belonging to a given lexical field may share similar denotational, categorical, subcategorization, and perhaps even selectional and relational features (i.e., argument structure), they may still reveal different grades of distance in prototypicality due to differences in their frame relatedness and the scriptal makeup of their background concepts. The higher the frame dominance, the greater the distance from the prototypical instance within the given lexical domain, and the looser the synonym relatedness to other members of the field.

This can be tested experimentally. For instance, within the domain of verbs of cutting, *mow*, *trim*, and *prune* are quite distant from *cut*, the prototypical member of the group, whereas *slice* is nearer. Concerning verbs of jumping, *bounce* is lower down in the gradience of prototypicality than are *spring* and *hop*, whereas *prance* and *dance* are even further away from the prototypical member *jump* in this lexical domain. Features of categories and their lexical representation in a certain domain occur as clusters, as pointed out by Jackendoff and others. However, an important property is overlooked: The more types and kinds of features are shared by members, the higher the rate of prototypicality manifested, but at the same time, a high coincidence of feature clusters results in a lower rate of frame dominance. In Jackendoff's view "the prototype is simply an instance that happens to maximally satisfy the cluster conditions" (2002, p. 356). I believe that the role of the prototype lexical concept in a lexical field is more marked: It is the item that provides the criteria of coherence within the lexical domain and sets boundary conditions on membership in its lexis.

Finally, let me briefly address Jackendoff's approach to the interesting issue of frame-based reference, the case of frame-based lexical items. In his conceptualist view, "reference is taken to be at its foundation dependent on a language user, . . . being in the *real* world is *not* a necessary condition" (2002, p. 304). Such is the case of *unicorns*, *dwarfs*, *trolls*, *goblins*, *chimera*, and so forth. All such entities require some rate of conceptualization, as Jackendoff suggests, in at least some minimal way to gain reference (2002, p. 304). However, he fails to provide adequate terminology for such cases of items. As frames are types of conceptual structures which are based on global and stereotypical information, are dominantly dependent on encyclopedic knowledge, and are acquired in lack of direct exposure to empirical experience contrary to scenic knowledge (Andor 1985), the above lexical items are typically acquired and retained in memory on such grounds. A great many lexical concepts such as *marmots*, but even *tigers* or *cows* may first be acquired on such grounds, and then, based on exposure to direct experience, scenic knowledge, their content is modified and standardized upon speakers' gaining full lexical competence. Thus, their feature makeup may show analogies to those acquired on the basis of scenic knowledge.

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brain, Meaning, Grammar, evolution

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Abstract: I reject Jackendoff's view of Universal Grammar as something that evolved biologically but applaud his integration of blackboard architectures. I thus recall the HEARSAY speech understanding system - the AI system that introduced the concept of "blackboard" - to provide another perspective on Jackendoff's architecture.

The subtitle "Brain, Meaning, Grammar, Evolution" for *Foundations of Language* (Jackendoff 2002) suggested that Jackendoff would devote major portions of his book to brain and evolution. Alas, there is no serious discussion of the brain (beyond a few passing references to aphasia) and the discussion of evolution (Ch. 8) focuses on an incremental account of Universal Grammar (UG) that ignores brain evolution. Space does not permit proper discussion of the brain here. Instead, I lament Jackendoff's view of Universal Grammar as something that evolved biologically; and then recall the HEARSAY speech understanding system to provide another perspective on Jackendoff's architecture.

Concerns about Universal Grammar. Jackendoff (2002, p. 263) views UG as "the unlearned basis from which language is learned" and argues that "it had better be available to help children learn case systems, agreement systems, fixed word order, and grammatical functions in case the language in the environment happens to have them."

I find this view incoherent if it implies that evolution yielded adaptations specific to each of these systems. What selective pressure would cause humans whose language does not use cases to evolve a brain with a device *specialized* for learning case systems?! Instead, I think we should seek to understand what made the brain "language ready," providing capacities that make possible the discovery of Jackendoff's language "components" over the course of many millennia, and their acquisition by the child over the course of a few years. One listing of such capacities (based on Arbib 2002b) follows:

Complex imitation: the ability to recognize another's performance as a combination of familiar movements and then repeat it.

Symbolization: The ability to associate an arbitrary symbol with a class of episodes, objects or actions. (These symbols may have been unitary utterances, rather than words in the modern sense, and may have been based on manual and facial gestures rather than being vocalized.)

Parity (mirror property): What counts for the "speaker" must count for the "listener."

Intentional communication: Communication is intended by the utterer to have a particular effect on the recipient, rather than being involuntary or a side effect of praxis.

From hierarchical structuring to temporal ordering: Perceiving that objects and actions have sub-parts; finding the appropriate timing of actions to achieve goals in relation to those hierarchically structured objects.

Beyond the here-and-now: The ability to recall past events or imagine future ones.

Paedomorphy and sociality: A prolonged period of infant dependency combines with social structures for caregiving to provide the conditions for complex social learning.

In hindsight we may see these as preadaptations for language

but they were adaptive in their own right, and underlie many modern human capacities other than language. In this view, Universal Grammar is only tenable as a descriptive umbrella for the immense variety of human languages, not as a "genetic reality" or "neural reality" that implausibly contains all possible grammatical structures in embryo (one is reminded of the "little man" that seventeenth century spermists "saw" inside the head of the spermatozoon [Pinto-Correia 1996; 1997]). I applaud Jackendoff's attempt to provide an evolutionary sequence for language but argue (e.g., Arbib 2002b) that case systems, agreement systems, and so on, are to be seen as human inventions that required no change in brain structure for their discovery and cultural transmission. Moreover, I see these as coarse grain compared to the actual inventions that were made across the millennia and which eventually coalesced into the more-or-less coherent structures that Jackendoff and other linguists tend to treat as natural and indivisible. What is universal is the need for expression, not the choice of linguistic structure for meeting those needs. The evolution of language from protolanguage is part of the history, not the biology, of *Homo sapiens*.

Déjà-entendu. Jackendoff makes much of the AI notion of blackboard in presenting his architecture for language, but does not cite HEARSAY-II (Erman et al. 1980; Lesser et al. 1975), perhaps the first AI system to develop a blackboard architecture. While obviously not the state of the art, it is of interest because it foreshadows features of Jackendoff's architecture. Digitized speech data provide input at the *parameter level*; the output at the *phrasal level* interprets the speech signal as a sequence of words with associated syntactic and semantic structure. Because of ambiguities in the spoken input, a variety of hypotheses must be considered. To keep track of all these hypotheses, HEARSAY uses a dynamic global data structure, called the *blackboard*, partitioned into various levels; processes called *knowledge sources* act upon hypotheses at one level to generate hypotheses at another.

First, a knowledge source takes data from the *parameter level* to hypothesize a phoneme at the *surface-phonemic level*. Many different phonemes may be posted as possible interpretations of the same speech segment. A lexical knowledge source takes phoneme hypotheses and finds words in its dictionary that are consistent with the phoneme data - thus posting hypotheses at the *lexical level* and allowing certain phoneme hypotheses to be discarded. To obtain hypotheses at the *phrasal level*, knowledge sources embodying syntax and semantics are brought to bear. Each hypothesis is annotated with a number expressing the current confidence level assigned to it. Each hypothesis is explicitly linked to those it supports at another level. Knowledge sources cooperate and compete to limit ambiguities. In addition to data-driven processing which works upward, HEARSAY also uses hypothesis-driven processing so that when a hypothesis is formed on the basis of partial data, a search may be initiated to find supporting data at lower levels. A hypothesis activated with sufficient confidence will provide the context for determination of other hypotheses. However, such an *island of reliability* need not survive into the final interpretation of the sentence. All we can ask is that it forwards the process which eventually yields this interpretation.

Hanson and Riseman (1987) based the architecture of their computer vision system VISIONS on the HEARSAY architecture as well as neurally inspired schema theory (Arbib 1981; Arbib et al. 1998). Such a conceptual rapprochement between visual perception and speech understanding offers a computational framework for further exploration of the Saussurean sign (Arbib 2003; Hurford 2003). Arbib and Caplan (1979) discussed how the knowledge sources of HEARSAY, which were scheduled serially, might be replaced by schemas distributed across the brain to capture the spirit of "distributed localization" of Luria (e.g., Luria 1973). Today, advances in the understanding of distributed computation and the flood of brain imaging data make the time ripe for a new push at a neurolinguistics informed by the understanding of distributed computation. Despite its disappointing inattention to the brain, Jackendoff's book could make a valuable contri-

tribution to this effort by showing generative linguists how to break out of the straitjacket of syntactocentrism by integrating their work into a rich multi-modal architecture.

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Language evolution without evolution

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Abstract: Jackendoff’s major syntactic exemplar is deeply unrepresentative of most syntactic relations and operations. His treatment of language evolution is vulnerable to Occam’s Razor, hypothesizing stages of dubious independence and unexplained adaptiveness, and effectively divorcing the evolution of language from other aspects of human evolution. In particular, it ignores connections between language and the massive discontinuities in human cognitive evolution.

I approach Jackendoff’s ambitious and many-faceted *Foundations of Language: Brain, Meaning, Grammar, Evolution* (Jackendoff 2002) as an unashamed syntactocentrist. Jackendoff, however, is far from being that, and the main example he picks to illustrate syntactic relations could hardly have been better chosen had he deliberately intended to marginalize and trivialize syntax:

(1) *The little star’s beside the big star.*

This sentence, first analyzed on pages 5 through 6, is returned to repeatedly throughout the text.

But, copular sentences like (1), sentences with the verb “to be,” form a small and highly idiosyncratic subset of sentences; their properties differ sharply from those of the vast majority of sentences. The latter describe actions, events, or a variety of states, and deploy a rich variety of argument structures; copular sentences express only identity, location, or the attribution of qualities (The rose is red/in the vase/a Molly Perkins) and take only a *theme* argument. In a non-copular clause, no two noun-phrases will have the same referent (unless a specifically reflexive form such as himself is used), and transposition of noun-phrases inevitably changes meaning:

(2) a. *John hit the captain.*
b. *The captain hit John.*

In copular clauses, no two noun-phrases will have different referents; consequently, transposition of noun-phrases inevitably leaves meaning unchanged:

(3) a. *John is the captain.*
b. *The captain is John.*

There are many more syntactic relations that can’t be illustrated via copular sentences, too many to list here. Perhaps in his response to commentary Jackendoff will tell us why he chose such an atypical sentence as his prime syntactic exemplar.

Much more could be said about Jackendoff’s treatment of syntax, but I must reserve the bulk of this commentary for his chapter on language evolution. Right off, Jackendoff confuses the issues with a straw-man version of “the common view of Universal Grammar” (p. 233). According to him, that view treats phonology and syntax as “passive handmaidens of syntax” that could not, therefore, have evolved prior to syntax. But syntax without phonology and semantics would be useless, so this view is absurd.

In fact the current status of semantics and phonology (whatever that may be) carries no entailment for their order of evolution. No one disputes that apes and hominids had some sort of conceptual structure, therefore semantics (in some form) had to precede syntax (indeed, this is made quite explicit in my own writings, from Bickerton 1990 on). As for phonology, this (at least in some primitive form) was presumably present in protolanguage,

which had no syntax. But the emergence of syntax selected for a sophisticated phonology, while the capacity to assemble semantic units into complex propositions radically expanded conceptual structure.

Jackendoff then turns to the proposal of Bickerton (1990) that language developed in two steps, an syntactic protolanguage and syntactified modern language, and instead opts for “a more graceful, incremental evolution” (p. 236). But are the incremental stages he proposes really stages at all?

Take the three stages: (1) “use of symbols in a non-situation-specific fashion,” (2) “use of an open, unlimited class of symbols,” and (3) “development of a phonological combinatorial system” that supposedly intervene between an alingual state and protolanguage. No real difference exists between the first two. A symbol freed from the here and now has to be cultural rather than biological; if you can invent one, you can invent an unlimited number. A protolanguage adequate for the needs of hominids two million years ago wouldn’t have needed many. Nothing suggests that an insatiable demand for new symbols would have driven the emergence of a phonological combinatorial system.

As Jackendoff is well aware, at least one current framework (Optimality Theory) proposes “a united grammatical framework for syntax and phonology” (Smolensky 1999). Whether or not one buys the theory itself, it seems highly likely that language’s two combinatorial systems came in together, perhaps exploiting some single underlying capacity, but more likely with phonology employing mechanisms derived directly or indirectly from syntax. This pushes the third of Jackendoff’s stages to a post-protolanguage position.

“Concatenation of symbols” is supposed to constitute another intermediate between call systems and protolanguage. But since “language-trained” apes appear to have concatenated symbols with no explicit training and minimal modeling, why is this stage not implicit in the development of symbols? And why invoke, as a distinct stage, “use of symbol position to convey basic semantic relations”? In every variety of protolanguage I know of, such use is not principle-based but merely a statistical tendency. The real evolution in language was not from unordered symbols to regularly ordered symbols to modern syntax. It was from concatenation in linear strings to concatenation in hierarchical structures (Bickerton 2002). Between these two types there is no intermediate, therefore, not even the possibility of a gradual evolution from one to the other.

Regarding post-protolanguage changes, I have already conceded (Bickerton 2000, sect. 4) that the original two-stage model has to be supplemented by a third stage, the grammaticization of a morphologically bare syntax to enhance parsability. I see no point in arbitrarily dividing this third stage into several sub-stages, as Jackendoff does in his Figure 8.1, especially as Creole languages quickly create both grammatical (albeit unbound) morphology and symbols encoding semantic relations through demotion of regular lexical items. Moreover, each hypothetical stage requires its own selectional history; it will not do merely to suppose that any improvement in a system is automatically selected for.

Whatever its defects, the three-stage model sought to ground itself in known human-evolutionary developments and anchor itself at least provisionally in time. Jackendoff rejects these constraints (explicitly, in the case of time) in the belief that they “make little difference” (p. 236). I’m sorry, they make a lot of difference.

The most striking fact about human evolution is the massive cognitive and behavioral difference between our species and all antecedent species. Moreover, most writers agree that language was strongly contributory to, if not wholly constitutive of, that difference. But if language was evolving gradually over a long period, as Jackendoff’s account implies, then why did improvements in language yield no apparent changes in cognition or behavior until the last hundred thousand years?

The gross mismatch between the archaeological record and any gradualist account of language evolution is something that linguists and nonlinguists alike have been studiously avoiding or

evading ever since I pointed it out more than a decade ago (Bickerton 1990). The cognitive discontinuity between humans and prehumans precisely mirrors the linguistic discontinuity between linear and hierarchical concatenation. Can this be mere coincidence?

Whether it is or not, any gradualist account of language evolution that does not even try to explain why, if language evolved gradually, human cognition and behavior did not evolve equally gradually has little explanatory value. I do not wish to single out Jackendoff in this respect. He himself says, "I see no need at the moment to hold myself to a higher standard than the rest of the field" (p. 237). But if somebody doesn't do just that, we might as well give up on language evolution.

Why behavior should matter to linguists

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Abstract: Jackendoff's *Foundations of Language: Brain, Meaning, Grammar, Evolution* has many points of similarity with Skinner's analysis of verbal behavior, though the former emphasizes structure whereas the latter emphasizes function. The parallels are explored in the context of a selectionist account of behavior in general and of verbal behavior in particular. Part of the argument is that behavior drives evolution and therefore also drives brain organization. Another concerns itself with the nature of explanation. Recent experimental developments in behavior analysis are reviewed as potential contributions to an understanding of language that incorporates its functional as well as structural dimensions.

It is easy to see where the constructive collaboration Jackendoff invites in his Preface (Jackendoff 2002) can be offered; but to present the relevant material within brief compass is hard. Despite many affinities outlined below, I argue that more is to be gained by focusing on how linguistic structures can be illuminated by behavioral functions than by using linguistic structures to illuminate hypothetical brain mechanisms.

It helps that Jackendoff places his account firmly within an evolutionary context, because evolution is driven by behavior. Whether an organism survives and reproduces depends on what it can do and the conditions under which it does it. Its environment consists not only of the physical world but also members of its own and other species. Its brains and muscles and other organ systems all evolved in the service of its behavior. Therefore, it is a reasonable proposition that behavior drives brain structure, not only through evolutionary contingencies that select behaving organisms with certain kinds of brains, but also through environmental contingencies that shape different patterns of behavior and alter brains within the lifetimes of individuals. Jackendoff acknowledges this when he states that "perceptual systems have evolved in order that organisms may act reliably in the real world" (p. 308).¹ But if behavior drives brain organization, behavior is the place to start (Catania 1972; 1995b; 1997; Catania & Harnad 1988; Skinner 1988).

Let us first dispose of some common misconceptions. Behavior is not defined by muscle movements or by glandular secretions. It is defined by function rather than form. Shifts of attention are behaviors, for example, even without overt movement; what matters is that they are modified by their consequences. So, also, are seeing and looking. You can look without seeing and see without having looked; both are subject to contingencies and either can occur in the absence of visual stimulation (Jackendoff calls these actions *percepts*, as in his bug example on pp. 311–12, but thinking of them as actions rather than states has advantages).

In biology, studies of structure and function are respectively called anatomy and physiology. Their priorities were once an issue (Russell 1916). Behavior also has both structure and function. For

example, when a horse runs, muscle flexions combine to produce coordinated leg movements that change with shifts from one gait to another. All gaits, either natural (trotting) or trained (the rack), are constrained by neurophysiological and mechanical factors and constitute a grammar of the horse's running. But that grammar is orthogonal to function: for example, when and where the horse runs; with which gait; what consequences follow. As organs differ in anatomy and physiology, so also varieties of behavior differ in what they look like and what they do. A horse may overtake another at lope or gallop, and gallop in overtaking others or in escaping from predators. In the former, actions of different form have similar functions; in the latter, actions of similar form have different functions. Language too has both structure and function.

Beyond the structure-function distinction is the issue of selection. Within individual lifetimes behavior is selected by its consequences, much as organisms are selected over generations by evolutionary contingencies. Operants, classes of behavior selected by their consequences, are fundamental units of behavior defined by function. All operants participate in three-term contingencies in which discriminative stimuli set occasions on which responses have consequences (e.g., at traffic intersections, the consequences that follow from stepping on the gas or the brakes vary with the colors of the traffic lights). Parallels between natural selection in phylogeny and in ontogeny have been explored in detail (Catania 1973a; 1973b; 1987; 1996b; Skinner 1935; 1938; 1981; Smith 1986). Behavioral accounts are often identified with S-R associations, but behavior analysis is a selectionist rather than associationist account (for a more detailed discussion, see Catania 1998; 2000).

The poverty of the stimulus (Chomsky 1959; Crain 1991) takes on a different aspect in the context of selection. The selection of operant classes by their consequences does not depend on extensive sampling of negative instances. Consider the evolutionary analogy: Populations are not selected from pools exposed to all possible environments, and not all variations are included in the pools upon which selection operates. It remains reasonable to consider structural constraints on what is selected, but those constraints do not negate genealogy. As Darwin made abundantly clear, both structure and function must be viewed through the lens of selection.

Other biological analogies are also relevant. For example, organisms have been characterized as theories of their environments. Jackendoff exemplifies this view when he pushes the world into the mind. But it is a risky move (Andresen 1990), and parallel moves in biology have not fared well. For example, genetic material is no longer said to carry blueprints of organisms, nor does it reveal properties of the environments within which it was selected; it is instead best regarded as a recipe for development (Dawkins 1982). It is, similarly, a useful move to think of what is remembered as a recipe rather than a blueprint for recall.

With these preliminaries, let us compare Jackendoff and Skinner. In this undertaking, it is on the one hand not reassuring that Jackendoff disposes of behaviorism with a 1913 reference to John B. Watson (p. 280) and comments on Skinner only in passing without citation (p. 372). On the other hand, it is intriguing that so many of Jackendoff's distinctions and categories have clear parallels in Skinner's (1957) account. Both present modular systems and their modules are necessarily heterogeneous (cf. Jackendoff 2002, p. 160). Both consider how the modules can arise and how they are coordinated with each other. When Jackendoff says "reading, for example, acts like a module in an accomplished reader, but it requires extensive training for most people in a way that the phonology-syntax module does not" (p. 227), he parallels Skinner's textual, tact and echoic classes of verbal responses. Consistent with the status of operant classes, Skinner's modules are based on considerations of function rather than form: "we cannot tell from form alone into which class a response falls" (Skinner 1957, p. 186).

Both Skinner and Jackendoff wrestle with the problem of defining verbal classes in terms of reference or meaning or environ-

mental determinants, arguing that nouns and verbs cannot be viewed as words corresponding to things and actions, respectively. Both are concerned with how terms for nonexistent entities (e.g., unicorns) arise as extensions from verbal units acquired in the context of real entities (e.g., horns), and with how entities that cannot be pointed to can come into existence through words (e.g., months, nations). While doing so, both also grapple with the transition from the local and concrete to the abstract and metaphorical (e.g., Jackendoff 2002, p. 299; Skinner 1957, pp. 91–116). Because his classes are defined by function, Skinner easily handles cases where members do not share physical properties: “Sometimes a genuine extension seems to occur when no similarity between stimuli expressible in the terms of physical science can be demonstrated” (1957, p. 97).

Both Jackendoff and Skinner reject chaining as a general basis for generating utterances, consistent with Lashley’s (1951) arguments about sequential structure, but both allow sequential dependencies (rote sequences) in specific instances. That Skinner made use of such a module, which he called *intraverbal*, demonstrates that he did not regard such sequential processes as universal. Both discuss ways in which sequences, first generated as chains, can become units in their own right. Both recognize that some but not all large verbal units are constructed as they are produced (constructed online versus memorized).

Both deal with single-word “primitive” verbal units, such as *hello* and *ouch* and *psst*. Both allow verbal units of varying size, from phonemes through syllables and words to sentences and larger forms. Both classify and interpret verbal devices such as metonymy and metaphor, and both use errors and pathologies for distinguishing among classes of verbal behavior.

Both are especially concerned with the coordinations that produce new verbal instances. Jackendoff deals with them in terms of multiple components and interface rules, and Skinner, in terms of multiple causation, the simultaneous determination of different aspects of an utterance by different variables (as when participants and timings and actions simultaneously determine sentence features such as nouns and verbs and tenses). Compare Jackendoff on generative components:

What is new in the present approach is that the idea of multiple generative components . . . has been extended in thoroughgoing fashion to every part of the grammar, so that it becomes a fundamental architectural design principle. (Jackendoff 2002, pp. 129–30)

and Skinner on the active editing of ongoing speech:

In the processes of composition and editing the speaker arranges, qualifies, withholds, or releases verbal behavior which already exists in some strength in his repertoire. Much of the behavior emitted upon any occasion “just grows” – it springs from the current changing environment and from other verbal behavior in progress. (Skinner 1957, p. 228)

Perhaps most significant, both deal with the hierarchical structure of verbal behavior, and, in particular, with nestings in which higher order structures depend on the speaker’s awareness of other levels (as in specifying one’s confidence in something said in a phrase like “I am sure that . . .”). Some of these higher order units cannot stand alone. Skinner (1957, pp. 311–43) wrote of them as *autoclitic* processes – in the sense of verbal behavior that leans upon other verbal behavior – and distinguished between relational and descriptive forms further divided into qualitative and quantitative and other dimensions (Catania 1980). Jackendoff makes similar distinctions, though applying the relational and descriptive labels somewhat differently. Though both discuss structure mainly in terms of rearrangements and transformations of units, they also allow a role for frames within which units can be placed (e.g., Jackendoff 2002, pp. 63, 176; Skinner 1957, pp. 336, 346).

When Jackendoff says “we must consider the domain of linguistic semantics to be continuous with human conceptualization as a whole” (p. 282), it is reminiscent of Skinner’s argument that thinking and behavior are coextensive; when he says “the seams of the mind must be determined empirically” (p. 283), he seems to

address what Skinner (1938) called natural lines of fracture in behavior.

The commonalities have not been exhausted (e.g., Jackendoff’s activation and Skinner’s priming, or concern with verbal dimensions like tone of voice or with the fuzzy boundaries of verbal classes, or appeals to the practices of verbal communities). But it is also crucial to acknowledge the vast differences, while noting that the convergences evident in such divergent approaches may themselves be of particular significance. Having already considered their different stances on brain and behavior, I concentrate on modes of explanation.

Jackendoff often offers explanations, when what he has provided is description (e.g., pp. 336–42). But the relation between sentence and structural diagram is similar to that between sentence and paraphrase: Diagrams may make subtle structural features easier to see and may help in taxonomic development, but they do not specify where the features came from or how they work or what effects they may have (cf. Skinner 1957, p. 388, on paraphrase).

It is good that Jackendoff is explicit about rules being in the heads of linguists rather than language users: “rules are nowhere present in the f-mind in the form we write them. Rather, these rules *are* indeed just descriptions of regularities in the organization of linguistic memory” (p. 57; cf. p. 167). Structure alone cannot justify explanatory appeals to conformity (p. 171), spontaneous generation (p. 188), convenience of usage (p. 358), or insight (p. 390).

I can only touch on the problems raised when language is interpreted in terms of the metaphors of meaning and communication (Catania 1998, pp. 239–78; Lakoff 1987; Lakoff & Johnson 1980). Those ancient philosophers who thought that vision depended on something traveling from the eye to the thing seen had it backwards, but we can be similarly misled when our language of reference leads us to speak of words as referring to things in the world, and therefore to neglect the other direction, in which events occasion what we say or provide conditions under which we learn what words mean (cf. Day 1969; Wittgenstein 1953). Jackendoff occasionally seems to move in this direction: “We do not have to worry about whether the phrase really refers, only about how language users treat it” (p. 325, n. 24). Furthermore, to speak of communication as the sharing of meanings is to neglect the irreducible function of all verbal behavior, which is that it is a way in which one individual can affect the behavior of another. This is not to dispose of meaning and communication, but rather to recognize that both are derivatives of that more fundamental function (Catania 1990; 1991; 1995b; 2001).

With regard to description as a form of explanation, Jackendoff’s statement that “We can determine properties of ‘language in the world’ only through its manifestations in human linguistic intuition and behavior” (p. 298) seems to share something with Skinner’s (1957, p. 6) remark that: “There is obviously something suspicious in the ease with which we discover in a set of ideas precisely those properties needed to account for the behavior which expresses them.”

Skinner, instead, looks to the environment. In accounting for the difference between offering a teapot to Nancy and offering Nancy a teapot (p. 54), we need to know whether a teapot was offered to Nancy, not Jane, or whether Nancy was offered a teapot, not a teacup. The practices of verbal communities will be more likely than brain structure to tell us whether the Frisbee on top of the house has been roofed or rooved (p. 158). Though it is unusual to say “John stayed the same distance from me he always stays” (p. 321), the sentence may tell us more about how often we interact with people with bodyguards than about how in general we talk about distance. Verbal religious practices will tell us more about the truth value of “God is a trinity” or “God is a man” than will questions about how these sentences relate to the world (p. 294). And the circumstances under which people say or respond to the word “stop” may be more important than whether the word should be regarded as a symbol (p. 239). If the above instances are to be

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

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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

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

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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; Halldane 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

brother). Clearly, goes the argument, the idiosyncrasies of imagery or other mental processes we each indulge in are irrelevant to the issue of semantics – the word-world relation that specifies, somehow, the set of objects in the world correctly referred to by “uncle,” “sue,” and “stockbroker.” So, we cancel out all the conflicting and irrelevant mental states and processes and leave the messy minds out of semantics altogether. In any case, since we semanticists have to get all the way to the world in the end, it won’t do to stop short in the mind (or the brain), so why tarry?

This is strikingly like the justification that has been offered by evolutionists for habitually ignoring developmental biology: We choose to go from the gene directly to the adaptation, the phenotypic structure or behavior that is actually selected for, because that is, in evolution, where the rubber meets the road. A gene for *x*, a gene for *y*, and we can postpone indefinitely the tricky job of charting the winding path from gene transcription to operational phenotypic asset. This is in fact a very valuable simplification, but it can be overdone. Reacting against it – today’s “evo-devo” bandwagon – can overshoot, too.

Jackendoff says, in italics, “*it is necessary to thoroughly psychologize not just language, but also ‘the world’*” (p. 294) and adds: “*the perceptual world is reality for us*” (p. 308). As he recognizes, this looks as if he’s stopping semantics in the brain, saddling his brilliant view of language with some weird sort of materialistic idealism. Let me try to put the matter more mundanely. Most people go through life without ever giving semantics any thought. You don’t have to figure out the semantics of your own language to use it, but if you do try to, you soon discover the set of issues that exercise Jackendoff. It helps keep the quandaries at bay to go *hetero-*, to do the semantics of some other guy’s language (and mind). Like this:

The words of his language refer to things. We mustn’t presuppose that his semantic system matches ours – the meta-language we use to *describe* his psychology. If we want to say what his words refer to, we have to see how his brain is designed by evolution (including cultural evolution) and by individual learning, to parse out his perceptual and conceptual world. Once we’ve done this we can ask: Do his terms refer to things in the world *as we parse it*, or “just” to things in the world as he experiences it (and as his conspecifics and companions experience it)? (For if there is a language, there is a shared system even if it isn’t *our* shared system.) If the former is true, then we share the world with him; our *manifest image* (Sellars 1963) is (roughly) the same as his, and theirs. If not, then we have to maintain something like scare-quotes when we refer to the “things” in his world. But either way, we eventually get all the way out to the world – where the rubber meets the road. What we can’t *express* in our terms, we can *describe* in our terms.

Jackendoff insists, rightly in my opinion, that it is only by taking this indirect path that analyzes the manifest image implicit in the language-users’ brains that we can complete the task of linguistics. For most purposes, however, we can continue using the traditional semantical talk about the word-world relation, just as biologists can continue to talk about genes for myopia or even dyslexia (Dawkins 1982; Dennett 1995), because we know how to take the longer, more complicated path when necessary.

A conceptuocentric shift in the characterization of language

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Abstract: Recognizing limitations of the “syntactocentric” perspective, Jackendoff proposes a model in which phonology, syntax, and conceptual systems are each independently combinatorial. We can ask, however, whether he has taken this issue to its logical conclusion. The fundamental

question that is not fully addressed is whether the combinatorial aspect of syntax originated in, and derives from, the indeed “far richer” conceptual system, a question to be discussed.

In *Foundations of Language*, Jackendoff (2002) has undertaken what is finally a rather profound reconfiguration of the generative framework in a manner that allows a potentially much more interesting interaction with related aspects of the other cognitive sciences. Recognizing limitations of the “syntactocentric” perspective, in which the free combinatoriality of language is attributed to syntax alone, Jackendoff proposes to correct the situation by promoting a model in which phonology, syntax, and the conceptual system are each independently combinatorial.

Of particular interest is the status of the conceptual system as a “combinatorial system independent of, and far richer than, syntactic structure” (p. 123) in the parallel architecture, and the resulting questions concerning the functional relation between the conceptual and the syntactic components. In this aspect, Jackendoff has initiated an interesting debate, but in a certain sense he has failed to take his position to its logical conclusion. The fundamental question that is not fully addressed is whether the combinatorial capability originated in the indeed “far richer” conceptual system. This is consistent with the consideration that language arose primarily to enhance communication (p. 236) of thoughts, which assumes the precondition of a combinatorial conceptual structure system (p. 238).

If the combinatoriality of language serves the purpose of transmitting messages constructed from an equally combinatorial system of thoughts (p. 272, and Ch. 3), then the precedence for combinatoriality appears to lie in the thought or conceptual system. In this case, it would have been more interesting to see Chapter 3 on combinatoriality organized around the combinatoriality of the conceptual system, with an analysis of the extent to which the combinatoriality of syntax derives from that of its predecessor.

In any event, Jackendoff’s view of the conceptual system invites one to consider things from a more conceptuocentric perspective. Indeed, Jackendoff notes that (p. 417) “languages differ in their syntactic strategies for expressing phrasal semantics; but the organization of what is to be expressed seems universal,” again suggesting that the origin of the universal combinatorial capacity lies more in the independent combinatorial capability of the conceptual system than in syntax. In this context, one could consider the syntactic integrative processor as an algorithm for reading or traversing the conceptual structure data structure in order to generate a linear string that would be processed in parallel by the phonological integrative processor. In this sense, the observed generative component of syntax would derive from that of the conceptual system. Indeed, on page 417 Jackendoff indicates that “what is part of Universal Grammar, of course, is the architecture of the interface components that allow conceptual structures to be expressed in syntactic and phonological structures.” The interesting part of what is universal then, is the conceptual system and its interfaces.

If this were the case, then the syntactic integrative processor would perform an interface between conceptual and phonological structures. This perspective focuses on the relation between the structure of language and the structure of meaning, more than the syntactocentric approach does. In this context, one would expect a certain degree of isomorphism between conceptual structures and the linguistic structures that communicate them. Jackendoff thus notes that for “simple compositional” structure based on argument satisfaction, modification, and lambda extraction and variable binding, there is a “close correspondence between the configurations of lexical items in syntax and conceptual structure” (p. 387). Enriched composition such as the reference transfer depicted in Nunberg’s (1979) sentence “The ham sandwich over in the corner wants more coffee” manifests situations in which this iconicity is claimed to break down. Indeed, the development and use of this type of “verbal shorthand” will lead to the development of grammatical constructions that partially circumvent iconicity, here simply referring to an individual by his or her most contex-

tually salient property, his or her restaurant order. Still, from a developmental perspective, we should explore how far the infant can go with the “close correspondence” hypothesis.

The issue of the source of compositionality in the conceptual system or in syntax is not a trivial issue, as it has massive impact on learnability. Learnability issues generally evoked in the “poverty of stimulus” framework focus largely on the complexity of inducing regularities derived from syntactic compositionality. This complexity could be significantly reduced if the compositionality were already present in the conceptual system. In this context, acquisition does not necessarily imply that the child perform a (demonstrably impossible) task of grammar induction on reduced input (see target article, sect. 4.6). Rather, it implies that the child learns how to interpret the meaning of sentences by any method. In his discussion of lexical storage versus online construction (p. 188) Jackendoff outlines an approach in which the infant initially is “storing everything,” and begins to generalize regular patterns “and extract explicit patterns containing typed variables;” allowing the system to “go productive,” via variable-based structures similar to those discussed by Marcus (2001). The resulting lexical construction-based developmental trajectory described in section 6.9 makes interesting contact with the usage-based account of language acquisition as developed by Tomasello (1999b; 2003). In making this connection, Jackendoff has quietly performed a remarkable stunt in theoretical diplomacy, by (at least partially) integrating the construction grammar framework into the parallel architecture.

What becomes interesting from this dual perspective of (1) the combinatorial precedence of the conceptual system, and (2) the use of a construction grammar style approach as suggested in Chapter 6, is the potential reduction in the processing complexity associated with language acquisition. Across languages, meaning is encoded by individual words, word order, grammatical marking, and prosody (Bates & MacWhinney 1982). Within a language, grammatical constructions will be identifiable based on their characteristic configurations of these cues. These grammatical constructions will each have their respective form-to-meaning correspondences – which the learner is expected to acquire. Thus, the mappings can be learned and subsequently accessed, based on the configuration of grammatical cues that serves as an index into the lexicon of stored constructions. A model based on these principles made interesting predictions concerning the neural bases of these operations (Dominey et al. 2003), and has also been effective in miniature language acquisition contexts, in which grammatical constructions are learned and productively generalized to new sentences (Dominey 2000; 2003). This suggests that when the brunt of the compositional load is put on the conceptual representation, a reliable scaffolding is thus in place, upon which syntactic compositionality may naturally repose.

Generative grammar with a human face?

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Abstract: The theoretical debate in linguistics during the past half-century bears an uncanny parallel to the politics of the (now defunct) Communist Bloc. The parallels are not so much in the revolutionary nature of Chomsky’s ideas as in the Bolshevik manner of his takeover of linguistics (Koerner 1994) and in the Trotskyist (“permanent revolution”) flavor of the subsequent development of the doctrine of Transformational Generative Grammar (TGG) (Townsend & Bever 2001, pp. 37–40). By those standards, Jackendoff is quite a party faithful (a Khrushchev or a Dubcek, rather than a Solzhenitsyn or a Sakharov) who questions some of the components of the dogma, yet stops far short of repudiating it.

In *Foundations of Language*, Jackendoff (2002) offers his version of TGG, in which the primacy of syntax (“an important mistake,”

p. 107) is abolished, the related notions of Deep Structure and Logical Form (“the broken promise,” cf. Précis, sect. 3) are set aside, the links to other domains of cognition are discussed, and a hand is extended in peace to psychologists and other cognitive scientists. *Foundations* is an enjoyable, thought-provoking and useful book that fulfills the promise of its title by presenting – and attempting to tackle – foundational issues in linguistics. It is an excellent overview of the ground that must be covered by any serious contender for a linguistic “theory of everything.” Its non-dogmatic style engages skeptical readers of cognitive and empiricist persuasions (“can my theory explain this set of facts better?”) instead of alienating them.

Among the more positive aspects of Jackendoff’s stance in *Foundations* are: the emancipation of semantics as one of the three equal-status components of the “parallel architecture” (p. 125); the realization that not all rules are fully productive (admitting constructions p. 189); and the construal of meaning as a system of conceptual structures (p. 306). The pervasiveness of TGG dogma is, however, very prominent throughout the book. On the most abstract level, the dogma manifests itself in the bizarre mentalistic nomenclature (*f-knowledge*, etc.) that Jackendoff uses instead of the standard explanatory machinery of representation found in all cognitive sciences. Jackendoff shuns a representational account of linguistic knowledge because of his (understandable) wish to avoid joining Fodor and Searle in the philosophical quagmire of intentionality. There exist, however, psychophysically and neurobiologically plausible accounts of symbolic representation that hinge on counterfactual causality and manage to stay clear of the Fodorian mire (Clark 2000; Edelman 1999).

The preponderance of Chomskian bricks in *Foundations* is revealed in Jackendoff’s official insistence, in the introductory chapters, on rule-based combinatoriality. His initial formulation of this concept (pp. 38–57) is so strong as to be incompatible with his own views on constructions (pp. 152–87) and on their graded entrenchment (p. 189), expressed later in the book. It is satisfying to observe that those latter views are on a convergence course with some of the best-known and most promising work in cognitive linguistics (Goldberg 1998; Langacker 1987). As such, they can stand on their own: Computationally explicit construction-based accounts of linguistic productivity need no extra propping (Solan et al. 2003). In any case, Jackendoff should not count on any help from TGG, a Protean theory that, despite decades of effort, has failed to garner empirical support for the psychological reality of the processes and entities postulated by its successive versions, such as movement and traces (Edelman, in press; Edelman & Christiansen 2003). In a recent attempt to obtain psycholinguistic evidence for traces, for example (Nakano et al. 2002), only 24 subjects out of the original 80 performed consistently with the predictions of a trace/movement theory, while 39 subjects exhibited the opposite behavior (the data from the rest of the subjects were discarded because their error rate was too high). Jackendoff’s continuing to cling to TGG (complete with movement and traces), despite its empirical bankruptcy and despite his self-proclaimed openness to reform, is difficult to explain.

Even Jackendoff’s highly commendable effort to treat semantics seriously may be undermined by his continuing commitment to TGG. Conceptualist semantics is an exciting idea, but to develop it fully one must listen to what cognitive psychologists have to say about the nature of concepts. Instead, Jackendoff erects his own theory of concepts around scaffolding left by the generative linguists, which, in turn, is only as sound as those decades-old intuitions of Chomsky and Fodor. In particular, incorporating Marr’s and Biederman’s respective theories of visual structure (pp. 346–47), themselves patterned on TGG-style syntax, into the foundations of semantics cannot be a good idea. Jackendoff’s acknowledgment, in a footnote 10 on p. 347, that Marr is “out of fashion” with the vision community holds a key to a resolution of this issue: Current perceptually grounded theories of vision (Edelman 1999; 2002) and symbol systems (Barsalou 1999) are a safe, additive-free alternative to TGG-style semantics.

In summary, Jackendoff's book is one of several recent manifestations in linguistics of the equivalent of the Prague Spring of 1968, when calls for putting a human face on Soviet-style "socialism" began to be heard (cf. the longing for "linguistics with a human face" expressed by Werth [1999, p. 18]). Jackendoff's stance, according to which the "mistakes" that were made do not invalidate the TGG framework, amounts to a bid to change the system from within. In a totalitarian political system, this may only work if the prime mover behind the change is at the very top of the power pyramid: Czechoslovakia's Dubcek in 1968 merely brought the Russian tanks to the streets of Prague, whereas Russia's Gorbachev in 1987 succeeded in dismantling the tyranny that had sent in the tanks. In generative linguistics, it may be too late for any further attempts to change the system from within, seeing that previous rounds of management-initiated reforms did little more than lead the field in circles (Edelman & Christiansen 2003). If so, transformational generative grammar, whose foundations Jackendoff ventures to repair, may have to follow the fate of the Communist Bloc to clear the way for real progress in understanding language and the brain.

Complexity underestimated?

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Abstract: Instead of commenting directly on *Foundations of Language: Brain, Meaning, Grammar, Evolution*, I provide some remarks from an interdisciplinary view. Language theory is examined from the perspective of the theory of complex systems. The gestural-vocal dichotomy, network theory, evolutionary mechanisms/algorithms, chaos theory, and constructive approach are briefly mentioned.

1. The perspective. I do not have a background in generative linguistics, and read the book *Foundations of Language* (Jackendoff 2002) from the perspective of how the author managed to embed linguistics into an interdisciplinary framework. I remain slightly disappointed. The author clearly abandoned Chomsky's grand isolation decades ago, but the real integrative approach is missing. For example, the title of the first chapter is "The Complexity of Linguistic Structure," but the author gives only a few references from the community of complex-system researchers. Still, though the book seems to be primarily a text written by a linguist for linguists, I have learned very much from it. My comments here are directed not so much at the book itself as at articulating the potential ingredients for a more interdisciplinary approach.

2. The gestural-vocal dichotomy. Jackendoff assumes that language arose in the vocal-auditory modality, and states (in my view, surprisingly) that "a gesture-visual origin would not materially change my story" (p. 236). Based on the fascinating findings of mirror neurons (for reviews, see Rizzolatti & Arbib 1998), the mirror system hypothesis of language evolution has been suggested (e.g., Arbib 2002a). Mirror neurons in monkeys are active both in order to execute motor actions and to observe similar actions of other monkeys or humans. The neural region involved in these operations is considered to be the homologue of Broca's area, the crucial speech area of humans. Language in humans evolved from a basic mechanism that was originally not related to communication, namely, the "capacity to recognize actions" (Rizzolatti & Arbib 1998). Should we believe now, in light of these newer results, that the gestural-visual systems implemented in the action-perception cycle might have a more important role in language evolution than was earlier thought? While I might see the difficulties in explaining the transfer from gestural to vocal modality, I don't

see why we should not consider these findings as a big step toward a new Neurolinguistics.

3. Network theory: Static and (statistical) characterization; self-organizing algorithms. Real world systems in many cases can be represented by networks, and complex networks can be seen everywhere. The organization of biological, technological, and social structures might be better understood by using network theoretical approaches (Albert & Barabasi 2002; Newmann 2003). "Small-world" graph properties (highly clustered and small average length between nodes) and power-law distributions are the key properties of the networks. Complex networks are neither purely ordered nor purely random.

Motivated by the big success of network theory, several works have shown that certain networks assigned to human language have the characteristic patterns of complex organization. Cancho and Solé (2001) analyzed the British National Corpus, and a network of interacting words has been constructed by taking into account only short-distance correlations. The authors don't deny that their algorithm is based on the analysis of the surface structures of sentences. Another network of words was constructed from a thesaurus by Motter et al. (2002). Roughly speaking, words are connected if they express "similar" concepts. In any case, both networks showed statistical properties very similar to those of other complex networks.

Dorogovtsev and Mendes (2001) gave a self-organizing algorithm for the development of word networks based on elementary interactions between words. This algorithm might be the basis of a mechanism to produce a kernel lexicon of the language.

4. Evolutionary mechanisms/algorithms. Jackendoff certainly gives some credit to recent work "on mathematical and computational modeling of communities of communicating organisms" (p. 81). At least from the perspective of integrative approaches, it is interesting to see how model frameworks of population dynamics and evolutionary game theory can be extended to describe language evolution (e.g., Nowak & Krakauer 1999), and specifically grammar acquisition (Komarova et al. 2001), which offers a model framework for describing signal-object association, word formation, and the emergence of syntax with coherent concepts.

5. Chaos theory. Chaos theory might have some role in linguistics. It certainly contributed to the explanation of the occurrence of the celebrated Zipf's law (Nicolis & Tsuda 1989). (I understand that statistical-empirical laws might have nothing to do with architectures, so Zipf's law should not necessarily be mentioned in the book.) The population-dynamical/game-theoretical models elaborated for the acquisition and evolution of language might lead to chaotic behavior under certain conditions. Mitchener and Nowak (2003) recently argued that small learning errors may lead to unpredictable language changes.

6. Constructive approach. While there are different strategies to simulate language evolution, the constructive approach seems to be particularly interesting (e.g., Hashimoto 2001). Language, as a complex dynamical system, can be studied at different hierarchical levels. The origin of the first linguistic systems, the evolution of various languages and language structures, the normal development and acquisition of language in children and adults, and the sense-making process of giving meanings to words during communication take place in different levels of language organization. The constructive approach takes into account both the subjective language-users and the communication among them. The prerequisites of simulating language evolution are language-users, that is, communicative individuals with an established communication system.

Recent efforts to understand emergent biological and social structures adopt the constructive approach. Accordingly, structures and processes emerge as a result of the interaction between the components of complex systems. Specifically, one can understand the emergence of linguistic structures and behaviors. These components consist of interacting autonomous agents, their neural, sensorimotor, cognitive, and communication abilities, and their physical and social environment.

Jackendoff might be right: “Linguistics alone cannot sustain the weight of the inquiry. We need all the help we can get from every possible quarter” (p. 429).

7. Afterthought. Jackendoff’s *Foundations* is a result of an incredible intellectual effort. I am very curious to see how the author reacts to remarks coming from an external world.

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Imaginary mistakes versus real problems in generative grammar

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Abstract: Jackendoff claims that current theories of generative grammar commit a “scientific mistake” by assuming that syntax is the sole source of linguistic organization (“syntactocentrism”). The claim is false, and furthermore, Jackendoff’s solution to the alleged problem, the parallel architecture, creates a real problem that exists in no other theory of generative grammar.

Jackendoff’s *Foundations of Language* (Jackendoff 2002) begins with a polemic about a perceived “scientific mistake” in standard generative grammar, which is corrected in his new proposal for the architecture of grammatical theory. The mistake, dubbed “syntactocentrism,” concerns theories in which the only formation rules (i.e., mechanisms that create linguistic representations) are those of the syntactic component. “In short, syntax is the source of all linguistic organization.” In contrast, Jackendoff proposes a model in which there are three independent sets of formation rules (for phonology, syntax, and semantics), a model he calls the parallel architecture. The three independent representations thereby generated must then be related by interface (or correspondence) rules, including rules that relate phonological representations directly to semantic representations.

Before discussing the parallel architecture proposed as a solution to the purportedly flawed standard theory, it is useful to consider exactly how current theories of generative grammar are syntactocentric, given Jackendoff’s characterization. Let us consider the case of the minimalist program (cf., Chomsky 1995; 2000; 2001), which is inaccurately represented in Figure 1.¹

Within a minimalist derivation (e.g., Chomsky 1995, Ch. 4), the first step is the selection from the lexicon of a lexical array, a set of lexical items designated *the numeration*. This lexical array is then used to build linguistic structures via the iterated application of the concatenation operation Merge. Merge builds syntactic structures bottom-up by concatenating two syntactic objects (lexical items from the numeration, or phrases constructed from previous applications of Merge) and labeling the concatenation with the syntactic category label of one of the two concatenated objects, thus creating a new syntactic object.² The syntactic object generated eventually produces a Phonetic Form (PF) that is interpreted at the sensory-motor interface and a Logical Form (LF) that is interpreted at the conceptual-intensional interface.³ Within the derivation of a linguistic expression, there is a point called “Spell-Out” (S/O) where the phonetic features of the expression are sent to the phonological component for further processing, and the rest of the structure moves on to the LF interface. Any changes to the structure of the expression after S/O are covert, because their effects cannot be seen in PF.

Even if Merge is the only formation rule available in the derivation, it does not follow that syntax is the sole source of linguistic organization. The charge of “syntactocentrism” ignores the contribution of the lexicon. Given that the lexicon specifies the

phonological, morphological, syntactic, and semantic structure of lexical items, it too constitutes a major source of “linguistic organization.” If lexical items enter the syntactic derivation with a specification of their syllable structure, then there is no need to independently generate a syllable structure for the whole linguistic expression generated.⁴ The charge of syntactocentrism is simply false for this theory, and as far as I can tell, for any previous theory of generative grammar that has ever been proposed. The notion is little more than a phantom.

Given that lexical entries contain phonological and semantic information, as well as syntactic information – the standard model since Chomsky 1965 – Jackendoff’s parallel architecture creates a serious dilemma. Presumably, the parallel architecture lexicon that feeds the syntactic component contains no phonological or semantic information. Otherwise, the parallel derivations of phonological and semantic representations would redundantly specify information that is already part of the syntactic derivation, thereby undermining the need for parallel derivations in the first place. Ironically, the syntactic derivation under the parallel architecture must be “syntactocentric” – in just the same way that the phonological derivation is “phonocentric” and the semantic derivation is “semantocentric.”

The parallel architecture puts an enormous burden on the interface/correspondence rules, one that they must surely fail to carry in even the simplest cases. If, as Jackendoff seems to be claiming, phonological representations contain no syntactic information, then there must be a correspondence rule that links the phonological representation of *persuade* to the lexical category V, rather than some other lexical category. However, the phonetic labels of words in a language are fundamentally arbitrary – what Chomsky (1993) calls “Sausseurian arbitrariness” – so there is no systematic way (i.e., via rules) to correlate phonetic labels and lexical categories. The same point applies to the connections between phonological and semantic representations. Given the parallel architecture, nothing in the phonological representation of *persuade* tells us that it corresponds to the semantic representation of *try*. The standard solution to the problem of Sausseurian arbitrariness is to list the correspondences in the lexicon, traditionally the repository for idiosyncratic properties of a language. But once we do this, the motivation for the parallel architecture evaporates.

NOTES

1. It is important to note that the minimalist program is a program for research investigating very general questions concerning the optimality (in some interesting sense) of the computational system for human language and more generally the possible “perfection” of language design. (See Chomsky 1995; Freidin 1997 for discussion.) These questions by themselves do not provide a theoretical framework or a particular model, let alone a specific theory. At present, the minimalist program is being investigated in a variety of ways, where specific proposals are often mutually exclusive, as is normally the case in linguistics, and rational inquiry more generally.

2. Thus phrase structure is constructed via transformation and therefore there is no phrase structure rule component. Movement transformations in this theory also involve a form of merger, where the syntactic object moved is concatenated with the root of the phrase containing it. When two independent objects are merged, this is called external Merge; whereas when a syntactic object is displaced to an edge of the constituent containing it, this is called internal Merge. The two types of Merge correspond to the distinction between generalized versus singularly [*sic*, technical term] transformations in Chomsky (1957 and earlier).

3. There is no further conversion of LF to “semantic representation” as indicated in Figure 1. Furthermore, following up on Note 1, recent proposals have questioned the existence of any level of representation like LF (see Chomsky 2002).

4. The same argument can be made regarding semantic representation. Assuming that the structures Jackendoff proposes for the semantic representation of verbs are on the right track, these structures could just as easily be part of the semantic specification of the lexical entry for predicates where the elements labeled “Object” in Jackendoff’s lexical representations are variables to be replaced with constant terms from the actual sentence in which the predicate occurs. Again, there is no need to generate

these semantic representations independently of the syntax and then have the problem of relating the two independent representations.

Linguistics fit for dialogue

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Abstract: *Foundations of Language* (Jackendoff 2002) sets out to reconcile generative accounts of language structure with psychological accounts of language processing. We argue that Jackendoff's "parallel architecture" is a particularly appropriate linguistic framework for the interactive alignment account of dialogue processing. It offers a helpful definition of linguistic levels of representation, it gives an interesting account of routine expressions, and it supports radical incrementality in processing.

It is easy to argue that dialogue is the basic setting for language use (Clark 1996). Yet historically, generative linguistics has developed theories of isolated, decontextualized sentences that are used in texts or speeches, in other words, in monologue. In turn, this failure to address dialogue at a linguistic level is one of the main reasons why psycholinguistics have also ignored dialogue. In contrast, Pickering and Garrod (in press) propose a specific mechanistic account of language processing in dialogue, called the *interactive alignment* model. This account assumes that in dialogue, interlocutors align their linguistic representations at many levels through a largely automatic process. It also assumes that alignment at one level can promote alignment at other levels. This explains why coming to a mutual understanding in dialogue is generally much easier than interpreting or producing utterances in monologue. In this commentary we consider how Jackendoff's framework in *Foundations* relates to this account.

Jackendoff considers how linguistic theory can elucidate language processing (Ch. 7), a surprisingly fresh approach from a generative linguist. However, he does not explicitly consider how his "parallel architecture" might relate to language processing in dialogue. Here, we argue that the architecture turns out to be particularly helpful in understanding how interactive alignment comes about. First, it is consistent with multiple independent levels of representation with links between the levels. Second, it offers interesting insights into the linguistic representation of semi-fixed or routine expressions such as idioms, which we argue play an important role in dialogue processing. Finally, it is consistent with incrementality in both production and comprehension, which appears necessary for understanding dialogue.

Independent levels and the interfaces between them. Jackendoff assumes that phonological, syntactic, and semantic formation rules generate phonological, syntactic, and semantic structures respectively, and these are brought into correspondence by interface rules, which encode the relationship between different systems (Ch. 5). This produces an architecture which is "logically non-directional" and hence not inherently biased toward either perception or production (Ch. 7, p. 198). These two general features of Jackendoff's account make it especially attractive as a linguistic framework for interactive alignment. First, interlocutors can align representations at different linguistic levels (e.g., Branigan et al. 2000; Garrod & Anderson 1987). These researchers argue that the alignment process is largely automatic (operating through so-called alignment channels) and that alignment at one level (e.g., the syntactic) reinforces alignment at other levels (e.g., the semantic) (e.g., Cleland & Pickering 2003). Hence, alignment channels can affect the application of the formation rules, and interface rules are encoded in the links between the levels. It would be difficult to find such a correspondence with traditional generative approaches where only syntax is generative and where

phonology and semantics are "read off" syntactic structures (e.g., Chomsky 1981). Second, the non-directional character of Jackendoff's architecture explains how perception of structure at one level can enhance subsequent production of structure at that level as the literature on alignment in dialogue demonstrates. In other words, so long as the linguistic structures called upon in comprehension and production are the same, there can be priming from comprehension to production and therefore alignment between interlocutors.

The structure of routine expressions. Pickering and Garrod (in press) argue that the interactive alignment process naturally leads to the development of routine expressions in dialogue. In other words, dialogue utterances become like stock phrases or idioms with semi-fixed structure and interpretation. This is reflected in the degree of lexical and structural repetition in dialogue corpora (Aijmer 1996; Tannen 1989). We argue that routinization greatly simplifies language processing because it allows interlocutors to call upon stored representations, which already encode many of the decisions normally required in production or comprehension, rather than having to compute everything from scratch.

Jackendoff provides an interesting discussion of the contrast between lexical storage and on-line construction (Ch. 6). In section 6.5 he specifically addresses the structure of idioms, and in section 6.6, what he calls *constructional idioms*. Constructional idioms are weakly generative constructions such as *take NP to task* or *put NP in (his, her, or their) place*. These behave like complex VPs but include a free variable position inside the complex structure. Of course, all such idioms are assumed to be represented in long-term memory, either as complete packages (i.e., for standard idioms) or as frames with variables (i.e., for constructional idioms). In our framework we assume that routines of all these kinds are constructed through alignment processes. They can therefore be "set up" for a particular conversation, with a particular meaning that holds for that interchange alone. In other words, routines can be transient.

Radical incrementality in processing. A crucial feature of Jackendoff's account for dialogue is that it supports radically incremental processing. Of course, there are good reasons for assuming incrementality in monologue comprehension, as well. Here, we merely point out that the fact that interlocutors can complete each other's utterances or clarify what they have just heard strongly suggests that it must be possible to comprehend fragments of language as they are encountered, and the fact that such contributions are constrained by the syntax of the original fragment indicates that incremental syntactic analysis must occur (see Pickering & Garrod, in press).

Where is the lexicon?

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Abstract: In an attempt to provide a unified model of language-related mental processes, Jackendoff puts forward significant modifications to the generative architecture of the language faculty. While sympathetic to the overall objective of the book, my review points out that one aspect of the proposal – the status of the lexicon – lacks sufficient empirical support.

In *Foundations of Language*, Jackendoff (2002) proposes a substantial "reconceptualization" of the generative architecture of language in order to better integrate linguistics into the study of the mind and the brain. This move is attractive because it allows the author to embrace a wide range of findings within the broader framework of cognitive neurosciences. Thus previously unrelated phenomena, such as grammaticalization in Creole languages, tip of the tongue states, or referential dependencies within sentences are discussed in a unified mental model. While I am in perfect

agreement with the general objective and some of the main conclusions of the book, I call into question Jackendoff's account of the lexicon. My criticism will focus on three points, comparing the traditional account with the author's from a linguistic, a neuronal, and an architectural point of view. The comments are not intended as a rejection or a rebuttal of Jackendoff's proposal as a whole. Rather, the aim is to highlight certain points in the text where the author's attempt at overall integration results in loss of explanatory power within specific domains.

As Jackendoff himself acknowledges (p. 425), the most dramatic modification he brings to the linguistic architecture established in generative grammar concerns the function and content of the lexicon (Chs. 5 and 6). The usual distinction (e.g., Chomsky 1995), between rules as syntactic functions and lexical entries as tokens on which these functions operate, is blurred. In the new model, both types of entities are contained in the lexicon as triplets of phonological, syntactic, and semantic representations. The only difference between the lexical items and the l(lexical)-rules is whether they contain typed variables: The former don't, the latter do.

The first problem with such an analysis is that Jackendoff does not provide enough linguistic evidence to ground it. His main argument is that even apparently fixed and memorized expressions, such as idioms and other semi-productive items, follow syntactic regularities, for example, the past tense of *take* in *take something for granted* is *took*, just as in any other case. Moreover, some of the semi-productive items even contain syntactic variables, as the noun phrase (NP) slot (*something*) in the previous example illustrates. Following the same logic, syntactic rules, that is, fully productive items, can be conceived of as structures with variable slots only, and can be included in the lexicon, just as set phrases or fixed idioms are included. While this lexicalist and representationalist approach might prove to be a good analysis of idioms, it certainly does not do justice to a wide range of phenomena syntactic research has been concerned within the last fifty years. As it stands, the model cannot handle derivational rules, for example, movement and constraints thereof, or purely structural-relational notions, such as c-command or government. How is one to account for the difference in grammaticality between the possible sentence *Who do you think will win?* and the complex NP violation **Who do you know [the fact that will win]* in Jackendoff's model? The description of the syntactic component and the two syntactic interfaces is not explicit enough to provide an answer. Since issues of this sort make up the bulk of syntactic research, this lacuna is not negligible. (Of course, this is not to say that representationalist or lexical accounts of syntax are in principle not possible. The question is, How much empirical material they are able to cover?)

Secondly, Jackendoff's proposal to conflate semi-productive and productive processes goes against neuropsychological findings. Pinker (1991; 1997) and Clahsen (1999) present convincing evidence that the mental dictionary and the mental grammar may be kept in different parts of the brain (Pinker 1997, p. 547). A double dissociation is found between lexically stored and rule-generated past tense forms in patients with specific brain lesions, and neuro-imaging studies also reveal a differential recruitment of brain areas. Although Jackendoff mentions some of these data, he has little to say about how to reconcile them with his fully lexicalized model.

Thirdly, the lexicalization of syntactic patterns is not without unwelcome consequences for the whole of the linguistic architecture. In the proposed model, lexical items and the l-rules have a double function; they act as interface conditions, but also provide material for the three generative components. In other words, the lexicon is constantly called upon during the derivation by the independent generative components, as well as the interfaces. Consequently, there is no clear-cut distinction between the subsystems of the grammar; the lexicon seems to have devoured the tripartite architecture. In his attempt to do away with syntactocentrism, Jackendoff seems to introduce heavy lexicocentrism in the design.

This architectural problem is especially acute when the model is extended to explain performance, that is, processing. "Now, when the lexicon is called, should we think of the processor calling the lexicon for a match? Or should we think of the lexicon, as part of the interface processor, as actively attempting to impose itself on the input? This perhaps awaits a better understanding of brain dynamics" (p. 207). Note that the two options make distinct empirical predictions about the relationship between the lexicon and the grammar or the mental processes underlying lexical access and retrieval. For example, one would expect lexical access to be a slower, two-step process in the first scenario (call to the lexicon plus word retrieval), whereas according to the second, access is immediate. Unfortunately, these predictions are not explored in detail, therefore the proposal is not comparable with existing accounts, which formulate empirically testable predictions (e.g., Levelt 1993).

In the foregoing discussion, I have been arguing that Jackendoff's reformulation of the status of the lexicon in the generative design of language lacks empirical support from linguistic, neuropsychological, and architectural viewpoint. As a consequence, more research is needed before Jackendoff's framework can be evaluated against rival theories of the language faculty. Although unification is a welcome development in the history of sciences, and the cognitive domain should be no exception, as Jackendoff convincingly argues, we have to make sure that we are not paying too high a price for it.

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Language shares neural prerequisites with non-verbal capacities

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Abstract: Based on neuropsychological evidence of nonverbal impairment accompanying aphasia, I propose that the neural prerequisites for language acquisition are shared with a range of nonverbal capacities. Their commonality concerns the ability to recognize a limited number of finite elements in manifold perceptual entities and to combine them for constructing manifold entities.

Although the brain figures prominently in the title of the book *Foundations of Language: Brain, Meaning, Grammar, Evolution* (Jackendoff 2002), little attention is devoted to the available empirical evidence on the neural substrate of linguistic competence. One of the most robust facts in neuropsychology is the cerebral asymmetry of the neural substrate of language. In the great majority of people, left brain damage (LBD) causes aphasia, but neither does aphasia affect all aspects of language nor is it the only sequel of LBD.

Aphasia affects syntax, phonology, and semantics, which can all be conceptualized as being based on combinatorial systems of finite elements. Other components of verbal communication cannot easily be reduced to combinations of finite elements because they demand fine-grained distinctions within distinct elements or categories. Such components, like emotional prosody or the pragmatics of communicative exchange, are relatively spared in aphasia but vulnerable to diffuse or right-sided brain lesions, which do not cause aphasia (McDonald 1993; Starkstein et al. 1994).

At the same time, most aphasic patients have difficulties with nonverbal tasks that require the extraction of a limited number of finite elements from a rich perceptual diversity. Such tasks are, for example, color sorting where colors have to be sorted according to

categories rather than to perceptual similarity (e.g., light green may be perceptually closer to yellow than to dark green but has to be sorted with the greens); or matching objects by type rather than perceptual appearance, as for example when an analogue clock has to be matched with a digital clock rather than a (visually more similar) compass; or matching images of objects with their characteristic sound (Vignolo 1990). By contrast, matching tasks that require consideration of variations within a category such as, for example, matching of individual faces, do not crucially depend on left hemisphere integrity (Benton & Van Allen 1968).

There are symptoms of LBD, which on first sight, do not fit into a left-hemisphere dominance for extraction and combination of finite elements. These are “high level” disorders of motor control traditionally termed “apraxia.” These symptoms have led to the proposal that left-hemisphere dominance concerns primarily motor control. Attempts to deduce language dominance from motor dominance have either emphasized the motor demands of speaking (Kimura 1983) or postulated that language evolved from gestural communication (Corballis 2002). Recent research suggests that apraxia has more to do with the application of combinatorial systems of finite elements than with motor control. Apraxia affects three domains of actions: imitation of gestures, performance of meaningful gestures on command, and use of tools and objects. Evidence has been provided that LBD patients fail imitation of novel gestures because they cannot reduce them to combinations of a limited number of defined body parts (Goldenberg 1996; Goldenberg & Strauss 2002). They have similar problems when this body part coding is required to match photographed gestures (Goldenberg 1999) or to replicate gestures on a mannequin (Goldenberg 1995), although motor control is trivial for pointing to photographs and very different from imitation for manipulating a mannequin. By contrast, the exclusive role of LBD is mitigated or vanishes completely when imitation puts fewer demands on body-part coding and requires instead fine-grained distinctions within one category of body parts (e.g., the fingers of one hand). Performance of meaningful gestures to command is frequently tested by asking for a pantomime of object use (e.g., “Show me how you would use a toothbrush”). Here the crucial difficulty of LBD patients seems to concern the demonstration of the object and its use by selecting distinctive features of the motor action associated with that use (Goldenberg et al. 2003). Use of tools and objects poses demands on many cognitive functions and can be impaired by brain lesions in many locations (Schwartz et al. 1999), but one component which is exclusively bound to left hemisphere integrity is the inference of possible functions from structural properties of objects. For example, LBD patients may fail to discover that a hook can be fixed to a ring by inserting it (Goldenberg & Hagmann 1998). Such failures can be attributed to an inability to detect a limited number of functionally relevant features and to solve mechanical problems by reducing them to basic functional relationships.

There is controversy concerning whether the co-occurrence of these difficulties with aphasia in LBD patients is a result of similarities between the affected functions or of anatomical contiguity between their neural substrates, but this opposition may be ill-conceived. Anatomical contiguity is unlikely to have arisen from arbitrary placement of unrelated functions. Presumably it reflects a deeper affinity of their neural substrate. It may be more fruitful to ask for the functional properties corresponding to this neural commonality. I propose that this commonality is to be sought in the ability to recognize a limited number of finite elements in manifold perceptual entities, and to combine them for reconstructing manifold entities. In this account, the neurally designed predisposition for language acquisition is not specific for language but also supports a range of nonverbal capacities.

Jackendoff’s conceptualism

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Abstract: In this commentary, I concentrate upon Ray Jackendoff’s view of the proper foundations for semantics within the context of generative grammar. Jackendoff (2002) favors a form of internalism that he calls “conceptualism.” I argue that a retreat from realism to conceptualism is not only unwarranted, but even self-defeating, in that the issues that prompt his view will inevitably reappear if the latter is adopted.

In *Foundations of Language: Brain, Meaning, Grammar, Evolution* (henceforth *Foundations*), Jackendoff is sympathetic – more sympathetic than I, for one, would have expected him to be – to the view that the theory of meaning in empirical linguistics should link language to human action and communication, and that the notions of reference and truth are indispensable both as explaining relations of sentences to one another, as in implication, and their relations to their subject matter and conditions on their use. Jackendoff holds, however, that the proper implementation of this view requires the adoption of a variety of irrealism about what we refer to, and what makes what we say true or false. In Part III of *Foundations* he offers a variety of reasons for this irrealism, or conceptualism, as he calls it. None of these seem to me effective; I will consider a few below. More than this, however: Jackendoff’s irrealism threatens to be self-defeating, in that the problems that he discerns for realist accounts are bound to return, in just the same form, under the interpretation of reference that he offers.

Having remarked, in my view rightly, that the signal contribution of generative grammar was to take for the subject of linguistics not the formal properties of language but rather the basis for human knowledge and capacity for language, Jackendoff is wary (to the point of abhorrence) of saying that languages themselves are abstract objects whose properties we know (or “cognize,” to use Chomsky’s suggestion of a more neutral terminology). He is wary of this, not because he rejects the notion of implicit or tacit knowledge, but rather because he thinks that, once we say that languages are abstract, we have cut ourselves off from the psychological investigation that is to be the core of the enterprise (p. 297). He is also repelled (p. 299) by the idea that these abstract objects have always been lying around, waiting for people to “grasp” them. Abstract objects in general, he thinks, must be “human creations.”

The conflicts here are illusory, however. What comes to hold only through human organization and activity is not the existence of abstract objects, but empirical identities: That language L has property P , may be a fact on a par with the truths of arithmetic; but that Higginbotham’s language or Jackendoff’s language = L , and therefore that Higginbotham’s language or Jackendoff’s language has property P , is a psychological contingency, to which all the available evidence, about them and other humans, is relevant. I suppose we may agree that a primitive mechanism of “grasping” is, if true, a counsel of despair. But how is the slogan that abstract objects are “human creations” supposed to help? Everyone knows on a moment’s reflection that to enclose the largest area with a piece of string, you should form it into a circle. Supposing that circles are human creations brings us no closer to an explanation of why this should be so.

Jackendoff opposes what he calls common-sense realism about reference – according to which (simplifying only a bit) words refer to things – to his own conceptualist account, according to which speakers judge words to refer to things in “the world as conceptualized” by them. The basis for the substitution of the conceptualist view for the standard one is a variety of questions about reference given in Chapter 10, section 3, (pp. 300–303). All our old friends are there: Sherlock Holmes, the unicorn in my dream, the value of my watch, virtual squares, “politically constructed entities” such as Wyoming, and so forth. There is no space here to consider all of these, but I make two remarks.

First, Jackendoff ignores the point that nominal reference must be considered, not in isolation, but in the context of a sentence. Thus, take the value of my watch, or the distance between New York and Boston. The things are identified with “mixed numbers,” \$50 for my watch, 204 miles for the distance. However, as observed originally by Carnap (1926), any mystification about them disappears when we observe that the reference is to the value of my watch in dollars, which is just the number 50, or the distance between New York and Boston in miles, which is 204. The virtual square formed by four properly placed dots, Jackendoff says, “isn’t there physically.” True enough, there are no lines, only the dots that are to be construed as the vertices of the square. There is, however, within the limits of perceptual accuracy, exactly one square of which they are the vertices, so that to say that the square is “formed by the four dots” indicates, not that there is no square, but rather how we are to understand the notion “*x is formed by y.*”

Second, and more critically, Jackendoff urges (p. 304) that “reference” need not be to things in the “*real world*” (his emphasis). So statements about Sherlock Holmes or the unicorn in my dream can be taken on a par with statements about Derek Jeter or the unicorn in the garden. But this, I think, conceals a mistake: The distinction between names like *Sherlock Holmes* on the one hand, and *Derek Jeter* on the other, is a distinction that is made *within* our speech, not outside it. If you think there is a serious question whether Sherlock Holmes ever visited Mongolia, or that what is responsible for the truth of the statement that he lived in London is the same sort of thing that is responsible for the truth of the statement that Derek Jeter lives in New York, then you don’t understand the name; for it is part of understanding the name *Sherlock Holmes* that Sherlock Holmes is a fictional character. The case is similar with dream-objects, but rather more interesting, since one could believe that one saw them, interacted with them, and so forth; but for us anyway there is common recognition that statements about some of the contents of dreams are made true or false in virtue of our representations alone, so that their superficial grammatical form is not a guide to their truth conditions. But the truth conditions are known, and known to be different from those of apparently similar statements. It is, therefore, no advance, and in fact an obscuring of the issues, to adopt for these reasons a conceptualist semantics. Jackendoff’s thought seems to be that, if we are casual enough about objects of reference, on the ground that they are in the merely conceptualized world, the problems of distinguishing the truth conditions of fictions, some statements about dreams, and so forth will go away. But they won’t.

The latter part of *Foundations* involves often very interesting discussion of semantic phenomena, both lexical and combinatoric. None of these, so far as I can see, require making a distinction such as he envisages between the “conceptualized world” and – the world.

Four challenges for cognitive neuroscience and the cortico-hippocampal division of memory

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Abstract: Jackendoff’s criticisms of the current state of theorization in cognitive neuroscience are defused by recent work on the computational complementarity of the hippocampus and neocortex. Such considerations lead to a grounding of Jackendoff’s processing model in the complementary methods of pattern analysis effected by independent component analysis (ICA) and principle component analysis (PCA).

Jackendoff elaborates four challenges for cognitive neuroscience whose consequences reverberate throughout his book, *Foundations of Language* (Jackendoff 2002). In a nutshell, if spreading activation (SA), firing synchrony of neural units (FS), and multi-

layer perceptrons trained by back-propagation of error (BP) constitute the apogee of current neurotheory, then it has a long way to go to reach even the lowest echelons of descriptive adequacy for human language. In this commentary, I briefly review a neurologically realistic alternative to the SA/FS/BP trio that meets most of Jackendoff’s challenges. Known as the Complementary Learning Systems (CLS) model, it was first developed by McClelland et al. (1995), and has been refined several times since then (see O’Reilly & Norman 2002). Its computational principles have been applied to a wide range of learning and memory phenomena (impaired and preserved learning capacities with hippocampal lesions in conditioning, habituation, contextual learning, recognition memory, recall, and retrograde amnesia) across several species (rats, monkeys, and humans). To explain how CLS works, we start at the end, with Jackendoff’s fourth challenge.

Jackendoff sees it as contradictory that a compositional phrase, such as “lift the shovel” should be encoded in short-term memory via SA or FS, while a structurally-equivalent idiomatic phrase such as “kick the bucket” should be stored in long-term memory by the slow modulation of synaptic weights via BP. The CLS literature implicitly raises a comparable objection, which is resolved as the computational difference between hippocampal and neocortical function. By way of illustration, let us call on a linguistic example that neither Jackendoff nor CLS discusses, but which has considerable empirical depth (see Bowerman 1996; Bowerman & Choi 2001; Choi & Bowerman 1991).

Imagine a child viewing two events, one in which a cassette is put in a bag and another in which the same cassette is put in its case. Korean, in contrast, lexicalizes the events with separate items, namely, the verbs *nehta*, “put loosely in or around” for the former and *kkita*, “interlock, fit tightly” for the latter. Thus, the brain must keep both events separate, and presumably with their full complement of real-world detail, in order to account for the specificity of Korean. Nevertheless, both events have overlapping parts, such as the cassette, the motion of bringing two things together, and maybe even the person performing the motion. The brain must therefore ensure that parts shared among events do not interfere with one another. CLS asserts that these characteristics define episodic memory and the function of the hippocampus: the fast and automatic learning of sparse representations. Nevertheless, if all events are kept separate, there will be no way to generalize across them. Yet humans do indeed generalize; witness the fact that English uses the same spatial vocabulary for both events, namely, the preposition *in*, with the aid of the motion verb *put*. The brain must therefore be able to integrate events so as to abstract away from their specifics and encode the overall statistical structure of the environment. CLS asserts that these characteristics define semantic memory and the function of neocortex: the slow and task-driven learning of overlapping representations.

Returning to as “lift the shovel” versus “kick the bucket,” we may conclude that the hippocampus makes the initial, short-term binding of the disparate features of the phrases from which the neocortex extracts any statistical regularities, such as the parallel [V [Det N]] structures. The idiomatic phrase sports an additional regularity, namely, the fact that it has a noncompositional reading which presumably can *only* be learned by the slow (i.e., multiple exposure) modulation of synaptic weights. The conclusion is that CLS avoids any inconsistent treatment of compositional and non-compositional phrases.

Turning to the first challenge, Jackendoff cites the multiple embedding of linguistic entities as leading to temporal incoherence for any solution to the feature-binding problem that relies on FS. The CLS model has made this same criticism from a more neurologically informed perspective. Its alternative is to return to the notion of conjunctive features, with a twist. The twist is to avoid the combinatorial explosion of units encoding a single feature conjunction by distributing the conjunctions across many units (O’Reilly & Busby 2002),

where each unit encodes some possibly-difficult to describe amalgam of input features, such that individual units are active at different levels

for different inputs, and many such units are active for each input (Hinton et al. 1986). Therefore, the input is represented by a complex distributed pattern of activation over units, and each unit can exhibit varying levels of sensitivity to the featural conjunctions present in the input. (O'Reilly & Busby 2002)

The conclusion is that Jackendoff's objection is vindicated, and resolved.

The "problem of 2" presumably disappears with a distributed encoding, since each repeated item would be represented by different levels of activation of the neural population encoding the feature conjunction according to the item's context. However, the current CLS literature does not address this issue directly, leaving the reader uncertain whether the approach will scale up correctly.

Finally, the derivation of typed variables is a goal of CLS, in the guise of the learning of relational structures. Its supporters echo Jackendoff's reiterated protestation that freely-combining typed variables are fundamental to higher cognition. Unfortunately, the CLS simulations in which a relational structure is learned suffer from the general opaqueness of distributed encoding. That is to say, the network appears to have learned the relations that it was exposed to, but from the text of the reports, one does not understand how it is done.

In this respect, the work of two other researchers is highly relevant. Jackendoff cites Shastri and Ajjanagadde (1993) as one of the few computational models that grapples with the representation of typed variables. Shastri's more recent model SMRITI (Shastri 2002) takes these ideas a step further by specifically attributing to the hippocampus the responsibility for creating role-entity bindings, so that it will assign to an event in which John gives Mary a book in the library on Tuesday, the representation *GIVE: giver = John, recipient = Mary, give-object = a book, location = library, temporal-location = Tuesday*. This is tantamount to the relational structure of first-order predicate logic, if not Event Semantics (see Parsons 1990). In a slightly different vein, Pulvermüller (2002) reviews and expands on the concept of syn-fire chains as a neurologically plausible mechanism for serial order in language. Such chains explicitly encode the relational structure of syntax, though Pulvermüller does not localize them to any particular cortical area. In fact, Pulvermüller provides a fascinating neuroscientifically grounded foil to Jackendoff, and reading the two of them together is a rewarding intellectual exercise. More to the point, both Shastri and Pulvermüller wind up invoking freely-combining typed variables in a way that is more transparent than CLS.

So what of the other 418 or so pages of Jackendoff's text? The various modules of the grammar-based processing architecture presumably reflect independent clusters of statistical regularities in the linguistic input, learned by the gradual adjustment of synaptic weights. This suggests a further distillation of the CLS: The hippocampus performs independent component analysis (ICA) on its input patterns in order to orthogonalize them, that is, to remove their common features and so make them maximally unrelated (Kempermann & Wiskott 2004). At the very least, this would separate a linguistic pattern into its phonological, syntactic, and conceptual components, and then into the independent subcomponents thereof, such as Shastri's role-entity bindings in the conceptual module. The drawback of ICA is that it separates linguistic patterns into an enormous number of dimensions, for example, *temporal-location = Monday, temporal-location = Tuesday*, and so on. It is the function of the neocortex to reduce these dimensions to the most relevant or informative ones, which suggests that neocortex performs principal component analysis (PCA, see Simoncelli & Olshausen 2001) on the hippocampal output. For instance, the independent temporal components mentioned two sentences ago could be reduced to the single principal component *temporal-location = day of the week*, thereby synthesizing a typed variable.

Psychologism and conceptual semantics

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Abstract: Psychologism is the attempt to account for the necessary truths of mathematics in terms of contingent psychological facts. It is widely regarded as a fallacy. Jackendoff's view of reference and truth entails psychologism. Therefore, he needs to either provide a defense of the doctrine, or show that the charge doesn't apply.

Jackendoff's vision of the language faculty in *Foundations of Language: Brain, Meaning, Grammar, Evolution* (2002) is impressive in scope and rich in insightful detail. However, his account of abstract objects (sect. 10.9.3) requires substantial elaboration and defense before it can safely avoid the fallacy of psychologism.

In Jackendoff's account, conceptual structures within the generative semantic module are not themselves interpreted – they do not *have* a semantics. They just *are* the semantics of natural language. The fine-grained data-structures that inhabit the semantic module interface richly with perceptual modalities and with motor outputs, while individually not necessarily representing anything in the world as such (cf. Brooks 1991). The familiar appearance that words refer to entities and events can be explained – for concrete referents, at least – in terms of the relationship between semantic constructs and the outputs of perceptual faculties. It is these outputs that we experience as our "world." Now, in the case of abstract objects (like beliefs, mortgages, obligations, and numbers), which manifestly lack perceptual features, the theory makes only slightly different provisions: The data-structures that encode them possess inferential rather than perceptual features. Interfaces to syntax and phonology treat all conceptual structures similarly, regardless of whether their constitutive features are exclusively inferential or, in part, perceptual. Thus, Jackendoff's naturalistic theory of concepts rejects Platonism and identifies abstract objects with the cognitive structures that express them.

The paradigm cases of abstract objects are mathematical and logical entities. Oddly, Jackendoff does not discuss these cases explicitly. Yet if the Conceptual Semantics (CS) account of abstract objects is to work at all, it must work for them. The trouble is that CS entails *psychologism*, the view that the necessary truths of mathematics and logic are to be accounted for in terms of contingent facts about human cognition. According to psychologism, $2 + 2 = 4$ is a fact of human psychology, not a fact that is independent of human beings. Frege (1953) raised seminal objections to this doctrine and today psychologism is typically viewed as a patent fallacy (Dartnall 2000). There is room for discussion, however. Haack (1978) points out that it is far from obvious whether Frege's objections continue to apply. Frege's target was the introspectionist psychology of the day, and Jackendoff (1987; 2002) carefully avoids this approach. But to get off the ground, a psychologistic account of abstract entities must cope with three challenges:

(1) *Universality*. Some norms derive their authority from community standards. Those norms are no less real for their conventional nature (traffic rules come to mind), but they are only true by agreement. By way of contrast, norms governing the behavior of abstract logical and mathematical entities are *universal* (a point stressed by Nagel 1997). Community standards derive their authority from these norms, and not vice versa. Even people with untutored intuitions can come to recognize the truth of a law of logic or mathematics, though they may require quite a bit of reflection to do so. CS needs an explanation of how some abstract objects (which are supposed to be mental entities) come to possess these inferential features. Are they innate? If so, Jackendoff's rejection of Fodor loses some of its bite. Are they learned? If so, the poverty of stimulus problem rears its ugly head.

(2) *Objectivity*. Logic, geometry, and mathematics are not uninterpreted formal systems that people happen to universally assent to regardless of which community they inhabit. Formal interpretations of physical phenomena permit predictions concerning the behavior of objective reality even in contexts vastly beyond the scope of actual (or possible) human experience. How then does mathematical reasoning manage to preserve truth about distant contexts if mathematical objects are merely psychological data structures with local inferential features? In other words, quite apart from its universality, how, in the psychologicistic account, does mathematics come by its objectivity (cf. Smith 1996)?

(3) *Error*. It is tempting to account for the validity of logical inference in terms of the way that (normal, healthy) cognitive systems actually reason. But we can make mistakes regarding the properties of abstract objects. Even professional mathematicians occasionally draw false inferences about mathematical objects. And a real feeling of surprise and discovery can accompany mathematical innovation, that moment when humanity discovers that we have been conceiving of some mathematical construct incorrectly all along. The intuition that mathematical objects can have properties quite different from those imputed to them, even by professionals, fuels Platonist intuitions (Dummett 1978). Validity cannot merely consist in conformity with the way people actually reason; it is a property of arguments that conform to the way we *ought* to reason. How psychologism can account for this remains uncertain.

Jackendoff (pp. 330–32) suggests several mechanisms of social “tuning” that can serve to establish (universal) norms within a community – norms against which error may be judged and the appearance of objectivity can arise. So when Joe mistakes a platypus for a duck (p. 329), his error is relative to the impressions of the rest of his community. “Objective” fact and the appearance of universality is established by community consensus. Unfortunately, this account does quite poorly with logic and mathematics. A mathematical or logical discovery happens when one member of the community realizes that something is wrong with the way the community conceptualizes some aspect of the field, and demonstrates that error to the other members of the community. The issue here is how a whole community can be shown to be in error when the objective reality against which the error is judged is mere community consensus. Platonism has an obvious solution to this issue, but CS will have to work for one.

We are by no means arguing that universality, objectivity, and error cannot be accommodated by CS. But Jackendoff does suggest that CS can provide insight into the appeal of formal approaches to semantics. Before it can explain the success of its rival, it must itself account for the nature of the logical apparatus on which formal work rests. We suspect that this can indeed be done. But until it is, CS remains incomplete in an important way.

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Delegation, subdivision, and modularity: How rich is Conceptual Structure?

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Abstract: *Contra* Jackendoff, we argue that within the parallel architecture framework, the generality of language does not require a rich conceptual structure. To show this, we put forward a delegation model of specialization. We find Jackendoff’s alternative, the subdivision model, insufficiently supported. In particular, the computational consequences of his representational notion of modularity need to be clarified.

In Jackendoff’s framework in *Foundations of Language* (2002), understanding the meaning of a sentence consists in constructing a representation in a specific cognitive structure, namely, Conceptual Structure (CS). CS is not dedicated to language, though. It is the structure that carries out most of our reasoning about the world. According to Jackendoff, this follows from what we call the Generality of Language Argument (GLA):

1. Language allows us to talk about virtually anything.
2. Every distinct meaning should be represented within CS.
3. CS must contain our knowledge about everything it represents.
4. Hence, CS contains large bodies of world knowledge: CS is “rich.”

For instance, if the difference between “to murder” and “to assassinate” is that the second requires a political motive, then CS contains knowledge about what it is to be a political motive (Jackendoff 2002, p. 286).

GLA excludes the idea that there is a specifically linguistic level of semantics, containing only a “dictionary meaning” as opposed to “encyclopedic information” (Jackendoff 2002, p. 285). It also excludes a minimal view of CS. We call *minimal* a CS that is able to represent all distinct meanings, but is not able to carry out computations other than the logical ones. A minimal CS could represent the meanings of “x is an elephant” and “x likes peanuts,” but would not be able to infer the second from the first.

We think that GLA is wrong: The generality of language is compatible with a minimal CS. Indeed, it is a viable possibility within Jackendoff’s general architecture of the mind. Consider the sentence: “The elephant fits in the mailbox.” To know that it is wrong is to represent its meaning and judge it to be false. Jackendoff would say that these two steps are carried out by different structures, namely, CS and Spatial Structure (SpS). Since only CS interacts directly with language, the sentence has to be translated into CS. From there it can in turn be translated into a representation in SpS. This would be done by dedicated interfaces. SpS is the place where the sentence is found false, for it is impossible to create a well-formed spatial representation of an elephant in a mailbox. We regard this as an instance of a delegation model:

(DM) Domain-specific computations are carried out outside CS, but their result is represented in CS, and may thus be expressed in language.

In this case the computation is very simple. It consists of checking whether an adequate SpS representation can be formed. Nevertheless, it is done outside CS. CS only represents its result, namely that the elephant does not fit in the mailbox.

It is a priori possible that DM applies to all the computations involved in our knowledge about physical objects, biological kinds,

other minds and so on. The resulting CS would be minimal. Hence, premise (3) is false: CS *could* represent meanings without containing world knowledge.

Jackendoff does not address this question. Instead, he directly proposes an alternative model for specialization. For instance, he takes social cognition as involving a specialized mental structure. But he claims that this is a substructure of CS, a “sub-specialization” (Jackendoff 1992a, Ch. 4). We call this the *subdivision model*:

(SM) Domain-specific computations are carried out within parts of CS, and can thus be expressed in language.

If most of our reasoning about specific domains has to be carried out within parts of CS, then CS has to be rich. But why should it be so? Jackendoff could put forward two distinct hypotheses.

The *computational unity hypothesis* claims that CS is a computational module, with a *unique processor*, and that sub-specializations are representational modules, that is, knowledge bases about specific domains.¹ On this hypothesis, domain-specific inferences are construed as logical inferences based on domain-specific premises and effected by a single processor, and this is why they are part of CS. However, such a claim is far from being uncontroversial. Many cognitive psychologists argue that putative “sub-specializations” such as Theory of Mind, carry out their computations independently of each other in a relatively autonomous way, and are possibly situated in distinct, dedicated neural structures (Leslie 1994; Segal 1996). Moreover, if the processor were damaged, it seems that one would lose all propositional computational abilities at once. But this pathology has not been observed.

A weaker hypothesis is that of a *unique representational format*. Jackendoff (2002, p. 220) seems to endorse it. It merely claims that all sub-specializations of CS share a common, propositional format and that all corresponding computations are of a quantificational-predicational character. Their computations need not be carried out by a common processor. However, we do not think that this view has any more plausibility than the hypothesis that some sub-specializations have their computations carried out in *sui generis* formats that are designed for the tasks that they solve. Our understanding of each other’s minds plausibly involves propositional representations, but this may be the exception rather than the rule. Moreover, it is not clear whether CS would, in this view, constitute a module in any interesting sense, or whether the hypothesis really differs from generalized delegation and a minimal CS.

To conclude, within Jackendoff’s architecture of the mind, the generality of language is compatible with either a rich or a minimal CS. The choice of the former requires that the computational consequences of Jackendoff’s representational notion of modularity be at the very least clarified.

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NOTE

1. For further discussion of representational (or intentional) and computational modularity, see Segal (1996).

Neuropsychological evidence for the distinction between grammatically relevant and irrelevant components of meaning

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Abstract: Jackendoff (2002) argues that grammatically relevant and irrelevant components of meaning do not occupy distinct levels of the semantic system. However, neuropsychological studies have found that the two components doubly dissociate in brain-damaged subjects, suggesting that they are in fact segregated. Neural regionalization of these multidimensional semantic subsystems might take place during language development.

Jackendoff’s *Foundations of Language* is, without a doubt, a monumental achievement. It both clarifies and begins to fulfill the deeply pressing need for integration not only within linguistics but also between linguistics and the connected disciplines of psychology, neuroscience, and evolutionary biology.

Here I concentrate on the relation between linguistics and neuroscience. Although Jackendoff points out that a great deal has been learned about the functional organization of various aspects of language in the brain, he doesn’t devote much space to exploring how these findings can shed light on current issues in linguistic theory. To illustrate the potential applications of recent neuro-linguistic research, I present an example that bears directly on two theoretical topics that are near to Jackendoff’s heart: the syntax-semantics interface, and the basic architecture of the semantic system.

As Jackendoff observes, many linguists have been converging on the notion that grammatical constructions consist of morphosyntactic patterns that are directly associated with schematic meanings; and, in order for a word to occur in a given construction, its own meaning must be compatible with that of the construction (Goldberg 2003). Consider the well-known locative alternation:

- (1) a. *Sam sprayed water on the flowers.*
 b. *Sam dripped water on the flowers.*
 c. **Sam drenched water on the flowers.*
- (2) a. *Sam sprayed the flowers with water.*
 b. **Sam dripped the flowers with water.*
 c. *Sam drenched the flowers with water.*

The construction in (1) has the broad-range meaning “X causes Y to go to Z in some manner,” whereas the one in (2) has the broad-range meaning “X causes Z to change state in some way by adding Y”; each construction also has a network of more restricted narrow-range meanings that are essentially generalizations over verb classes (Pinker 1989). *Spray* can occur in both constructions because it encodes not only a particular manner of motion (a substance moves in a mist) but also a particular change of state (a surface becomes covered with a substance). However, *drip* and *drench* are in complementary distribution, for the following reasons. One of the narrow-range meanings of the first construction is “X enables a mass Y to go to Z via the force of gravity,” and this licenses expressions like *drip/dribble/pour/spill water on the flowers* and excludes expressions like **drench water on the flowers*. Similarly, one of the narrow-range meanings of the second construction is “X causes a solid or layer-like medium Z to have a mass Y distributed throughout it,” and this licenses expressions like *drench/douse/soak/saturate the flowers with water* and excludes expressions like **drip the flowers with water*.

According to the Grammatically Relevant Subsystem Hypothesis (GRSH), a fundamental division exists between, on the one hand, semantic features that determine the compatibility between verb meanings and constructional meanings, and on the other, semantic features that capture idiosyncratic nuances of verb meanings, for example, the featural distinctions between *drip*, *dribble*,

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; Pulvermuller 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

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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

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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

1966, mentioned (p. 128) as an early model outside the tradition of generative grammar, sharing Jackendoff's current view of parallel architecture for different structural components. But apart from this fundamental similarity, several other important points of similarity emerge in this work:

1. Retreat from the insistence on derivational/transformational rules in recognition of their lack of realism.
2. Serious attention to the need for a model of linguistic competence to be made usable as the basis for a model of performance.
3. Adoption of a more constrained view of Universal Grammar falling far short of the innateness of abstract grammatical categories, as envisioned in the Principles and Parameters model. Besides being constrained, Jackendoff's view is much more articulated, and is presented with a scenario explaining how it might have developed incrementally in human evolution, rather than appearing suddenly and mysteriously as a whole.
4. Conclusion that the distinction between "rules" of language and lexical items does not have to be seen as so fundamental, as in more orthodox Chomskyan models.
5. A view of the lexical item more like Lamb's version of the lexeme, including the consideration of a possible separate internal syntax for the word (morphotactics).
6. A view of semantics that includes many aspects treated as syntactic in more orthodox generativism and sees the possibility of going beyond single sentences to take discourse relations into account.
7. Serious attention to the relation of language to other matters of neural functioning like visual perception.

There is no indication that Jackendoff is aware of Lamb's more recent work (as summarized in Lamb 1999). Lamb's interest in relating his model of language to the brain began in classroom presentations and public lectures in the late 1960s. It took about 30 years, however, before Lamb was sufficiently satisfied to publish the results, though his product is a textbook introduction to what he now terms "neuro-cognitive linguistics," rather than a research monograph. The neuro-cognitive model is a development of the stratificational which "uses mainly linguistic evidence but attempts also to integrate the findings from psycholinguistics and neurolinguistics" (1999, pp. 7–8).

Lamb's relational networks are more sophisticated and less limited than those of the connectionists cited by Jackendoff. In discussing recent attacks against this form of connectionism, Lamb states: "We shall see that some of them are based on misunderstanding of connectionism – or at least, of what connectionism ought to be – while some of them, along with additional evidence, oblige us to refine the theory" (1999, p. 4).

Though excluding Lamb's more recent work, Jackendoff nevertheless attempts to synthesize a remarkable breadth of research areas from different disciplines. More attention to Lamb's work, however, would not only open up possibilities of an enriched dialogue among scholars, it would provide a potential for bringing in an implementation of the model in a way that has a chance to ultimately relate to the neural connections involved in the brain. The parallel architecture model has justifiably retreated from the old model of derivational rules, which constituted more a mathematical abstraction than a realistic way to look at language as a system acquired and used by humans. Lamb's more recent model, however, provides a way to relate language modeling more positively to neural facts, and it would be well worth examining how relational networks of the Lambian sort could be used to implement this model.

The most fundamental difference between Jackendoff's stratificational model and Lamb's concerns nativism. Lamb has always been skeptical of claims of innate universal grammar, while Jackendoff sees nativism as the most essential feature of the older Chomskysm to be retained. Still, he presents a modified and articulated variety of the latter, and Lamb has always been more concerned with questions of language structure than with language acquisition, meaning that his model is not totally incom-

patible with nativism in general. Dialogue based on this model is much more feasible than with relation to older, more monolithic forms of nativism.

Cartesian and empirical linguistics: The growing gulf

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Abstract: Jackendoff's *Foundations of Language: Brain, Meaning, Grammar, Evolution* (2002) achieves a major shift in the focus and methods of Generative Linguistics (GL). Yet some of the original restrictive features of GL, cognitivism and Cartesianism in particular, remain intact in the new work and take on a more extreme form with the addition of a phenomenalist ontology.

Jackendoff's *Foundations* presents a striking new view of language as a component in a general theory of mind. By taking a more piecemeal interpretation of the traditional formalisms of Generative Linguistics (GL) and supplementing them with a new semantics, it opens up GL to a wide range of research areas in cognitive science. Since *Foundations* is also a true product of the GL tradition, certain of its chapters (notably Chs. 3, 5, 6, 11, and 12) also make an excellent introduction to state-of-the-art GL for language researchers in other disciplines.

But some traditional principles of GL, strongly at variance with the objectives of interdisciplinarity, have passed unchanged into *Foundations*. The first is "cognitivism" (Keijzer & Bem 1996; MacAogáin 1999), the practice of referring to all linguistic competences as "knowledge" or "cognition," including those that are unconscious. The difficulty with cognitivism is that it leaves us with only one form of activation, regardless of how levels and interfaces were ascribed to the structure so activated. All we ever have is the whole lot "running off" as a unit in f-mind. But in order to model the most elementary of behaviour systems, we already need several forms of activation that are irreducibly different; two at the very least to correspond to perceptions and wants. In addition, we need a separation between forms of activation that are belief-forming or want-forming from those that merely determine content. In spite of the mentalistic idiom, these distinctions are well established in neurology, down to the invertebrates, and are separable also in psychological models of cognition, inference, learning, and decision-making, which embody notions of reinforcement and adaptation.

While cognitivism can be defended in a lot of cognitive science, wherever truth, value, and reinforcement are well-defined in the task environment, in GL, where grammaticality is all we have, its effect is to split language off irretrievably from behaviour and the environment, as is acknowledged by the f-prefixes of *Foundations*. The cognitive linguist can claim to be already working on the physicalist account, in conjunction with the brain sciences. But adding neurological glosses to the cognitivist account leaves it just as isolated as it was before from the quantitative study of language as a form of response to the environment.

The isolation is compounded in *Foundations* by the addition of a phenomenalist or "conceptualist" ontology, most explicitly in the attack on the notion of external object (Ch. 10). Phenomenalism retreats from the external world to the world as perceived by individuals, or in the language of *Foundations*, it pushes the world back into the mind (p. 303). This is necessary, according to Jackendoff, to open up the border between GL and psychology and thus to "integrate semantics with the other human sciences" (p. 329).

The suggestion is that psychology, and perhaps other human sciences, are phenomenalist in nature. "Psychological" (as opposed to "philosophical" or "truth conditional") is Jackendoff's

preferred term for “phenomenalist” in other contexts (e.g., Jackendoff 1992a, p. 158). An argument offered in *Foundations* (p. 304) is that the study of mental events independently of possible referents in the real world – undoubtedly a common practice in psychology and linguistics – is already the beginnings of phenomenism.

But while some areas of psychology may be able to get by with a phenomenalist ontology, those of most interest to linguists, namely perception, cognition, and language, are not among them. Here the psychologist very often doesn't even care about the exact nature of the internal events, as long as the responses come out right. The important thing about language is that its phrases and sentences are keyed to the same events in the environment for all of its users, regardless of what goes on in their heads.

The phenomenism of *Foundations* will ensure that it will preserve the traditional Cartesian aloofness of GL from behaviourist and corpus-based approaches to language, in spite of the new interdisciplinary forum it has created. Neither does *Foundations* offer any points of contact with applied linguistics. To take a single example, the search for a sensible lexical and phrasal semantics, so central to *Foundations*, is going on with equal urgency in modern lexicography, a new corpus-based discipline with strong links to the empirical study of second-language learning (e.g., Humblé 2001). Yet it seems that neither can help the other.

The difference between Cartesian and empirical linguistics can be illustrated by asking how “valid” *Foundations* is, in the old psychological sense of really being about what it claims to be about. How representative are the numbered phrases and sentences of English in *Foundations*, of the English language as a whole? With the ready availability of corpora of high validity for the major languages, it is significant that linguists in general are still not required to estimate for us the percentage of the language that their structures will cover, and the size of the remaining piece that will require a different treatment. The relativity of structures to individual languages poses the same problem. It is fortunate that English had the international status to allow GL to direct so much of its efforts on a single language. But even if English were the only language in the world we would still have the validity problem. As Jackendoff puts it, “there are just so many goddamned words” (2002, p. 377). We need some assurance that our semantic structures have invariance over a good part of the language we are studying, and are not trapped in lexical pockets. And ideally we would like to know which structures have some chance of being invariant over languages, or at least a few languages from different groups.

Perhaps the explanation for the gulf between Cartesian and empirical approaches is that the terms “pure” and “applied” have a special meaning in linguistics that does not imply that the theory and its referent remain the same across domains. “Pure” in linguistics could also mean that we are dealing with a different kind of reality. *Foundations* makes the case more strongly than ever before.

How Jackendoff helps us think

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Abstract: The nature of the relationship between language and thought has been quite elusive. We believe that its understanding is crucially dependent on the available notions of language and thought. *Foundations of Language* offers an unusually clear and complete account of both, providing a fruitful and much needed framework for future research. No doubt it will *help us think* better about these elusive complexities.

In a recent article published in this journal, philosopher Peter Carruthers put forward the hypothesis that natural language

(more specifically, the Logical Forms [LF] underlying natural language sentences) is the mechanism that enables what he calls “intermodular and non-domain-specific thinking” (Carruthers 2002). According to this view, each domain-specific-module can translate its mentalesse thoughts into LFs due to the language faculty. This common format, in turn, is what enables the combination and integration of information from different modalities to occur.

I believe that one of the reasons that Carruthers appeals to LFs is the prestige of Chomsky's theories. In some respects this prestige is fully deserved, but because of his selection, Carruthers is “limited” by a theory that has no semantic motivations, and which is not concerned with linguistic performance (only competence). And all this in spite of the fact that his main concern is to understand “how we think.”¹ On the other hand, Carruthers' hypothesis is based on a syntactocentric theory: All generative power comes from syntactic structure alone; the semantic and phonological components are derived from it.

One of Jackendoff's main concerns in his new book, *Foundations* (Jackendoff 2002), is to provide a critical view of the syntactocentric viewpoint that permeates modern linguistics and the isolation from the discipline that it has imposed on the rest of the mind/brain sciences. In what I see as an important *methodological lesson* of *Foundations*, we must begin our theorizing by establishing the *boundary conditions* of a specific problem (e.g., by “thought” we understand such and such . . .). Only then will we be able to see what kind of architecture may arise from it, while making as few assumptions as possible. In a certain sense, this is what *Foundations* is all about – and on a massive scale.

In *Foundations*, as well as in the author's previous work (Jackendoff 1987; 1996; 1997, Ch. 8), the whole problem of the relationship between language and thought is expressed with what I consider to be unusual clarity. In this framework, semantics, phonology, and syntax are different and autonomous computational spaces, each connected to the others by interface rules. Here, the *locus* of thought is at the level of Conceptual Structure, and this is where the integration of information – one of Carruthers' main concerns – takes place. It is the combinatoriality at the level of Conceptual Structure, and not at the level of Syntactic Structure (or LF based on syntactic structures), which enables the integration of conceptual information in the generation of more complex thoughts. In addition, Jackendoff delves into Conceptual Structures and shows us a rich landscape of substructures composed of different tiers (descriptive, referential, and informational tiers; Spatial Structure; Qualia structure).

This architecture leaves language with a role in thought which we believe to be more interesting than the one that a coarse syntactic structure would be capable of providing. It is also a more interesting role than what a hypothetical “semantic level” – distinct from the conceptual level – would be capable of fulfilling. Jackendoff's proposal is basically that the tripartite nature of language *permits* the mapping of unconscious conceptual representations (through syntactic representations) onto conscious phonological representations.² As Jackendoff puts it:

Linguistic form provides one means for thought to be made available to awareness (another is visual imagery); we “hear the little voice in the head” and thereby “know what we are thinking.” Notice however that the form of the awareness in question is essentially phonological. What we “hear” is *words*, pronounced with *stress patterns*. At the same time, one cannot define rules of inference over phonological structure, so it is not an appropriate medium for reasoning. The correct level for carrying out reasoning is conceptual structure, and reasoning can take place even without any connection to language, in which case it is unconscious. (Jackendoff 2002, p. 274)

In a stimulating article entitled “How Language Helps Us Think” the author provides some cues on the role of language on thought:

Language is the only modality of consciousness that makes perceptible the relational (or predicational) form of thought and the abstract elements of thought. Through these elements being present as isolable en-

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 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 register 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

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 untouchedability 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

and those that are semi-productive. Though he admits that even with these two variations the meanings of, say, denominal verbs, will not be fully predictable, he maintains that this is the best that can be done and that the inconsistent outputs must simply be listed in long-term memory (p. 159). No attempt is made to account for the unpredictability by other means, that is, conceptual metaphor, prototype theory. Jackendoff does not make the connection because he is still too attached to the notion that linguistic structure is separable from meaning (cf. Janda 1993; Wierzbicka 1985). If he acknowledged the possibility that structure can contain meaning, a hypothesis basic to so many thinkers outside of the generative framework (Halliday 1985; Hopper 1998; Langacker 1987; 1991; Talmy 2000), the problems he posits in Chapter 6 would not only be tremendously simplified, genuine strides could be made toward his promised integration.

But the real troubles with Jackendoff's position run deeper than that. Two of the basic tenets of generative linguistics that he insists on perpetuating, mentalism and discrete-variable combinatoriality, do not actually belong solely to Chomsky and generative linguistics; they are in fact deeply rooted in much of cognitive psychology and psycholinguistics. Ironically, these very tenets are now at the forefront of a massive challenge to traditional cognitive science, coming from the recent confluence of dynamical, ecological, and embodied approaches to cognition in general (Elman et al. 1996; Port & Van Gelder 1995; Van Orden et al. 2003).

In his discussion of mentalism (Ch. 2), and how to avoid the huncular problems endemic to it, Jackendoff sidesteps into an elegant description of how all the phonological, syntactic, and semantic properties of a sentence could be encoded as a location in a high-dimensional space (he even gets amazingly close to describing graded attractor basins, without using that term). But he then laments that this static high-dimensional space framework cannot address the time-dependent nature of one variable being followed by another variable. In fact, in recent dynamical-systems accounts of syntax, that time-dependent nature of sentence processing is quite nicely accommodated by a trajectory (rather than a single location) moving through the state-space (e.g., Culicover 1999a; Tabor & Tanenhaus 1999).

In his discussion of combinatoriality and language processing (Chs. 3 and 7), rather than turning a blind eye to psycholinguistic data, or invoking a hard competence/performance distinction, Jackendoff responsibly embraces experimental evidence for continuous interaction between linguistic "modules" (curiously postulating "interface modules," rather than questioning the modularity framework itself; cf. Uttal [2001]). Yet he still maintains the notion of discrete-variable combinatoriality in language, as though he were unaware of its collision course with a combinatorial explosion when faced with the on-line incremental processing of real-time language comprehension. This combinatorial overload of working memory is precisely what led researchers to propose modular syntactic heuristics (e.g., Frazier & Fodor 1978), which, in the end, did not stand up to tests of interaction with semantics and discourse (e.g., Spivey & Tanenhaus 1998; Trueswell et al. 1994). Therefore, if incremental language comprehension does indeed involve an interactive constraint-satisfaction process, then in order to avoid a combinatorial explosion of discrete "cognitive entities," it cannot possibly be using discrete-variable combinatoriality. Instead, language processing is perhaps better described as a single trajectory through a high-dimensional space of multiple competing attractor basins. In such a framework, the combinatorial expansion resulting from incremental temporary ambiguity becomes an issue of graded state-space resolvability (admittedly, no small problem), rather than an issue of exceeding a countable capacity limitation in a "working memory module," and hence no system overload.

Overall, this courageous and wonderfully written attempt by Jackendoff to please both sides of a rather bitter war may instead leave both sides teased and unsatisfied. The book serves as a detailed confession of the limitations of generative linguistics, with only hints about alternative frameworks that are more successfully

grappling with crucial issues that generative linguistics has either complicated into oblivion or chosen to ignore. It is unfortunate that one of the few opportunities he allows himself to directly address theories outside of generative linguistics is used to accuse them of not making "full contact with the overall goals of generative linguistics" (p. 269). Somehow Jackendoff maintains his allegiance to generative linguistics, despite revealing, again and again, deep insights into the failures and weaknesses of the framework. One cannot help but wonder, when his rescue attempt reaches this lost expedition, will they just eat him too?

From Frege to dynamic theories of meaning

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Abstract: In designing stratified models of human language, understanding notions of logical consequence and validity of inference require separating the aspects of meaning that vary between models from logical constants. Modelling meaning requires choices regarding the primitives, where the Fregean program is still offering us the fundamental insights on the role of truth, judgement, and grasping or sharing of thoughts.

Kudos to Ray Jackendoff for proclaiming so vigorously in *Foundations of Language: Brain, Meaning, Grammar, Evolution* that the time has come to haul down the "firewalls" separating linguistic theories into a fragmented multitude of explanatory models, which restrains us from advancing the ecologically sound research program of how we understand language (p. 329). It is very much en vogue indeed to bid farewell to narrow syntax-centered explanation, to make room for interfaces not only to other modules of grammar, but also to results of psycholinguistic experiments and of other scientific advances in neuroscience and cognition. The tangible bottleneck still is to characterize a feasible account of how these interfaces are to be modelled, what these models are intended to represent and how core concepts at one tier in such stratified models are mapped onto a next higher tier. Unfortunately, Jackendoff's efforts to help us pass this bottleneck cannot serve this otherwise laudable purpose, as explained below.

Frege's agenda, set well over a century ago (Frege 1892), characterized meaning as whatever it was, disregarding the subjective associations and contextual factors that determined reference. Expressions of various syntactic categories, including sentences, could co-refer and yet differ in meaning. Thoughts, that is, meanings of sentences, could hence be shared in communication to produce mutual understanding. Concepts, that is, the meaning of predicates, were considered to be unsaturated functions needing objects to determine the truth-value of a proposition. Much of the subsequent century of semantics research was given to explaining Frege's notion of meaning, modelled in possible world semantics of the Carnap-Kaplan-Montague-Kripke-Lewis lineage as functions from primitive possible worlds to extensions of the appropriate type. Jackendoff proposes an interesting slant on Fregean theories of meaning when stating: "Frege's problem is not a uniquely linguistic problem; rather, it lies in a more general theory of how the f-mind keeps track of individuated entities" (p. 314). This more dynamic view of Frege's theory of meaning has been developed and advocated by Gareth Evans (1985) – not mentioned at all in *Foundations* – who did more to advance our understanding of meaning and interpretation in his short life span than most of us with longer life may ever hope to do.

Jackendoff pleads for the primacy of speaker reference, rather reminiscent of Grice, Donnellan, and Kripke (cf. Hale & Wright 1999), correctly emphasizing a very Fregean notion of judgement to connect concepts to "reality" (Jackendoff's percepts). He admits to coming close to Kamp's Discourse Representation Theory (DRT) and its variants of dynamic semantics, since reference gives

the interpreter something to bind descriptive information to. Although DRT accounted nicely for the fact that indefinites in the scope of negation cannot bind pronouns in subsequent sentences, for example, “Joan did not buy [a car]₁. **[It]₁ is a Jaguar,*” the box notation Jackendoff introduces does not use boxes to characterize accessibility constraints for pronoun resolution, neither does it have a proper semantics in which valid inferences can be characterized.

We part company when, rather surprisingly, Jackendoff is led to conclude that “there is little to be gained in theoretical economy or explanatory adequacy by making a strict cut between logical and non-logical properties . . . logical properties are just some heterogeneous subset of semantic properties” (p. 289). What is logical and what is not depends on one’s choices in modelling meaning, but anyone must make such choices. If one desires to model inference as a matter of form, irrespective of content, an inference is logically valid just in case the premises entail the conclusion in every possible model, for its validity must depend only on its logical constants. Choices of the modeller also determine the furniture of the models: I concur entirely with Jackendoff’s “yes” to events and “no” to possible worlds. But he erroneously believes that variables for places are new with his conceptual semantics (p. 322), for Situation Semantics already introduced locations as constituents of their situation types (Barwise & Perry 1983). Truly puzzling is his later usage of lambda-abstraction, usually forming functions from truth-value denoting expressions, but now filling terminal nodes in conceptual tree structures with semantic objects as labels of higher nodes (p. 389). For this technique familiar from model theoretic semantics to do any work, the tree structures must be given a semantics, specifying in a Fregean, compositional way how functions and arguments may be composed to compute appropriately typed values. It can be adopted in DRT as well, assisting in separating compositional aspects of meaning from constraining accessibility in declaring reference markers, and opening a route to alleviate representation by delegating more to inference rules, as in Blackburn and Bos (1999). No semantics, no inference; no inference, no interpretation; no interpretation, no understanding!

What we badly need in order to advance the scientific study of human language understanding is a novel language, a scientific pidgin to communicate across different tiers of modelling meaning. Our explanatory theories need not be incommensurable, if we strive to integrate the various explanatory toolkits carefully and systematically. Because Jackendoff has written his book to promote a culture of collaboration, he should be responded to by incorporating the many useful insights, such as the three aspects, sensory, material, and constitutive, of his “telic quale” into current theories of meaning and interpretation.

The architecture is not exactly parallel: Some modules are more equal than others

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Abstract: Despite its computational elegance, Jackendoff’s proposal to reconcile competing approaches by postulating a parallel architecture for phonological, syntactic, and semantic modules is disappointing. We argue that it is a pragmatic version of the leading module which Jackendoff would probably prefer, but which he does not explicitly acknowledge. This internal conflict leads to several shortcomings and even distortions of information presented in the book.

Foundations of Language: Brain, Meaning, Grammar, Evolution (Jackendoff 2002) presents a major revision of the syntactocentric approaches in contemporary linguistics. It may well become an established interpretation for the next decades, especially in view of difficulties with an empirical refutation of such a multicomponent model. Taking apart the delicate question of what belongs to an interface and what is a core module of one or another domain (the answer may in fact vary for different languages), we would like to comment on another issue central to Jackendoff’s approach: the overall architecture and its relations to the function of linguistic mechanisms. As in the case of several other, presumably parallel, mechanisms of processing (e.g., dorsal and ventral streams of visual processing, or semantic and episodic memories), what is stressed in the book is the parallelism, not the hierarchical organization of the modules. Clearly, Jackendoff also undermines systematic asymmetries in the relationships of the constituent mechanisms.

When considering language use from a functional point of view, that is, as goal-oriented activity, a distinction between leading versus background functions should be made. For the whole domain of language, such a leading function is the communication of meaning and enhancement of social coordination. Variable and progressively automated syntactic and phonological processes are only means to achieve this general goal. This is also what Jackendoff emphasizes when he writes, “Phonology and syntax, after all, evolved to *express* meaning, which has a far longer evolutionary pedigree. Moreover . . . much of conveyed meaning is tacit, not expressed at all in the words uttered or the syntactic structure combining them” (p. 428). The testimony to a priority of meaning evaporates when the modular architecture starts to be discussed.

The problem is not so much that semantics is “only equated” in its role to phonology and syntax. Strangely, the upper level – variously named as pragmatic, executive, or metacognitive – is rarely overlooked. When Jackendoff writes that interpersonal tuning (“attunement,” or also “alignment”; see Pickering & Garrod, in press) is “the last piece” (p. 322) in the overall picture, our impression is that it has to be the first, and not only in evolutionary or ontogenetic considerations. For instance, recent experimental data consistently prove that interpersonal (pragmatic) variables, such as addressees’ needs, influence syntactic forms early in on-line processing (Lockridge & Brennan 2002).

There are even some distortions and omissions, amounting to cognitive scotomata, in this otherwise precise and almost exhaustive text. For example, take Jackendoff’s entire line of analysis of communication and interpersonal cognition. Jackendoff mentions the “theory of mind” only once, in a footnote on page 323, where its development is attributed to the fifth year of life; whereas in fact it culminates (with a relatively difficult false-belief task) already in the fourth year (see Perner & Lang 1999, among many others) and, what is important, it has a long history of earlier achievements, such as a “social cognitive revolution” at nine months (documented by Tomasello 1999a), when children start to consider other people as intentional agents. The related steps are self-recognition in a mirror test and empathy for others, both important motives for communication. Further, Jackendoff ignores completely the rich research literature on joint-attention effects (not to be confused with the less specific idea of joint intention) in language development and practical interaction.

The second scotoma is the virtual lack of neurolinguistic and neuropsychological data although, ironically, “brain” is the first word in the subtitle of the book. The prefrontal cortex is of particular importance as the crucial part of interpersonal neurocognition in that its damages correlate with autism and the underdevelopment of language (e.g., Stuss et al. 2001). Relevant data demonstrate that prefrontal executive functions go beyond working memory, being connected to attentive processing, task switching, and resource allocation (the dorsolateral prefrontal cortex) and to emotions, self-reference, and social behavior (ventromedial and frontopolar areas). This is where we should look for brain mechanisms of not only joint intention, but also joint attention and creative usage of language in situated collaborative actions.

These examples show how thorny is the path from the one-sided conceptualizations of the last half-century. The book demonstrates certain first cautious steps. Universal Grammar (UG) is now seen as an attractor state. Semantics has been equated to phonology and syntax. The latter has been reduced, in its proto-form, to simple (and thoroughly pragmatic) heuristics of the type: “first noun is the actor” and “focus first” – which is already known from the literature on child development, as well as from aphasiology (e.g., Velichkovsky 1996). However, for a reader from the discourse-and-activity-oriented camp, this formidable work is also an illustration of the attractor power of generative grammar, which is still preventing the due revision and proper integration of linguistics with the rest of cognitive science. Sooner or later, we will certainly come from UG to something like *UP*, that is, the *universal pragmatics* of cooperative action, with exact solutions for syntactic peculiarities of different languages (Greenberg 1986; Kibrik 2001).

Linear order and its place in grammar

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Abstract: This commentary discusses the division of labor between syntax and phonology, starting with the parallel model of grammar developed by Jackendoff. It is proposed that linear, left-to-right order of linguistic items is not represented in syntax, but in phonology. Syntax concerns the abstract relations of categories alone. All components of grammar contribute to linear order, by means of the interface rules.

1. The problem of linear order. One of the important lines of thought in Jackendoff’s (2002) book *Foundations of Language: Brain, Meaning, Grammar, Evolution* is that Generative Grammar (GG) was wrong in presupposing syntactocentrism, the view that only syntax generates structures and that these structures are merely interpreted by the other components of grammar, phonology, and semantics. Syntactocentrism should be replaced by a parallel architecture of grammar, which gives phonology, semantics, and syntax equal rights in the grammar.

In this commentary, I will argue that the case against syntactocentrism should be carried one step further: The division of labor between syntax and phonology is not complete in Jackendoff’s present description of these components. I propose that linear order has no place in syntax; order (what remains of it after syntactic and semantic constraints have had their say) is under the control of phonology or the syntax-phonology interface rules. In other words, while Jackendoff claims that syntactic structure represents (a) categories, (b) linear order, (c) constituency (p. 24), I would like to propose that (b) be removed from this list.

2. Why syntax is neither necessary nor sufficient. In discussing possible steps in the evolution of language (Ch. 8, pp. 255ff), Jackendoff uses a set of examples in which syntax does not specify the position of a phrase within the sentence. As demonstrated here, sentence adverbials are free to occur before or after the sentence, or in the major break of the sentence. That is, the model proposed by Jackendoff is very explicit about the fact that syntax underdetermines the actual linear order of constituents in a sentence.

It is also well known in general that some ordering phenomena crucially relate to nonsyntactic information. Perhaps foremost among these phenomena are the so-called “heaviness shifts,” that is, presumed movement operations which shift heavy, that is, long constituents toward the left or right edge of a sentence. I claim that length in these cases is phonological length, measured perhaps in number of syllables or phonological words, and cannot be measured in terms of number of grammatical words or syntactic

complexity. In considering the syntax-phonology relation, Jackendoff points to examples in which prosodic demands, such as heaviness, are active in overriding syntactic requirements (pp. 120f). For cases such as these, the conclusion must either be that prosody (heaviness) can override syntactic rules, or that syntax simply has nothing to say about the actual order.

The parallel structures proposed by Jackendoff are synchronized by the use of subscripts identifying corresponding pieces of information (such as words). Now, given the fact that the phonological structures as given in Figure 1.1 of *Foundations* and throughout the book are fully specified with respect to linear order, all other order information, in the syntax or in the semantics, is redundant and superfluous. Any information on the linear order of a linguistic expression can be read off the phonological structure. We note in passing that semantic structure is ignorant as to linear order. It is only in the syntax that ordering information is duplicated. Within a discussion of constraints on syntactic rules (Ch. 3, sect. 3.2.3), Jackendoff actually lifts this observation into an interface constraint for the correspondence of syntactic and phonological structures (17b, p. 50): “The linear order of elements in phonological structure (normally) corresponds to the linear order of corresponding elements in syntactic structure.” This does not do away with the redundancy.

Second, the sort of syntax advocated by Jackendoff right from the beginning of his book is characterized by a considerable amount of abstractness, by structural elements which do not contain actual words in their actual order. For example, the syntactic structure of *The little star’s beside a big star* (p. 6 and later) has terminal elements consisting of grammatical features alone. Their position in the tree bears little relation to the fact that they are (often, but not always) realized at the end of their respective words. Some syntactic items can also be “unpronounceable,” as noted on page 13. For such elements, it makes no sense to specify ordering information, whereas information on the dominating constituent is relevant, in fact crucial. The same point holds for lexical items: While it makes sense to place the past tense morpheme in the word *devoured* at the end of the syntactic structure for this word (see [6] in Ch. 6.2.3), it does not make sense for the irregular verb *ate*, as in (7). As the placement of the regular *-ed* suffix is also specified in the phonological structure of *devoured*, I conclude that it is more adequate to let the syntax be ignorant about the linear order of inflectional morphemes.

Finally, in his discussion of a possible time-course of incremental language evolution, Jackendoff characterizes the protolanguage “concatenation of symbols” as the “beginning of syntax” (p. 241). It is not clear whether this connection is a necessary one. A juxtaposition of symbols in a protolanguage could well be “syntagmatic phonology,” for example syllabic concatenation, going hand in hand with “the beginning of semantics.” In a similar vein, Jackendoff explicitly questions (pp. 252–53, Note 11) the usefulness of versions of syntax with unordered trees. Here, he argues that it would be against the spirit of an evolutionist account of language competence not to assume that syntax contains information on linear order. I fail to see why linear order in a protolanguage must be in the domain of syntax. If the assumed protolanguage has some linear order, this order can just as well be under the control of other components.

3. The proper role of syntax in grammar. The elementary formal notions of phrase structure syntax are those of domination and sisterhood (co-domination), but not that of linear order. Two nodes A and B co-dominated by a node C are necessarily adjacent in a binary structure, but no right-to-left relationship need be assumed. Consider an elementary (but quite powerful) phrase structure syntax allowing for a head to license a complement phrase, with the result to be modified by an adjunct phrase. Schematically, this gives $\{ \{ X Y \} Z \}$ in an a-temporal syntax (with “{ }” marking nonlinear constituency, and with X, Y, Z as variables for syntactic categories). Translated into linear-order standard syntax, four possibilities arise, namely $[Z [X Y]]$, $[Z [Y X]]$, $[[X Y] Z]$, $[[Y X] Z]$. Generally, for any structure with n binary-branching

nodes, there are 2^n linear orders. This can quickly lead to large numbers, but these are still smaller than the $m(!)$ possible permutations that would result from m constituents (for $m = 3$ as in the present example, this gives six).

The present proposal is that syntax does indeed provide only for the more modest constraints given by a-temporal syntax. A-temporal syntax is sufficient to specify a crucial ingredient of syntax, called structure-dependence in many of Chomsky's publications. Structure dependence is decidedly not the specification of linear order, but the specification of domination and sisterhood alone.

Order of constituents is only partially determined by structure dependence. The remaining task is that of phonology, semantics, and pragmatics combined. I have nothing to say about the latter two, but will assume that principles of information structure (such as "Agent First" and "Focus Last," *Foundations*, Ch. 8, sect. 8.7) are of primary importance here. Again, avoidance of duplication seems to make a syntactic determination of order superfluous at best in those cases in which other principles are at work already.

4. The role of phonology. As for linear order in phonology, it is indisputable that phonology (in contrast to syntax) needs linear order as a core concept. The string of phonemes /pit/ is in contrast with the string /tip/, while /ipt/ is a possible, but unrealized word in English, and any other permutation of the three phonemes is ill-formed in English. In other words, the elementary notions of contrast, distinctiveness, and well-formedness in phonology include linear order. Structuralist phonology used the term "syntagmatic relation" in this connection; here, "syntagmatic" literally means "in accordance to the time axis." Furthermore, a number of phonological rules are generally cast in terms of linear order. For example, the basic rule of compound stress in English or German says that the *first* of two parts in a compound carries main stress. For stress in phrases, the reverse holds (simplifying considerably): the *second* of two constituents in a phrase receives main stress. In other words, phonology is very much about the temporal line-up of chunks of speech. Given that it is grounded in the phonetics of speech, this does not come as a surprise.

Furthermore, some of the syntactic movement operations assumed in syntactic theory are clearly related, at least functionally, to either information structure (as in "topic first") or to preferred positions for constituents with either strong stress (focus positions) or weak stress (deaccentuation). Given that syntax is not conceived as "knowing" about nonsyntactic principles such as stress, it is almost inevitable to assign the respective movement operations to some other domain.

5. Where does order come from? If the present hypothesis about temporally unordered syntactic constituents should be correct, it would leave us with one crucial question: From what rules or principles does the actual order (encoded in phonological structures) derive? No complete answer can possibly be given here, but parts of the answer have been identified already: Jackendoff points out in several places that there are principles of ordering which are part of semantics, information structure in particular, and of phonology, heaviness constraints and stress preferences in particular.

Lexical information (either on individual items or on more or less extended lexical classes) must be another source of temporal order: Prepositions versus postpositions are an obvious example, prenominal versus postnominal adjectives might provide a further case.

Next, phonology itself provides ordering information, as we can see from principles, such as the one requiring long constituents to follow short ones (Behaghel's law).

Setting aside the cases just enumerated, there are substantial remaining problems. My formal proposal at this point is that the rules providing the interface between syntax and phonology – Jackendoff's "PS-SS interface rules" (Ch. 5, sect. 5.6) – provide the natural locus for stating the constraints on linear order for syntactic and/or semantic constituents. Such rules are, by necessity, sensitive to information stemming from both of the components between which they mediate. Here again, the architecture of

grammar proposed by Jackendoff provides a fruitful base for further research.

How did we get from there to here in the evolution of language?

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Abstract: Jackendoff's scenario of the evolution of language is a major contribution towards a more rigorous theory of the origins of language, because it is theoretically constrained by a testable theory of modern language. However, the theoretical constraints from evolutionary theory are not really recognized in his work. We hope that Jackendoff's lead will be followed by intensive cooperation between linguistic theorists and evolutionary modellers.

There has been a vigorous debate in the evolution of language literature on whether the human capacity for language evolved gradually or with an abrupt "big bang." One of the arguments in favor of the latter position has been that human language is an all or nothing phenomenon that is of no value when only part of its apparatus is in place. From a developmental perspective this has always been a peculiar argument, seemingly at odds with the gradual development of phonological, syntactic, and semantic skills of infants. In the context of the evolution of language, the argument was eloquently refuted in a seminal paper by Pinker and Bloom (1990). However, Pinker and Bloom did not go much further than stating that a gradual evolution of Universal Grammar was possible. They did not explore the consequences of such a view for linguistic theory, and their approach was criticized by both the orthodox generativists and the latter's long-term opponents.

Jackendoff (2002) has now gone one step further. If linguistic theory is incompatible with gradual evolution and development, perhaps linguistic theory needs to be revised. Jackendoff has written a powerful book around the thesis that the language capacity is a collection of skills ("a toolbox"). Some of these skills are language-specific, some not, and each of them is functional even without all or some of the other skills present. From his decomposition of linguistic skills follow a number of hypotheses on plausible intermediate stages in the evolution of language, that fit in neatly with many other theories, models, and findings in this field.

Jackendoff's book therefore presents a significant departure from the generative, "formalist" tradition, where the evolution of language has received little attention. In this tradition, the structure of human language has often been viewed as accidental rather than as adapted to the functions that language fulfills in life. Chomsky and others have been dismissive about attempts to reconstruct the evolution of language, which they regard as unscientific speculation. Chomsky famously observed that "we know very little about what happens when 10^{10} neurons are crammed into something the size of a basketball" (Chomsky 1975).

In contrast, Jackendoff presents the different tools from the "toolbox" as adaptations for better communication. Moreover, he gives a rather complete scenario of successive, incremental adaptations that is consistent with his view on how modern language works, and how it can be decomposed. Interestingly, he argues that present-day languages show "fossils" of each of the earlier stages: expressions and constructions that do not exploit the full combinatorial apparatus of modern language. Jackendoff's book is therefore a major contribution towards a more rigorous, scientific theory of the evolution of language, in part because it leads to some testable predictions, but more importantly because it is theoretically constrained by a testable theory of modern language.

However, Jackendoff does not really recognize that, in addition, evolutionary theory brings stringent theoretical constraints (Barton & Partridge 2000). Good evolutionary explanations specify the assumptions on genotypic and phenotypic variation and selection pressures, of which the consequences can be worked out in mathematical and computational models. For instance, Nowak et al. (2001) derive a “coherence threshold” for the evolution of language, which poses a strict constraint on the accuracy of both genetic and cultural transmission of language for linguistic coherence in a population to be possible. In this type of work, one often finds that “adaptive explanations” that seem so obvious in a verbal treatment such as Jackendoff’s, are in fact insufficient.

Cavalli-Sforza and Feldman (1983) studied a “conformism constraint” that arises from the positive frequency dependency of language evolution: Linguistic innovations are not advantageous in a population where that innovation is very infrequent. Imagine, for instance, a population that is in the second state of Jackendoff’s scenario. That is, individuals can use a large vocabulary of learned signals in a non-situation-specific manner, but their language is not compositional: Signals cannot be analyzed as consisting of meaningful parts. Suppose that a child is born with a genetic mutation that makes her more inclined to analyze sentences compositionally. Would this child profit significantly from this mutation, even if the language of the population she is born into is not at all compositional? If not – and it takes some creativity to come up with reasons why she would – evolutionary theory predicts that the new gene will disappear through negative selection or random drift (Fisher 1922).

That is not to say that language did not evolve according to Jackendoff’s scenario, but just to emphasize that each of the transitions between the phases he proposes is a challenge in itself. The evolution of language is not, as is sometimes suggested, a domain for just-so stories. Rather, it turns out that it is very difficult to find even a single plausible scenario for the evolutionary path from primate-like communication to the sophisticated toolbox of human language that will survive close scrutiny from mathematical and computational modeling. Recently, this insight has led to a surge in the interest in “explorative,” computational models (see Kirby 2002b; Steels 1997; for reviews). They have yielded intriguing ideas on adaptive and nonadaptive explanations for the emergence of shared, symbolic vocabularies (e.g., Oliphant & Batali 1996), combinatorial phonology (e.g., de Boer 2000; Oudeyer 2002), compositionality and recursive phrase-structure (e.g., Batali 2002; Kirby 2002a).

For instance, the suggestion of Kirby (2000) – referred to but not discussed in Jackendoff’s book – is that a process of cultural evolution might facilitate the emergence of compositionality. If a language is transmitted culturally from generation to generation, signals might frequently get lost through a bottleneck effect (that arises from the finite number of learning opportunities for the child). Signals that can be inferred from other signals in the language, because they follow some or other systematicity, have an inherent advantage over signals that compete for transmission through the bottleneck. With some sort of generalization mechanism in place (not necessarily adapted for language), one always expects a language to become more compositional (Kirby 2000), and, more generally, better adapted to the idiosyncrasies of the individual learning skills (Zuidema 2003).

Throughout his book, Jackendoff uses metaphors and terminology from computer science. Terms like processing, working memory, and interface make it sometimes appear as if he is describing a computer rather than processes in the human brain. However, nowhere do his descriptions become sufficiently formal and exact to make them really implementable as a computer program. In this light, his criticism of neural network models of language acquisition and his mentioning only in passing of computational models of the evolution of language is unsatisfactory. Jackendoff’s challenges for connectionists are interesting and to the point, but it is equally necessary for theories such as Jackendoff’s, especially their implications for development and evolution,

to be made more precise and to be extended in computational and mathematical models.

In sum, in the effort to find a plausible scenario for the evolution of human language, a book like Jackendoff’s *Foundations of Language*, based on a broad and thorough review of linguistic theory and facts, is extremely welcome. But as explorative computational models such as the ones discussed have been very fruitful in showing new opportunities and constraints for evolutionary explanations of human language, we hope that Jackendoff’s lead will be followed by intensive cooperation between linguistic theorists and evolutionary modellers.

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Author’s Response

Toward better mutual understanding

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Abstract. The commentaries show the wide variety of incommensurable viewpoints on language that *Foundations of Language* attempts to integrate. In order to achieve a more comprehensive framework that preserves genuine insights coming from all sides, everyone will have to give a little.

R1. Goals

My goal in writing *Foundations of Language* was threefold. First, I wished to develop a framework for studying language – the parallel architecture – which would permit a better integration of all the subfields and theoretical frameworks of linguistics with each other and with the other cognitive neurosciences. Second, I wished to persuade linguists to join more fully in this integrative enterprise. Third, I wished to persuade cognitive neuroscientists outside linguistics that the past forty years have brought genuine insights in linguistic description – albeit somewhat obscured by the technical opacity of linguistic theory – and that the parallel architecture offers better prospects for renewed dialogue. The commentaries suggest that I have succeeded to some extent, but that there still is a long way to go and a lot of preconceptions to overcome (including, no doubt, my own). The difficulties of integration are legion: The study of language, more than any other cognitive capacity, stretches the limits of interdisciplinarity, all the way from neuroscience and genetics to social policy and literary theory, with linguistics, psychology, and anthropology in between.

Many of the commentators focus on issues in *Foundations* that are touched upon only tangentially or not at all in the précis appearing here. In this response I will do my best to make clear what is at stake. My hope, of course, is that readers will thereby be engaged enough to want to tackle the whole book.

R2. Sociology

Let me clear some sociological remarks out of the way first. Some commentators found the book fatally flawed because it has abandoned traditional generative grammar and philosophy of language (**Adams, Freidin, Higginbotham, Jerzykiewicz & Scott, ter Meulen**), whereas others found the book fatally flawed because it clings to traditional generative grammar, which to them is clearly a dead letter (**Edelman, Lavie, MacAogáin, Spivey & Gonzalez-Marquez**). Edelman compares me to Khrushchev propping up the doomed Soviet regime, neglecting the fact that I concur with him in emphatically rejecting Chomsky's Minimalist Program (and also neglecting the fact that I have no secret police or gulag with which to suppress dissent). Spivey and Gonzalez-Marquez compare generative grammar to the Donner Pass party, who ended up eating each other. Although I acknowledge that linguists can often be less than civil (see the discussion of the generative semantics dispute, *Foundations*, pp. 73–74), I submit that the debate isn't always that much better elsewhere.

In both groups who dismiss the work, I find a reluctance to acknowledge my larger goals. As the Précis says, "To understand language and the brain, we need all the tools we can get. But everyone will have to give a little in order for the pieces to fit together properly" (sect. 1, para. 1). On one hand, the structures that linguists have discovered represent real empirical generalizations and real problems for learning; on the other hand, we do have to figure out how the neurons do it. I think a total integration of these inquiries is still quite a few years off, but I am trying to find a way to move each of them in a direction that recognizes the mutual value of the other in achieving the common goal.

I am also delighted that several commentators see possibilities for linking my parallel architecture to their own concerns. For instance, **Garrod & Pickering** see the architecture as an opening for connecting linguistics with the study of discourse, an issue *Foundations* touches on very superficially at the end of Chapter 12. **Érdi** offers a smorgasbord of issues in network theory, chaos theory, and dynamic systems theory that make contact with points in my discussion.

R3. Literature of which I was unaware

As I acknowledged in the Preface, the undertaking is by this point too large for one person to grasp all of its parts equally. If nothing else, there just is too much literature and it grows far faster than anyone can read much less digest it. Several commentators suggest that there is relevant literature out there that I should have read. Guilty as charged, but after all life is short and you have to make your choices: either read for another 90 years or write the damn book, knowing you haven't covered all the bases.

In particular, **Catania** points out many parallels between my thought and Skinner's. These may well be valid and worth examination. On the other hand, I am not aware of any work emerging from the Skinnerian tradition that approaches the level of detail of linguistic organization routinely investigated by generative grammar (and discussed in Chs. 1, 5, 6, 11, and 12 of *Foundations*). This may be my ignorance, or it may be a sociological consequence of behaviorism's eclipse since the cognitive revolution, or it may be an inherent insufficiency in behaviorist theory. Likewise,

Lockwood points out parallels between my work and Sydney Lamb's, some of which I briefly alluded to in *Foundations* (cf. Précis, Note 9), and much of which I was unaware of. In this case I am happy to report that I had a productive discussion with Lamb at a conference in 2002 after the publication of *Foundations*. We could see the commonalities, but he also acknowledged that his neurally inspired approach could not solve my Problem of 2 ("How does a neural network encode multiple tokens of a known category in working memory?"), crucial to an account of language processing (cf. *Foundations*, pp. 61–63).

Csépe observes that there are other models of working memory besides the one I take to task, citing in particular that of Just and Carpenter (1992) as better fitting my account. I am pleased. **Zuidema & de Boer** refer to literature on mathematical constraints in evolutionary theory that I was only vaguely aware of; certainly I am not conversant enough with the mathematics to bring this research to bear usefully on my approach. Again, I would be happy if my work could add greater linguistic sophistication to this valuable line of inquiry.

Howard and **Schnelle** each point out theories of neural architecture and function with which I was unacquainted. I am glad to see that they acknowledge the challenges for neuroscience presented by the combinatoriality of language, and I am glad that they claim to have solved some of them. This is all to the good. However, I don't think it is for me to evaluate their models; it is a long intellectual stretch from details of syntax to details of neurons. Rather, I can hope that they might use their models' convergences with the demands of language processing to try to win greater acceptance in the broader neuroscience community.

Velichkovsky, Kibrik, & Velichkovsky (Velichkovsky et al.) allude to a literature on language as communication and enhancement of social coordination with which I was not acquainted. Again, I see no conflict here. I think the parallel architecture, in particular the independence of semantics from syntax, opens the door to connections being made in a way that is impossible within the syntactocentric architecture. **Arbib** draws attention to the HEARSAY architecture for speech understanding, which introduced the notion of a "blackboard" for parallel computation of different levels of language structure. I was aware of the term as something in common currency but was not aware of its original source. **ter Meulen** reminds us of the notion of Place in semantics, as proposed within Situation Semantics by Barwise and Perry (1983), so I was not alone. I do not remember whether Barwise and Perry refer to the discussion in Jackendoff (1978). In any event, I am sure ter Meulen would agree that the notion has not exactly taken hold in formal semantics as a whole. Thanks to all of these commentators for pointing out the further connections.

R4. Localization of brain function

Several commentators take seriously the possibility of connecting components of the parallel architecture to brain location, and raise interesting issues about whether the theory makes the right distinctions. **Csépe** asks what could be meant in a biological sense by inheritance, innateness, and wiring. This question applies of course to any formal theory of grammar, and *Foundations* (Chs. 2–4) stresses that it is one of the major problems to be faced in unifying neuro-

science with a formal theory of language or of any other cognitive capacity. Csépe goes on to ask whether the three generative components have neural correlates, and she cites various imaging data concerning the separability of semantic, syntactic, and phonological processing, as well as finer distinctions such as morphosyntax versus phrasal syntax. The parallel architecture also bids us ask how the interfaces among the generative components are neurally realized, presumably as connections among areas that subservise different formats of structure. On the other hand, there is the possibility that some of the components interweave in the brain or that some of them (especially semantics) are spread out among various brain areas. Whatever the answers, the parallel architecture makes for a better potential correlation between components of the theory and areas of the brain, in the same way that *Foundations* demonstrates better correlations between components of the theory and components of processing.

Goldenberg observes that left brain damage often results not just in some variety of aphasia, but also in diminished performance in certain nonverbal sorting tasks and high-level disorders of motor control (apraxias). He suggests that the common element among these three is recombination of a finite repertoire of elements into new combinations. He is wise enough to not say that language reduces to these other functions, but rather to say that all three are special applications of this common function. In itself this proposal has no bearing on which theory of grammar one adopts, except that it may help explain the general location of language in the brain. Still, I find this approach a friendly addendum to the parallel architecture, with a (to me) novel proposal about the evolutionary antecedents of combinatoriality.

Gervain wonders whether the double dissociation of lexically stored and rule-generated past tense forms is a problem for my treatment of productive versus semi-productive regularities, which she says I conflate. In fact, my claim (*Foundations*, Ch. 6; Précis, sect. 8) is that the semi-productive forms are stored in long-term memory as a whole, but the productive forms arise from free combination of a stored stem with a stored affix. Thus, the processing involved in relating a semi-regular past to its present is lexical association, but the processing involved in relating a regular past to its present is variable instantiation. I think this provides room in the theory for the observed double dissociation in processing.

Kemmerer, in one of the most interesting of the commentaries, shows how evidence from neuroscience might be brought to bear on fine details of the theory. *Foundations* claims (Ch. 11) that certain aspects of meaning are encoded not in the algebraic format of conceptual structure but, rather, in some visuospatial format; Kemmerer cites references from the neurolinguistics literature that support this view.

On the other hand, **Kemmerer** takes issue with my claim (Ch. 9) that within conceptual structure there is no principled distinction of format between those aspects of semantics that are relevant to grammar (time, person, evidentiary status, etc.) and those that are not (the distinction between *dog* and *kangaroo* or between *five* and *six*). In particular, the distinction between caused motion and caused change of property is correlated with the syntactic difference between (1a) and (1b), which can however describe the same event.

- (1) a. Sam sprayed water on the flowers. [water moves to position on flowers]
- b. Sam sprayed the flowers with water. [flowers come to have water on them]

However, other verbs permit only one construal:

- (2) a. Sam dripped/poured/spilled water on the flowers.
- b. *Sam dripped/poured/spilled the flowers with water. [unacceptable]
- (3) a. *Sam drenched/doused/soaked water on the flowers. [unacceptable]
- b. Sam drenched/doused/soaked the flowers with water.

Kemmerer's experiments reveal a double dissociation in different aphasias: Some patients could distinguish the meanings of verbs within classes (2) and (3) but could not judge (2b) or (3a) to be ungrammatical, and some patients were just the reverse. This suggests that the grammatically relevant aspect of meaning that distinguishes (2) from (3) is neurally segregated from the grammatically irrelevant aspect of meaning that distinguishes verbs within the classes.

Kemmerer offers a reconciliation of this finding with my position: that the neural structures that implement grammatical semantics might not be genetically programmed for this function, but they become functionally specialized as the child learns. He observes that this resolution accommodates the fact that such grammatically relevant aspects of meaning vary considerably from one language to the next. I am sympathetic to this suggestion. If a piece of meaning is relevant to grammar, it must be encoded as one component of an interface rule, perhaps as constructional meaning; the other end of this interface rule is a bit of syntactic and/or phonological structure with which this meaning correlates. Many such rules must be learned, although some, such as the preference for Agents to be subjects, are so widespread as to suggest that they are wired in. By contrast, grammatically irrelevant aspects of meaning will appear not as part of a general interface rule, but only in the mapping of individual word meanings from semantics to phonology. This difference might be the basis for the dissociation Kemmerer observes.

The argument in *Foundations*, therefore, was that grammatically relevant aspects of meaning don't differ in *format* from grammatically irrelevant aspects of meaning – that is, there is no separate level of linguistic semantics distinct from general-purpose meaning. What makes a particular piece of meaning grammatically relevant is its playing a role in a relatively general interface rule between meaning and syntax.

The larger question for linguistic theory and cognitive neuroscience is to determine the exact range of possible grammatically relevant aspects of meaning. Person is always relevant, and number, relative status of the speaker and hearer, causation, agenthood, patienthood, evidentiary status, time, and many other things often appear. The distinction between (2) and (3) appears to be a special case of the more general principle that direct objects are construed as Patients if possible; the special use of *with* in (3) appears to be a construction of English (Jackendoff 1990, Ch. 8) with parallels in other languages as well. In any event, **Kemmerer's** work is exactly the sort of research in neurolinguistics that is pertinent to the parallel architecture; the interest arises from the close contact between the theoretical model and its possible interpretation in brain terms.

R5. Evolution

A number of commentators addressed themselves to my discussion of the evolution of language (*Foundations*, Ch. 8; Précis, sect. 9.4). I ought to make my goal in this chapter clear. One of the issues for the evolution of the language faculty is how its apparent complexity could have evolved incrementally, in such a way that each stage was a usable form of communication and each successive innovation was an improvement on the previous system. I offered a hypothesis about such a sequence of innovations, with no presumptions about the absolute timing. Various stages could have been nearly simultaneous or widely spaced in time: It is the *relative* order that makes a difference. In addition to the standard sorts of evidence offered for the evolution of language, I was able to connect some of my hypothesized stages with the present-day architecture of grammar. In particular, certain grammatical phenomena appear as “fossils” of earlier stages.

Erdi is enthusiastic about the idea that language arose in the visual-gestural modality and changed later into the auditory-vocal modality, citing the recent investigations of “mirror neurons.” *Foundations* said that this would not materially change my story, and I stand by this statement. Even if visual-gestural language did emerge first, it is still necessary to account for the emergence of the auditory-vocal modality, in particular the digitization of the speech signal into phonological segments – a major innovation, for which there are no animal homologues or analogues. Visual-gestural origins might permit some differences in the ordering: The amazing expansion of the vocabulary (which in my story is interdependent with phonology) could precede the initiation of phonology, and some of the syntactic innovations could as well. In the end, however, all the innovations must still take place, and at the moment I know of no non-speculative evidence for the primacy of the visual-gestural modality in language (possibly my ignorance of course), so I would prefer to remain agnostic.

Arbib mentions a number of capacities that had to exist prior to getting language off the ground at all: imitation (which I mention); symbolization (which I take to be the essential move); parity (which I neglected, but about which he is right); intentional communication (for me, probably part of symbolization, but worth separating out); beyond the here-and-now (which I take to be a characteristic of primate thought); paedomorphy and sociality (with which I agree); and the ability to time actions in relation to hierarchical goals (I agree here too). But he seems to think this is all one needs to get language: “What is universal is the need for expression, not the choice of linguistic structure for meeting those needs.” This – again – does not take into account the digitization of phonology, which calls for something more than just a need to express oneself. (*Foundations*, p. 244: “As many linguists [references omitted] – but not many nonlinguists – have recognized, the innovation of phonological structure is a major cognitive advance.”) Chapter 4 (cf. Précis, sect. 2) offers a list of well-known symptoms that collectively suggest that language is a biological specialization that goes far beyond just a need for expression.

Arbib takes the position that case and agreement systems are cultural inventions, whereas I supposed that they are a product of late stages of evolution in the language faculty. This is an interesting topic for future research. It would certainly be nicer if these elements of grammar were not

partly specified by the toolkit of Universal Grammar: there would be less needed in the genome, and less necessary innovation for evolution. On the other hand, one would want to account for the linguistically widespread properties of case and agreement systems – what happens and, crucially, what *doesn't* happen. One would also want to account for the fragility of these systems in agrammatic aphasia, Specific Language Impairment, and second language learning: the impairments appear not to be due simply to phonological difficulties.

Bickerton, to whom *Foundations* gives grateful credit for his insight into the evidence for an evolutionary stage of protolanguage, complains that there is no evidence for my further decomposition of the evolutionary process. But he flatly denies, without argument, the difference between the use of symbols (which might be limited to a small innate or learned vocabulary, like primate calls) and the use of an open and unlimited class of symbols, which requires the possibility of imitating others and learning fast, and, at least for some individuals, the possibility of innovating symbols. He similarly denies any difference between these and the innovation of the phonological combinatorial system, ignoring my argument that the digitization of phonology is necessary in order to keep a vocabulary of thousands of symbols separate in memory and perception. Then he offers the raw speculation: “It seems highly likely that language’s two combinatorial systems came in together, perhaps exploiting some single underlying capacity, but more likely with phonology employing mechanisms derived directly or indirectly from syntax.” I honestly don’t see what makes this highly likely, other than a prejudice about the primacy of syntax. In addition, Bickerton himself has argued that protolanguage (like pidgins) lacked syntax; but pidgins and agrammatic aphasia certainly don’t lack phonology. Thus, the logic of Bickerton’s original position demands that phonology belongs to an earlier stratum of language than syntax, in concurrence with my position, and in conflict with the position he takes in this commentary.

Bickerton also complains that I do not address his evidence about the timing of the development of protolanguage and modern language. I do not deny his evidence or the interest of the issue. It’s just that you can’t do everything.

Zuidema & de Boer raise the important issue that every transition between phases is a challenge. Suppose one speaker has a mutation that allows her to construct fancier sentences. What good will it do her if no one else can perceive them advantageously? This is, of course, a problem, whether the evolution of language was in many phases, or all at once (as Chomsky often seems to think), or in two phases, à la **Bickerton**. In fact, this is a potential problem for any cognitive system that requires mutuality. *Foundations* recognized this problem and declined to address it, citing ignorance. One possibility, suggested by Chomsky (in his plenary address to the Linguistic Society of America, January 2004), is that some of the offspring of this single individual will share the relevant gene, and it is they who will reap the communicative advantage that leads to comparative reproductive success. I look forward to further discussion of this issue.

R6. Syntax

At the core of *Foundations* (Précis, sect. 4), is the argument that generative grammar since its inception has labored un-

der an incorrect assumption, never argued for: that the combinatorial complexity of language arises from the syntactic component alone. *Foundations* proposes instead (Précis, sects. 5–7) a parallel architecture, in which phonology, syntax, and semantics are equally generative, and argues that such an organization both reflects actual practice (outside syntactic theory) and also provides a more revealing account of language as a whole. I am gratified that so many of the commentaries appreciated the value of this argument.

On the other hand, there were dissenters. **Bickerton** describes himself as “an unashamed syntactocentrist,” though he offers no defense of syntactocentrism. He more or less accuses me of marginalizing and trivializing syntax by analyzing such a trivial sentence in Chapter 1 (whose point was to show the richness of linguistic structure in even the most trivial sentence); he overlooks all the discussion of syntactic detail in Chapters 5, 6, and 12. Of course, there is a lot more to syntax (and phonology and semantics) than I have presented in *Foundations*. Culicover and Jackendoff (forthcoming) addresses what might be left of syntax in the new framework; it is hardly marginal or trivial, though far less complex than the Chomskyan models of the last thirty years.

Freidin attempts to defend the standard Chomskyan models. He claims that my term “syntactocentric” mischaracterizes these models, in that the phonological and semantic content of the lexical items embedded in a syntactic tree provides a source for phonological and semantic combinatoriality (as described in the Précis, sects. 4 and 8). But he misses my point. Much of Chapters 5, 6, and 12 (and also the Précis, sects. 5 and 6, as well as a large proportion of my previously published work) demonstrates that the independent combinatorial structure of phonology and semantics simply cannot be derived from syntactic constituency, because the correspondence between the three components is imperfect in many interesting ways. Freidin does not address any of these phenomena, and as far as I know no one else in the recent Chomskyan tradition has addressed them either.

Freidin also offers the argument that the parallel architecture puts a huge burden on the interface/correspondence rules that establish the relations among the parallel structures, because each word requires an interface rule linking its phonological structure, its syntactic features, and its semantic structure. Then he says that since the linking of phonology with meaning is arbitrary, there cannot be any such rules. He misses the point of section 5.7 in *Foundations* (Précis, sect. 8): words *are* interface rules! They are not general and systematic, of course. However, Chapter 6 demonstrates a cline of phenomena ranging from the very specific to the very general, such that words are on one end and general rules of phrase structure are on the other. So it is no longer possible to make a sharp distinction between the truly exceptional and the truly general, as the Chomskyan theoretical technology has always done. Freidin ignores all the arguments for this view of the lexicon (e.g., idioms, constructions, regular morphology, role in processing); again, as far as I know, no one else in the Chomskyan tradition has addressed them either.

Gervain worries that *Foundations* provides no account of the standard syntactic phenomena usually treated as movement and constraints on movement. This is correct. *Foundations* acknowledges their existence (Chs. 1 and 5)

but does not discuss them in detail. As observed in the Précis, section 7, the parallel architecture still leaves open the possibility that the syntactic component involves movement rules with the standard definitions and the standard constraints. But it also allows for the possibility that there is no syntactic movement per se, and that passive, raising, wh-fronting, and so on are accounted for in a fashion akin to that of Lexical-Functional Grammar (Bresnan 1982) or Head-Driven Phrase Structure Grammar (Pollard & Sag 1987; 1994), where the constraints are on configurations rather than on movement. My inclinations as a whole are for the latter possibility, but *Foundations*, being long enough already, was not the place to argue the point. Culicover and Jackendoff (forthcoming) take up these matters in more detail.

Gervain also worries that in an effort to do away with syntactocentrism, I have substituted “lexicocentrism.” Perhaps, but to call something “X-centric” doesn’t make it bad (consider “heliocentric” in reference to Copernicus). Gervain observes that the parallel architecture’s claims about the lexicon become crucial when the model is extended to explain processing. I certainly agree, and Chapter 7 of *Foundations* is devoted to working out some of the implications. There is clearly much more to be done, but my impression is that psycholinguists are on the whole enthusiastic about this model because of the connections it makes with processing. (By contrast, a prominent psycholinguist once told me that he had not referred to Chomsky at all in his influential book on processing, because he could find no way to make contact with Chomsky’s recent work; and Chomsky and his close colleagues have likewise done little to make contact with processing concerns, so far as I know.)

Wiese, on the other hand, suggests that my slimming down of syntax does not go far enough, and that linear order should also be purged from syntactic structure. The consequence would be that a rule like “verb goes at the beginning of verb phrase” would be not a rule of syntax, but rather, a rule of the syntax-phonology interface. I have no objection to such a move in principle, and it would be interesting to see how it works out. This is precisely the sort of question that the parallel architecture encourages: the balance of power among components of the grammar. Many other such issues are addressed in *Foundations*, for instance, the balance of syntax and semantics in determining the overt argument structure of verbs (Ch. 5); the contribution of syntax in semantic coercion (Ch. 12); and the balance of syntax and phonology in determining intonation contours (Ch. 5). I hope I will be forgiven for drawing the line on what is included in the book.

Lavie, too, thinks I retained too much of traditional generative grammar. In his case, the issue is the notion of syntactic categories. I have some difficulty following his argument, but the key seems to be that in his ruleless model “all the computation takes place among exemplars and occurrences” with no reference to types; computations are based on “analogy.” However, it is not clear to me from the commentary alone how Lavie’s idea differs, for example, from the connectionist approaches attacked by Marcus (2001), and how it solves my “Problem of 2” (*Foundations*, Ch. 4). Lavie also does not address my direct argument against analogy in Chapter 3:

Nor can we figure out . . . rhymes by analogy, reasoning for example, “Well, *ling* sounds sort of like the word *link*, and *link* rhymes with *think*, so maybe *ling* rhymes with *think*.” The only

words for which such an analogical argument works are the words with which *ling* already rhymes – which is of course no help. (*Foundations*, p. 64)

Lavie notices that I mention Tomasello's work on acquisition (e.g., Tomasello & Merriman 1995), which points out that at an early stage, children learn new constructions one word at a time; therefore, he asks why I need rules and categories at all. However, Chapter 6 endorses Tomasello's position only at the outset of rule acquisition: There has to be a further stage where some similarities coalesce as regularities, producing a lexical item with a variable that functions in a rule-like fashion. Lavie cites Maurice Gross (1975) to the effect that no two verbs have exactly the same distributional behavior (which wouldn't surprise me); he concludes from this that it is a mistake to have a category such as verb. However, all verbs go in the same position in the sentence, and with very few exceptions they all have full inflectional paradigms (regular or irregular, as the case may be). These absolute uniformities among verbs are a function of their syntactic category; the differences in distribution follow from differences in meaning, subcategorization, participation in idioms and collocations, and so forth. I've never heard of a language in which different verbs occur in different positions, say, one class of verbs that occurs at the beginning of the verb phrase (VP) and another class that occurs at the end – certainly a logical possibility. This suggests that some of the distributional behavior of verbs is a function of their being *verbs*, and that Universal Grammar specifies that within a given language all verbs have their position specified in the same way. I don't see how this can be done without categories, but perhaps Lavie has a more sophisticated notion of analogy than I am imagining.

R7. Semantics

Finally, I turn to the issue that *Foundations* characterizes as the "holy grail" of linguistics and cognitive science: the pursuit of a theory of meaning. *Foundations* contends (Précis, sect. 9.2) that just as it is necessary to study syntax and phonology as mental structure, it is necessary to abandon the realist conception of meaning bequeathed to us by standard philosophical approaches and to situate meaning in the mind of the meaner. This aspect of *Foundations* drew the most outcries from commentators.

Adams, Higginbotham, Jerzykiewicz & Scott, MacAogáin, and ter Meulen all offer essentially the same argument: A proper theory of meaning cannot merely account for the relation between linguistic expressions and the language user's mind, it must also account for the relation between linguistic expressions and the world. They cite Frege, Searle's Chinese Room, the universality of truths of arithmetic, and the need for an objective notion of logical validity as evidence for this position. Much of Chapters 9 and 10 of *Foundations* is devoted to answering this argument. However, I explicitly say (pp. 279–80):

My approach might in a way be seen as hedging one's bets. I am hoping that we can arrive at a naturalized view of meaning without invoking intentionality. On the other hand . . . conceptual structure might indeed need to be intentional in some sense. Whichever is the case, we still have to work out the details of the combinatorial system constituting semantic/conceptual structure/LoT, as well as its interfaces with language, inference, perception, and action – which is what I take as the task of conceptualist semantics.

(A similar statement appears in the Précis, sect. 9.2.) For example, whether or not there are universal Platonic truths to which arithmetic statements such as "2+2 = 4" refer, we still have to account for how human beings *conceptualize* number such that they grasp these truths as universal and timeless. This is something the Anglo-American philosophical tradition pretty much ignores but cognitive neuroscience cannot.

In addition, Chapter 10 (Précis, sect. 9.2) argues that our sense of contact with the external world is not just a problem for semantics, it is a problem for perception as well. Everyone studying visual perception knows that our visual sense of the "world out there" is the result of fantastically complex brain processing going on between the retina and central cognition. I am unaware of literature in the philosophy of language that addresses this problem. My own position is that linguistic reference to objects in the world simply piggybacks on the output of the perceptual systems: "the world" as given to us by the perceptual systems is what we refer to. This does indeed leave the issue of what it is to refer to a number (as *Foundations* acknowledges). However, I take it that the first-order problem, the one that language had to deal with as it evolved in our ancestors, is reference to people and trees and things to eat and things to do, and this falls in naturally with the problem of perception.

Jerzykiewicz & Scott think I have identified truth with "community consensus." Far from it. As Chapter 10 of *Foundations* says, community consensus is one way we have of checking our judgments of truth, especially when we have no personal experience with the matters at hand. But if that were *all* there was, I couldn't logically claim that the community consensus on reference is false, could I? (The case in *Foundations* was the Emperor's New Clothes.) In the general case, one's judgments of truth require a delicate balancing of evidential sources from personal experience, memory, inference, and community opinion. And, given that "absolute truth" (about most matters, anyway) is in principle inaccessible to us, our *judgments* of truth are all we have to work with in dealing with life.

Higginbotham says that if all reference is via concepts, as I claim, then the theory can't distinguish Sherlock Holmes (fictional) from Derek Jeter (nonfictional) and from the unicorn in my dream. However, Chapter 10 specifically *does* offer a distinction in terms of "valuation features," which Higginbotham evidently overlooks. This account may or may not be correct, but it is there.

Higginbotham also offers what he calls a "demystification" of the reference of "the distance between New York and Boston": If the distance is 204 miles, the actual referential expression is "the distance between New York and Boston in miles," which refers unproblematically to the number 204. This approach, which he attributes to Carnap (who was of course innocent of modern linguistics), is simply not feasible. First of all, it leaves unexplained what "mile" refers to, not an easy task without a referential category of distances. Second, it does not address my example (given in Ch. 10 and cited in my works since 1983): *The fish that got away was this* [demonstrating] *long*. This example invokes no numbers. Rather, the distance referred to by *this long* (a spatial extent, not a number) is picked up from observation of the position of the speaker's hands. Third, as *Foundations* (p. 301) observes, the truth-conditions depend on how the distance between New York and Boston is

measured: “From center to center, from nearest border to nearest border, along some particular highway, on a straight line through the earth’s crust? Much depends on one’s purpose.”

Dennett, in an attempt to head off critics of my conceptualist view of reference, offers the suggestion that we consider it an account of “some other guy’s language (and mind).” He suggests that such an approach is necessary for a proper theory of linguistics, but for everyday purposes “we can continue using the traditional semantical talk about the word-world relation.” He compares this stance-switching to the practice in evolutionary psychology, where everyone talks about “the gene for such-and-such” (everyday talk), knowing full well that there is really a very intricate biochemical story to be told (technical talk). I am reminded also of Dennett’s own rhetoric about consciousness: He constantly insists that the everyday talk of “you, perceiving the world there in your brain” (or the “Cartesian theater”) has to be carefully expunged from technical talk about how the brain produces awareness of the world. In fact, he and I indeed face the same problem: People accuse him of claiming we’re not conscious, and accuse me of believing language doesn’t refer to the world – in both cases because they are unwilling to engage in the proper technical talk necessary to approach the problem scientifically.

Fortunately, there are some commentators who find the conceptualist approach appealing. **Molina** and **Dominey** contrast the parallel architecture’s account of meaning with one based on the syntactocentric framework for language. Molina observes, correctly I think, that the Chomskyan theory of LF does not provide an adequate vehicle for thought in general. Dominey suggests that I have gone “conceptuocentric” (there’s that X-centric again!): In the parallel architecture it is natural to suppose that the hierarchical complexity of syntax is but a pale reflection of that in meaning, and it exists only insofar as it helps express thought more precisely. Moreover, Dominey says, access to the compositionality of meaning provides a scaffolding for the child’s discovery of syntactic structure. I concur. It is nice to hear that he is developing experimental tests of these propositions.

Velichkovsky et al., on the other hand, think that I have not gone far enough. For them, the whole theory of language should be based on the pragmatic, executive, or metacognitive aspects of communication. They predict that the notion of Universal Grammar will come to be replaced by Universal Pragmatics of cooperative action. I am fully in agreement with them in thinking that pragmatic and communicative aspects of language should play a role in the account of the system as a whole. However, a full theory of language still must account for all the details of syntax, phonology, morphology, and word and phrasal meaning – as well as how they are all learned. If there are pragmatic and communicative aspects that interact with these, well, we need more cognitive structures and more interfaces. Among such aspects briefly discussed in Chapter 12 of *Foundations* (and sect. 6 of the *Précis*) are information structure (topic vs. focus, old vs. new information) and the “reference transfer” constructions in sentences like *Plato is on the top shelf of the bookcase*, in which a speaker uses a person’s name to refer to a book by that person. Again, this is not an area in which I’ve done much research, but that doesn’t mean I believe it’s unimportant.

Finally, other commentators propose further articulation

of conceptual structure. **Andor** advocates closer attention to frame-based semantics, which I allude to at the end of Chapter 11. True, there is much more to do. I hope others will join in the work. **Justo et al.** chew over the idea that conceptual structure can be subdivided into a number of different and more restricted components. If spatial understanding is regarded as outside conceptual structure, why is social understanding regarded as inside conceptual structure? What do such decisions imply about processing? These are exactly the right questions, and they can only be settled if researchers accept the overall framework so we can work on its details. Such questions cannot be settled by the logicians’ insistence on realism and/or intentionality.

Ter Meulen takes issue with my claim that there is no strict cut between logical and nonlogical properties, insisting that

If one desires to model inference as a matter of form, irrespective of content, an inference is logically valid just in case the premises entail the conclusion in every possible model, for its validity must depend only on its logical constants. (para. 4)

Similarly, **Justo et al.** advocate a “minimal view” of conceptual structure, in which it “is able to represent all distinct meanings, but is not able to carry out computations other than the logical ones.” My own take on the logical/nonlogical distinction (Ch. 9) is that certainly it must be accounted for, but not by a difference in level, say, between truly semantic structure and some sort of pragmatic structure. For instance, I am interested in the fact that (4a) leads to the inference (4b), but (5a) leaves both (5b) and (5c) as possible continuations.

- (4) a. Bill forced Harry to leave.
- b. Harry left.
- (5) a. Bill pressured Harry to leave.
- b. . . . and Harry left.
- c. . . . but Harry didn’t leave

Now it turns out, at least according to the “force-dynamic” theory of the semantics of causation (Jackendoff 1990; Talmy 1988), that *force* and *pressure* have almost the same semantic content: they both mean roughly “apply force to an individual toward the end of that individual performing some action.” They differ only in that *force* indicates that the application of force is successful, whereas *pressure* leaves the issue of success open, while biasing toward a successful result (that is why (5b) uses *and* and (5c) uses *but*). The result is that *force* has a logical property whereas *pressure* has a merely heuristic property. Unlike logicians in the truth-conditional tradition, I wish to capture both these properties, in a way that brings out their similarity. More generally, I think that an account of logical inference is only one aspect of the theory of meaning, an important one to be sure, but not important enough as to demand precedence over everything else.

R8. Final remarks

Overall, these commentaries illustrate, more vividly than I could have done myself, the huge rifts among the many communities engaged in the study of language, as well as the attitudes that keep these communities isolated from each other. I wrote *Foundations* in an effort to throw some ropes across the rifts, to provide some slender basis for communication. However, it is clearly impossible for one individual alone to bridge all the gaps, fill in all the missing

parts, and make all the connections. I hope the present discussion, along with *Foundations* itself, can serve as an invitation to others to take part in the enterprise.

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Letters “a” and “r” appearing before authors’ initials refer to target article and response, respectively.

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