

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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