

*Reply***Meaning is first*: A reply to the commentaries**

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I wish to sincerely thank the commentators for their willingness to devote time and effort to responding to the Forum Lead article. All the contributors compliment me by their thoughtfulness in this volume. Concerning the “attack mode” of some of the remarks, I was warned ahead of time by Joseph E. Bogen, who read an earlier draft, against “leading with the chin” in selecting the title (“Is your syntactic component really necessary?”). In other words, I asked for it. I chose the title intentionally to dramatise the challenge to well-established views—the challenge afforded by accidents of nature.

Over several years, exposure to the paper by Lorber (1983) (“Is your brain really necessary?”), the commentary with the same title by Lewin (1980), and subsequent lectures and presentations of brain images of Lorber’s hydrocephalic patients, stimulated chagrin in many neuroscientists over standard brain locationist theories. These articles were much discussed in the Department of Neurology at the University of Southern California, where I was working at the time. Similarly, studying a left-hemispherectomised adult (BL), who had superior verbal and nonverbal intelligence, powerfully challenged my assumptions as a researcher in hemispheric specialisation (Bogen et al., 1998). In both these cases, I found that the most basic and implicit assumptions took the biggest hit.

In the case of hydrocephalic adults with normal cognitive function, I was confronted by dissonance with standard beliefs about cortical and subcortical “real estate” expected to correlate with intact behaviours. How could a thin “rind” of brain produce the same behaviours as brain matter spatially arranged in the familiar shapes of lobes, cortical convolutions, and subcortical nuclei? While testing BL, the highly intelligent, verbally gifted man with only one cerebral hemisphere (the right one), even subtler assumptions took a fall: that the hemispheres naturally (innately?) perform different tasks, that both are needed for normal function, and, again, that cerebral spatial extent is a major consideration in explaining behavioural function. Of course, these two neurological conditions are blatant and extreme; other well-known observations also leave us perplexed: autism, where brain anomaly is unseen, slight, or inconsistent across those afflicted; Tourette’s syndrome, where no clear neuropathology has been demonstrated; stories of dramatic recovery following brain damage; examples are numerous. Again and again, we are led to realise that we know only a little.

Obviously, no one would respond to Lorber’s essay by defending the brain, enumerating its qualities, its parts, nuclei, and excellent continuity with the spinal cord. It would not occur to any serious writer to devote several pages to arguing the necessity, the

* Apologies to e.e. cummings’ poem “since feeling is first.”

probity, the central importance to human behaviour of the brain. No respondent to the query “Is your brain really necessary” would authoritatively shriek “yes!!!” or condescendingly deride the author of an article with that title. Nor did I intend the title, any more than Lorber did, to be taken literally (although both of us undoubtedly assumed it would be jarring). To do so is to miss the joke, miss the point, and to lose an opportunity to experience the renewing process of basic assumptions challenged. The point is that, of course, syntax is important: then why, WHY is its loss so much less devastating than the loss of semantic ability? And further, as Michel Kac pointed out (1999), if we don’t need it, why do we have it?

Hence it was surprising to see how resoundingly the early portion of the response of Shapiro and Friedmann, beginning with a “simple demonstration of the necessity of syntax”, misses both the joke (less unexpected) and the point (more unfortunate). While it would be poor form on my part to cower back with cries that “I was just kidding”, surely the concept was abundantly clear that the question is one of syntactic centrality in human language, not whether syntax, or formal structures, exist at all, or whether they are important, useful, even crucial to a description of language for any purpose. Shapiro and Friedmann’s protesting so strongly, again, reflects the troubling tenacity and rigidity of the standard view of syntax. The review of data on yes/no questions is nice; curiously, the language sample from the patient RN has little in the way of classic Broca-like features, and there are probably many other explanations for his inability to formulate a yes/no question. This example, and those cited of Grodzinsky, depend on a currently popular model of syntactic structure. Many of us who have been around linguistic science for a long time have experienced the continuous turnover of syntactic models, and have less confidence that something of substance will have been discovered in these examples. The apparent inability to appreciate the value of even a suggestion of a proposed shift in emphasis (from formal syntax to meaning), reflected in these commentators’ remarks, underscores the very point I was making: the strong cathexis to generative syntax (as the centre of language ability) in the personal outlook of practitioners as well as in the public consciousness. This phenomenon—the synergy between the individual and social aspects of science—is explored in great detail by Fleck (1979). One quote (p. 43) is particularly pertinent here:

The social character inherent in the very nature of scientific activity is not without its substantive consequences. Words which formerly were simple terms become slogans; sentences which once were single statements become calls to battle. This completely alters their socio-cognitive value. They no longer influence the mind through their logical meaning—indeed, they often act against it—but rather they acquire a magical power and exert a mental influence simply by being used.

For Fleck, this is a natural process in scientific activity, because “thinking is a supremely social activity” (1979, p. 98).

The editor’s solicitation of responses both by C. Thompson and L. Shapiro hardly surveys the field, as Thompson and Shapiro have published together for years, and have essentially the same points of view. Nonetheless, C. Thompson’s paper (with R. Bastiaanse and S. Fix) seems at least to get the joke, if their amusing title (“Yes”) serves as a clue. Their review of the considerable contributions of semantic studies was heartening. The allegation that there are “a number of important theories . . . that have emphasised the structure of meaning” is entrancing but not convincing. We know about the work in semantics; the question pertains to its relative status in the minds and hearts

of linguists and common folk alike. A favourite philosophy professor used frequently to say “never argue facts”: Cindy and I would have to perform some serious (funded) written surveys to settle the question of what most people take to be more important in linguistic descriptions, syntax or semantics. But as I am already way, way out on a limb, I hazard a prediction that given such targets as students in introductory linguistics classes and the educated layperson, the “importance” of syntactic models in human language would win out over any or all of the semantic theories mentioned, however we would fashion the survey questions. Further, I predict that a compilation of courses taught in linguistics departments would weigh in more heavily for syntax than semantics classes. In the end, of course, I fully agree with these commentators that “both syntax and semantics (as well as other language components, such as phonology) are important in order to fully understand language and language disorders”; who wouldn’t? I would be happier if there were evidence that this perspective had found its way into general understanding. Instead, I believe we are still in the “stage of development” when “habits and standards of thoughts [are] felt to be the natural and only possible ones” (Fleck, 1979, p. 107), namely, the primacy of syntax.

Thompson et al. accuse me of criticising “syntactic theories for their lack of appreciation of meaning”; they then cite examples to show that syntacticians have in fact cared about meaning all the way along. They and other contributors to this volume complain that no component of language should be considered more important than another, or that comparing degree of handicapping when any one component is impaired is not meaningful or useful. These remarks are like numerous others in the commentaries. The notion I was trying to express is subtle, and/or I didn’t depict it very well. The notion relates to what part of language is the most essential, in the sense of representing the essence (primary characteristic) of the object of examination: is it formal syntactic relationships, or is it meaning relationships? A similar lack of meeting of the minds occurred when I read Thompson et al.’s remark (difficult to unpack, as a triple-negative) “no one proposes that lack of syntax renders humans incapable of communicating”. I didn’t say that anyone DID propose that. Instead, I fully agree with that point, and tried to express it myself, and further propose, on the other hand, that lack of *meaning* (or semantic ability) DOES render people incapable of communicating. This argument leads to the main point, that MEANING is “more central” to communication than syntax.

The Thompson, Bastiaanse, & Fix refutation of the subcortical hypothesis for the representation of syntax makes some good points. I am aware that naming disturbances have been observed in basal ganglia disease, including in subgroups of Parkinson patients (e.g., Beatty & Monson, 1989). (Observations from thalamic disease do not pertain here; that structure likely has very different functions from anything I’ve tried to discuss in the Lead article.) Subcortical naming disturbances do not necessarily dispute the important role of the basal ganglia in syntactic production, and possibly also syntactic comprehension, although I do take their point in a positive spirit. Thompson et al. state that I base this suggestion (of a role of the basal ganglia in syntactic formulation) “largely on the observation that some patients with subcortical lesions evince grammatical impairments”, but the notion is also significantly based on observations in production in patients with aphasia and dementia, who have intact basal ganglia, and produce intact syntactic structures. When Code (1982) and Blanken and Marini (1997) surveyed recurrent utterances in severely aphasic patients, they found that sentence-initial pronoun + verb was preserved. It is unconvincing, given the population, to attribute these intact productions, in the context of little other speech, to intact left hemisphere structures. Such attribution will not work at all in the case of the EC, who, although

profoundly aphasic following removal of his left hemisphere as an adult, produced similar pronoun + verb structures (Smith & Burklund, 1966; Smith & Sugar, 1975; Van Lancker & Cummings, 1999). An interaction of the right hemisphere with the motor planning structures of the basal ganglia provides the more satisfying account. Rather than being dismissed, these extreme examples should be a goad to considering a new perspective. When combined with observations that Alzheimer patients play music (trombone and piano) and (contract) bridge with severely deficient cognitive (or declarative) knowledge in those areas (Beatty et al., 1994, 1988), we are “permitted” to think in a new way about complex motor skills, even those with a very large logic of combinatorial possibilities (such as bridge and musical forms); we can entertain the notion of these skills being represented by the basal ganglia.

This perspective was clearly grasped and promoted by Kempler’s comments. Kempler’s commentary very much takes on the spirit of the Lead article’s inquiry, undoubtedly due in part to his exposure to disordered language, in a course similar to mine. We both started out fully imbued with the formalist view of language structure, by training and by Zeitgeist; and then empirical pressures led in various other directions. In explaining the differential picture of syntax and semantics in patients with Alzheimer’s disease, Kempler, Curtiss, and Jackson (1987) promoted the idea some time ago that there is an “automaticity” to syntactic production which accounts for its preservation in advanced stages of dementia. While the relation of frontal and subcortical structures is well known, cortical areas, including those encroaching into frontal lobes, are selectively affected in progressive Alzheimer’s disease; motor functions remain normal long after cognitive function has declined to where full-time care is necessary. At this time, speech formulas as well as scaffolded syntactic structures (devoid of intelligible content) are observed in speech production. How this fact relates to comprehension of syntax is not known. The Thompson et al. commentary from comprehension studies (ERP and PET) does not provide strong counterevidence to the notion of subcortical areas as the source of syntax as motor skill. Comprehension via the tasks used in these methods involves many levels and functions.

Kempler has described the notion of routinisation in syntactic output clearly. This routinisation, as he goes on to explain, does not provide the complete picture. Kempler has pointed to the possible model of speech production as one of cycling between two processes: habitual motor programs integrated with assemblage of novel strings. In this regard, it is likely that brain structures differ considerably for comprehension versus production processes. Whereas Thompson et al. reject the notion that aphasia data lead to a theoretically critical distinction between comprehension and production, I maintain that recognising this distinction will lead to better models of language behaviour. The gradient between rare and frequent structures, highlighted by Kempler, would be expected to differ between these two modalities. Discrepant behaviours (between production and comprehension) are seen in child language acquisition, aphasia, dementia, second language learning, and autism. Attempts to find an underlying competence in any of these domains have not been convincing. People can go on trying to find this unifying level, but surely some scepticism, at least, is appropriate.

Beretta was inadvertently pleasing in selecting a favourite medieval saying, *nihil ex nihilo*, as his theme, but he gave an uncommon translation in his title; the usual English version, as popularised in Shakespeare’s *King Lear*, is “Nothing comes of nothing”. It was all the more surprising that Beretta used this saying as the theme of his reply, given that he agreed with so many of the points made in the Lead article. In agreeing with many of the points but finding them noncontributory in any forum, he illustrated another (more

implicit) idea I was trying to express: the valence towards an idea (the value given it) is a potent determiner of what is believed, studied, and pursued by scholars. Beretta's dismissal of some of the ideas I was highlighting is a case in point. He sees them as obvious, or "trivially true" as the philosophers say, while I see them as compellingly important and in need of more serious consideration. Beretta and I, in many ways, agree but disagree: we agree on certain facts, but disagree on their value. With regard to his final sentence ("the contribution of her forum paper to any understanding regarding the nature of language is precisely nil"), which is strangely inconsistent with the rest of his mostly agreeing and amplifying essay, I refer him to the whimsical analysis of "nothing" by Barthelme (1973), in which it becomes clear that *nothing* is very, very difficult—probably impossible—to define. Interestingly, I am in agreement with most of his substantive points. His major snide comment—that most of my linguistic comments come from the "videos, popularisations, and introductory textbooks"—was quite unworthy of the rest of his generally thoughtful discussion. Obviously, in my essay, the popular examples were selected in order to illustrate the role of syntax hegemony in general, popular usage, while the numerous scholarly linguistic references were brought in to illustrate other points. I must congratulate Beretta for being one of the few commentators to touch on the most radical of the notions in the Lead article, that involving repeated use, and therefore frequency, of syntactic frames as a meaningful principle in explaining human language competence and performance. Unfortunately, he is unable to take the fascinating next steps, for example, to bring in the forbidden notion "analogy" as a potent force in language production and comprehension. If this concept were a usable one in the current collective thought (it isn't), it could also help explain Kolk's (see later) results on priming of passive sentences. Beretta is apparently unable to consider a psycholinguistic model of human language processing that features a constant shifting between well-rehearsed to novel formal structures, allowing for time to formulate during times of routinised behaviour; or to entertain a neurolinguistic model that views aspects of language processing as procedural skill drawing on basal ganglia function. But there is much compelling evidence for this model. Although I appreciated the detailed and reflective effort of Beretta, as I did all of the commentators, I suspect that his stance of loathing bewilderment towards some of the ideas expressed in the Lead article contributed to the dissonance in his essay, which interleaved revulsion with explicit agreement. Again, his very tone illustrates (rather unpleasantly) the point that contempt prior to investigation dominates much of linguistic scientific work, or, in Fleck's (1979) words, "Once a structurally complete and closed system of opinions consisting of many details and relations has been formed, it offers constant resistance to anything that contradicts it" (p. 27).

Kolk's title "Syntactic impairment is the bottleneck to communication in nonfluent aphasia" should have been cast in the question form. His work strongly qualifies the role of syntactic impairment in aphasic speech. It is more accurate to say that *he* "underplays" the role of the syntactic component (given the results of his studies) than to say this of me; I merely say that syntactic impairment (granting that it exists) does not impair communicative function as much as semantic impairment does. Kolk states that "to explain the behaviour of patients with Broca's aphasia . . . it was necessary to consider lexical, pragmatic and general cognitive factors as well . . .". This kind of open inquiry is the most hopeful in advancing our understanding of aphasic speech. Indeed, his work, partly because of its qualities of both openmindedness and attention to methodological detail, reflects and embodies many of the problems touched on in my essay. The "slow-down" allegedly accountable for agrammatic output is also used to

explain word-formation problems (Kolk, 1995). This is unassailable; slower processing is the hallmark of brain damage. Establishing a baseline of psychomotor retardation across linguistic functions in aphasic patients has limited value in addressing specific neurolinguistic questions. The notion of decay of the “syntactic tree” due to slowness in processing is unsatisfying because of the numerous assumptions that must be taken on. For some time now, trees have had a dubious status in formal models of grammar, and as changing, insubstantial chimera over the years, any particular type of tree has not lasted as useful in processing models. In Hartsuiker and Kolk (1998), it is not surprising that presenting the format of a passive structure results in more productions of passive sentences in aphasic speakers. With formal models no longer supporting a rule-governed relationship between active and passive sentence forms, the relevance of this observation to any question about grammatic (i.e., syntactic) competence in aphasia is not compelling. It is equally likely that nonfluent aphasic speakers rely on the more frequent and common forms, using the more highly habituated structures (active sentences) until given external templates (e.g., an analogue) for producing the less habituated (passives). The interesting finding by Kolk and his colleagues that both normal and aphasic persons switch from elliptical to nonelliptical speech (Kolk, this issue) would seem to look more successfully to a sociolinguistic explanation, involving register or contextual style. In the normal speaker, the learned or elaborated style is more “effortful” than the conversational; the formal more “demanding” than the casual or reduced. With the compromised capacity of the aphasic speaker, as Kolk’s work has tried to characterise, these differences are predictable. From this point, it is difficult to say anything formal or structural about either syntax or semantics; the point is about style, and it draws on assumptions (which may or may not be cogent) about effortfulness. In Kolk’s commentary, the appearance of locative prepositions in picture description by aphasic speakers was meant to highlight the role of pragmatic factors; it may also reflect a relatively intact semantic system. There’s meaning in those locative prepositions, and the assignment of prepositions to the category of grammar (or syntax) is arbitrary and controversial. My detailed comments on Kolk’s work, which I admire very much, relate most vividly to the original Lead article point that linguistic models since 1957 have not been revealing in efforts to understand aphasia. Useful, as seen also in the Opler and Gjerlow (1999) example given later, has been the traditional and evolving linguistic nomenclature describing units and levels in language.

An especially thoughtful consideration of the ideas expressed in the Lead article was presented by Susan Edwards. She provides numerous counterexamples and counterarguments, well presented and enumerated. I very much appreciated her array of fact and interpretation. She rightly complained about the difficulty in grasping consistent meanings of the terms “grammar” and “syntax”. Her exposition of the various usages of the word “grammar” was helpful and illustrated the range of uses of the term. I agree, and provided a definition in the beginning of my original article because of the ambiguity usually present in writings on the topic. In fact, it is in part to this ambiguity (grammar=syntax; contrasted with grammar=a whole model of language) that the ongoing reign of syntax can be attributed. The two original publications by Chomsky (1957, 1965) focused unequivocally on syntax (look at the titles). Semantic processes were different and separate, and were meant to be appended. The entire model of language could then be called the grammar, and it would include semantics, but the central system, the one that provided for the infinite productivity of human language, was syntax. However, notion of grammatical-type rules has broadened immensely since then, even to underlie and account for the workings of the entire mind (Pinker, 1997).

Edwards disagrees that generative grammar is dominant in linguistics courses, citing the UK. I would not expect generative grammar to have a dominant position in the British Isles, given their very different linguistic traditions. Nonetheless, I have heard the opposite from other British citizens. Again, a survey study would help resolve this question. A number of the commentators disputed the notion that the Chomskyan syntactic model is the reigning one. In the Lead article, I gave examples from introductory linguistics texts. Here is another example: in the book *Language and the Brain*, in Chapter 11 (of 12 chapters), the authors (Oblor & Gjerlow, 1999), leaders in the field, visit the question of "Language organization" as revealed in neurolinguistic studies: "Since Noam Chomsky's 1957 book *Syntactic Structures*, a major goal for modern linguistics has been to construct a model for the grammar of an 'ideal' native speaker of a language." First, they dismiss the notion of the model as divorced from actual speech production and comprehension, even though this notion is frequently reiterated in the generative tradition. Then they review neurolinguistic evidence mainly in terms of traditional linguistic entities: phonetics, phonology, morphology, syntax, semantics, pragmatics, prosody, and written language, with only a few comments relating to anything deriving from the Chomskyan tradition, any of which (e.g., sentence ambiguity) could have a place in other models. These authors could as well have invoked the descriptive linguistics of the Bloomfieldian school, generative semantics, or any one of several other models or traditions, in pursuing questions about how neurolinguistic studies provide material to test, verify, or support the linguistic models. Note that they do not mention Montague grammars, or functionalism, or any of the other schools purported by the commentators in this issue to be prominent on the landscape. I do not doubt that these others are on the landscape, but they have not taken a meaningful place in the larger exoteric circle (Fleck, 1979) of public opinion about linguistics.

As another example of the prevailing social thought, consider the video production, *The Human Language Series* (1995), targeting introductory college classes. The purpose was to provide an overview of what was known about human language in the twentieth century. The formal generative model with syntactic rules as the major feature underlies this presentation exclusively. Chomsky appears frequently, seated in various venues, answering questions that come up throughout the investigation of language. In the first programme of the video series, one spokesperson explains that language is a "deceptively simple system. The *genius of the system is that with a small number of words and this system of grammar,*" the speaker can make up an infinite number of sentences. There is no ambiguity about the meaning of the word *grammar* here: it means *syntax*, and there is no doubt about the relegation of "words" to the periphery. Child language acquisition is depicted entirely as a process dependent on the interaction of modest input with an unfolding innate competence made up of rules. No mention of other processes is made, as, for example, that depicted in Gleason and Weintraub (1976), whereby parents are observed teaching children how to use specific speech formulas in appropriate contexts. I give this pragmatics-based study as one example: the rather vast literature emerging at the time on meaning-based or function-based language learning is not represented. The video series is an outstanding achievement—funny, stimulating, interesting, brilliant with charming commentaries and with examples of the true wonder of human language. But again, despite the various other efforts at understanding language mentioned by the commentators in this issue, we are surrounded by signs that the official flagbearer of modern linguistics is Chomsky and his many followers in the school of generative grammar. The Oblor and Gjerlow (1999) example shows that at the level of the introductory text in linguistic science, the invocation will be to the standard model.

These examples illustrate a key notion in history of science: a prevailing idea depends on “parallel social forces, which create a special shared mood and, to an ever-increasing extent, impart solidity and conformity of style to these thought structures” (Fleck, 1979, p. 106). It is also true that the linguistic sciences may be becoming considerably heterogeneous, and that serious and productive work in numerous other formats is well under way; this suggests a time of “change in thought style, that is, change in readiness for directed perception” offering “new possibilities for discovery” and the creation of “new facts” (Fleck, 1979, p. 110). As this volume shows, some people are ready, and some are not.

Edwards then reviews some recent lexical-semantic scholarship, highlighting the difficulties in modelling language in this way. This point merely underscores the unfortunate attraction to formal syntax because it is tidier to study and analyse. The same criticism has been directed to the functionalist school (Darnell et al., 1998): the array of semantic and pragmatic phenomena it attempts to describe leads to models viewed as implicit and pre-systematic (Anderson, 1998). Rather than disputing my main tenet, Edwards’ comments serve to support it: while other models of language have spun off into fragmentation, generative (syntactic) grammar has remained conceptually centralised. Edwards wonders whether I am talking about Chomsky as a charismatic leader or generative grammar *per se*. In the past century, who could imagine one without the other?

For much of her essay, Edwards takes my title literally, and insists that “meaning is important but it is not an alternative to syntax”. In the spirit of “we really do need a brain”, she gives many examples of how information about formal structure is needed in order to completely understand sentences (especially complex ones). The end of the essay gives examples of certain severely impaired children who, despite some ability to communicate using vocabulary, remain “communicatively impaired” because of limited syntactic skills. This does not refute the point that they would be MORE impaired communicatively if the limitation were the other way round: less lexicon and more syntactic function. However, Edwards rightly implies that direct comparisons between the two abilities are a bit absurd, and comparing putative impairments is even worse; this is a local example of incommensurability (of theories or models) (Feyerabend, 1975, p. 166, 206 ff). Nonetheless, she engages in the same practice, asserting that persons with Broca’s aphasia “do not convey more information than the fluent speaker”. Despite agreeing with Edwards that “the amount of meaning conveyed depends on a number of factors”, my point remains the same: given some admittedly idealised notion of equal severity (I’d accept the AQ measure on the *Western Aphasia Battery*), nonfluent aphasics *do* convey more information than the fluent aphasic speaker.

Edwards’ comments on speech formulas revealed a misunderstanding of this topic in several ways. In the original Lead article, the disability of the aphasic speaker who utilised only conventional expressions was a semantic one; his apparently fluent speech conveyed no useful, communicative meaning. In normal speakers, conventional expressions are prepackaged, and are not newly created; of course they can be modified, but then the other language skill—the one that produces novel sentences—is being applied to the one that stores and processes holistic, memorised utterances. In the case of normal persons, these utterances do convey meaning of a special kind: context-bound, conventional meanings. The importance of these utterances has been downplayed in modern linguistic research because of its emphasis on newly created sentences. The percentage of prepackaged utterances in everyday speech is not known. The percentage of conventionalised utterances in a well known screenplay (*Some Like it Hot*) is 22% of total utterances, 17% of total words (Van Lancker & Rallon, in prep). Seeing these

utterances produced fluently, but devoid of meaning or abnormal in meaning content, in aphasic or demented persons, merely reinforces the fact that the utterances are routinised motor speech gestures.

Bates' commentary "Tailoring the Emperor's New Clothes" is helpful in explicating some of the points about linguistic models that I tried to make, only less successfully than she did. Bates concurs with a current linguistic fixation on syntax, but she disagrees that it has marginalised linguistics; instead "this fixation on syntax has kept linguistics at centre stage" and the corollary tenets of "innate syntactic universals" and "a bounded grammar organ" have furthered child language studies and neurolinguistics respectively. I suggest that we are both correct, in different realms. Most of us have experienced, anecdotally, resistance to the arcane and technical features of formal models of syntax expressed by people who we'd like to see have an interest: secondary school and second language teachers, speech pathologists, behavioural neurologists. Yet there is no doubt that the generative grammar movement has stimulated and excited the elite, avant garde intellectuals. Bates is astute in relating the hegemony of syntactic grammar to current metaphors serving "current social, economic and political purposes". Her remarks are consistent with a study of science by Fleck (1979, p. 38), who speaks of a thought style belonging to the social collective, undergoing social reinforcement in various ways:

The tenacity of systems of opinion shows us that we must regard these systems to some extent as units, as independent, style-permeated structures that do not function as mere aggregates but harmonically and holistically display certain characteristics of that style, characteristics which determine and condition every single function of cognition. The self-contained nature of the system ... also preserve[s] the harmony of illusions...

Bates brilliantly refers to the cachet of syntax over semantics, remarking that "the intelligent layperson understands enough about the word 'semantics' to suspect that meaning itself is a very broad property of the mind, the brain, and the genome". In a humorous paragraph about other beasts, Bates explains how it is likely more comfortable to think of syntax as the *sine qua non* of human language, rather than semantics, which it's been said that the animals also have. Her insights reflect portions of Feyerabend's (1975) characterisation of science: "allegiance to the new ideas will have to be brought about by irrational means such as propaganda, emotion, ad hoc hypotheses, and appeal to prejudices of all kinds" (p. 114) in part because "concepts have not only a logical content; they also have associations, they give rise to emotions, they are corrected with images" (p. 124).

Bates lists "three different claims" in the Lead article: (1) Neurolinguistics should include more semantics studies; (2) Grammar should incorporate more semantics; (3) Grammar is semantics. The first is not controversial, and the second is less so because of studies of neural networks; the third, she points out, is radical. She is also correct in saying that my article does not attempt to stand for one or the other approach, but is intended only to offer up the others as more viable approaches to a genuine picture of human language. Bates' description of current lexicalist positions presents a convincing argument for that model. My point was simpler and much more elementary: that meaning is central in human communication. A model that explains and describes lexical processes, and incorporates grammatical relationships into those processes as dependent on them, would be the most amenable to the central thesis. I am especially grateful for her insight into the ideas of the Lead article and her well-known skill at clarifying them. Her voice has been a strong one in critically appraising current notions and providing a highly

sophisticated integration of data and theory in advancing new models of speech production and comprehension. Bates' work is a prime example of the optimal strategies in science: adoption of a pluralistic methodology, and willingness to compare ideas with other ideas (Feyerabend, 1975, p. 21).

The ideas in this Forum reinforce Feyerabend's statement that "knowledge . . . is not a gradual approach to the truth. It is rather an ever increasing ocean of mutually incompatible alternatives" (1975, p. 21). What is represented in our efforts here is an example of science as a "complex and heterogeneous historical process which contains vague and incoherent anticipations of future ideologies side by side with highly sophisticated systems and ancient and petrified forms of thought" (p. 106). It is comforting to see linguistics performing its role in history. This Forum serves a point made in *Farewell to Reason*: "collaboration does not need a shared ideology" (Feyerabend, 1987, p. 16).

In closing these final comments, I wish to utilise historical cosmic models as metaphor. It is well known that the local universe can fairly well be described with the earth at the centre, but only with the artifactual complexities of apparent motion (motions around equants) and other mathematical gyrations. Put the sun at the centre, and the world falls more readily into place. But accomplishing this revised scenario involved massive, wrenching shifts in assumptions, established "fact," emotions, perspective, and social context. Our work will be easier in some ways because the domain is smaller, but more difficult in others, because the subject is mostly phenomenological. I believe it will eventually be seen (again) that the essence, original substance, evolutionary source, soul, and specialness of human language resides in meaning, from which (and around which) structural relations emerge and take form. This is the job of linguists to describe and explain, and to miss this point, or to avoid it, will retard progress in the science of language. *The genius of the system is meaning.*

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