REVIEW

D. Terence Langendoen and Paul M. Postal, *The Vastness of Natural Languages*, Basil Blackwell, Oxford, England 1984, ix + 189pp.

A long standing and fundamental assumption of the generative grammar approach to the study of natural language is that each language consists of a denumerably infinite set of sentences, where each sentence is finite in length. This assumption can be seen to follow from the general view that generative grammars are constructive systems which recursively enumerate sets of sentences in a Turing machine-like manner. In their volume *The Vastness of Natural Language* (*VNL*) Langendoen and Postal challenge this basic assumption, arguing that natural languages in fact contain infinitely long sentences like (1) and further that there is no bound, whether finite or transfinite, on the length of sentences in natural language.

(1) Babar is happy, and I know that Babar is happy, and I know that I know that Babar is happy, . . .

The authors conclude from this that natural languages are not sets in the strict sense but are instead proper classes (or in L&P's terms, megacollections) with the size of the collection of all sets,[1] and therefore the only type of system that can adequately characterize such objects is a nonconstructive grammar, i.e., one which does not enumerate the members of the class but rather intensionally defines the conditions which all members of the class must meet, in a fashion parallel to that used in standard set abstraction. In the process of reaching this conclusion, which is sure to strike many linguists as rather startling, L&P argue at length against a wide array of potential responses and counterarguments to their line of reasoning. The reader is left with the sense that no stone has been left unturned and that the authors' results are therefore inescapable. On closer inspection, however, it turns out that several of L&P's arguments are based on premises which are at best open to considerable debate, despite their efforts to convince the reader of the contrary.

The volume, an expanded and revised version of an article manuscript which L&P were circulating a few years ago, is organized along the following lines. Since much of L&P's argument rests on the parallels

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between collections of natural language sentences and arbitrary collections as well as on the classical set theoretic paradoxes, the authors devote the first chapter to reviewing some of the basic results of set theory and to pointing out their relevance to the study of natural languages. In the brief second chapter, L&P present the standard generative assumptions about the size of natural languages and chide various writers for the invalid logic that they seem frequently to have used in discussing these assumptions. In addition, the authors lay the groundwork for the next two chapters by emphasizing that none of the following claims which make up the standard assumption have been adequately argued for in the literature: (a) the collections making up natural languages are sets in the strict sense, (b) the sets are countably (as opposed to uncountably) infinite, and (c) each sentence is a finite string.

Chapters 3 and 4 form the heart of L&P's arguments against the standard assumption about sentence length and language size. In Chapter 3, the authors consider potential claims about least upper bounds on sentence length like those in (2) below, which they refer to as *size laws*.

(2) a. Each sentence is less than k elements long, for k a finite number.
b. Each sentence is less than $\aleph_0$ elements long.
c. Each sentence is less than $2^{\aleph_0}$ elements long.
d. Each sentence is less than $2^{2^{\aleph_0}}$ elements long.
—etc.—

L&P observe that arguments against sentence length bounds that appear in the literature all argue against (2a) and conclude that (2b) is correct. They note that most of these arguments are flawed and point out some of the problems with them. However, they suggest that one argument, due to Katz (1966) appears to be valid; a somewhat modified version of the argument from the one L&P present can be given along the following lines. Suppose a grammar G conforms to (2a) for some fixed k. Take grammar G′ to be just like G in terms of the structural aspects of the definitions of syntactic well-formedness which it includes, but assume that G′ conforms to the length bound $k+m$, for some finite $m>0$. Let L(G) and L(G′) be the languages generated by these two grammars. We note that in the second of these, there are two sets of sentences. The first set consists of all sentences with lengths less than k; these are in fact all of the sentences in L(G) as well. The other set in L(G′) contains all sentences with lengths greater than or equal to k but less than $k+m$; none of these sentences are in L(G). However, the *only* thing preventing sentences in this second set from being generated by G is the length bound k, since by hypothesis G and G′ define the structural aspects of syntactic well-formedness in exactly the same way. Therefore, from the standpoint of those structural properties, the bound k is an arbitrary restriction on G since it does not provide a linguistically meaningful criterion for distinguishing sentences in L(G) from nonsentences. As a result, there are no nonarbitrary finite bounds k on G, and the sentences in L(G′) with lengths greater than or equal to k are in fact generated by G as well.

However, L&P observe that the same logic when applied in the case where the bound is some *transfinite* number c will lead to a parallel conclusion to the one Katz obtained in the case of finite bounds k. Take a language whose sentences are limited to having lengths less than c. There are infinitely many potential sentences with lengths greater than c that share the structurally relevant defining characteristics of the sentences of L. But, as Katz's argument suggests, a string meeting the structurally relevant defining characteristics of a language *is* a sentence of that language, since size bounds are in general *not* structurally relevant. Therefore, there are no size bounds, either finite or transfinite, on the lengths of natural language sentences. So, the authors conclude that the one solid argument against a finite bound on sentence length of the form in (2a) actually generalizes to an argument against *any* of the infinitely many size laws summarized in (2). Hence the vastness of natural languages.

L&P go on to note that the incorporation of any of the size laws in (2) into a grammatical description of a natural language will amount to a needless complication of that description and so by Occam's razor must be eliminated. They argue that this conclusion holds even in the case (pointed out to them by E. Keenan) of constructive systems in which the length bound (2b) seems to follow as an automatic consequence of the way the systems characterize the derivation of a sentence as being a finite sequence of finite strings. L&P point out that there are infinitary logics in which derivations are permitted to be infinite sequences and that the existence of such systems indicates that a finiteness bound on derivations will in general add an arbitrary complication to their otherwise size-independent characterization. Therefore, even in the standard sorts of constructive systems the requirement that each derivation be finite does not come "for free" but in fact demands that special statements be made in order to impose this restriction. Since these statements amount to complications of the notion 'well-formed sentence' which are in general not grammatically relevant, they too must fall to Occam's razor.

Having shown in Chapter 3 that there is no basis for imposing a size law on collections of natural language sentences, L&P in Chapter 4 present a property of coordinate structures, closure under coordinate compounding, which they take to be a property of natural languages and from which they derive two conclusions: (a) natural languages are not only larger than countably infinite but are in fact megacollections, and (b) since this result entails that there is no (finite or infinite) bound on the length of natural language sentences, it provides a linguistically principled basis for the claim that natural language sentences do not conform to any size laws.

The argument begins with L&P's formal definition of coordinate structures (which they refer to as 'coordinate compounds') and the notion of a 'coordinate projection of a set U of constituents', which is a coordinate structure in which each conjunct contains a constituent of *U*, every element of U appears in a conjunct of the structure, and the conjuncts appear in fixed order. (Fixing the order simply allows L&P to refer to the unique coordinate projection of a set.) The authors then define *closure under coordinate compounding* (hereafter, *closure under CC*) for natural language sentences in the standard way; if U is a subcollection of the entire collection of sentences in a language, then the coordinate projection of U is also a sentence in the language. L&P then provide an informal demonstration that natural languages must be megacollections if they are closed under CC. Begin with a natural language like English, an infinite subset of noncompound sentences of the language, like S_0 in (3), and the closure assumption.

(3) $S_0 = \{$Babar is happy, I know that Babar is happy, I know that I know that Babar is happy, . . .$\}$

For each subset of S_0 with more than two elements, there is a coordinate projection, and if English is closed under CC, then all of these coordinate structures will be in the language. Call the set containing S_0 and all of its coordinate projections S_1[2]. Since the cardinality of S_0 ($\#S_0$) is $\aleph_0$, $\#S_1 = 2^{\aleph_0}$, and so English must contain at least this many sentences. However, if S_1 is a subset of English, and English is closed under CC, then there must be a set S_2 containing S_1 and all of *its* coordinate projections, and all of these must be English sentences as well. Since $\#S_1 = 2^{\aleph_0}$, $\#S_2 = 2^{2^{\aleph_0}}$, and so English must be at least *this* large. If English is closed under CC, there is apparently no end to this progression of sets of coordinate projections; it thus appears that English has a magnitude on the order of the collection of all sets, i.e., is a megacollection. L&P proceed to prove this formally in their NL Vastness Theorem

(p. 58) which is an analogue of Cantor's Paradox applied in the particular case of sets closed under CC.

The proof of the NL Vastness Theorem rests crucially on the assumption that natural languages are in fact closed under CC. L&P argue for several pages that this is the case. Employing arguments parallel to those used in Chapter 3 to show that no sentence length laws hold of natural languages, they contend that the closure of sentences under CC is not restricted to any arbitrary size bounds in natural languages and hence that the unrestricted version of closure given above indeed applies to such collections. The main point of their argument is summarized in the following passage (p. 61).

No matter how one characterizes the collection of coordinate structures of English, closure would be violated if some *independent* English rule said, for example, that there was a maximum bound on number of conjuncts.... But the known facts about coordinate compounding in NLs reveal the existence of no such constraints. Principle (8) [the closure principle – SGL] claims that the lack of such is nonaccidental.

L&P end the chapter by proving that the results of Chapter 3 follow from the NL Vastness Theorem, and hence, since the latter is grounded in what L&P take to be the empirically justified closure principle, so is the absence of any size bound on sentences.

Chapter 5 deals with some of the immediate consequences of the NL Vastness Theorem. The first section is concerned with showing that nearly all of the currently available syntactic frameworks are incapable of adequately characterizing the properties of natural languages since these theories all involve constructive systems which necessarily enumerate at most countably infinite sets of sentences. The second section takes up the straightforward ways in which the *nonconstructive* system of Johnson and Postal (1980) can be modified in order to account for the results of the preceding two chapters. The third section considers the implications of the NL Vastness Theorem for Katz's (1981) notion of effability, the property that "[each] proposition (thought) is expressible by some sentence in every natural language." The final section briefly treats some issues in language learning and introduces several notions, including that of a *psychogrammar*, a term introduced by Bever (1982) to mean a grammar which is mentally represented in some actual individual, which are taken up again in more detail in the following chapter.

Chapter 6 is devoted to arguing that (a) the previous results are consistent with Katz's (1981) view that natural languages are platonic objects, but (b) even if the more usual conceptualist views of natural languages are adopted, they cannot in themselves be used to contradict the conclusions of Chapter 4. The authors explicitly argue against three brands of conceptualism. The first, which L&P dub 'standard conceptualism', holds that grammars are psychogrammars, that psychogrammars describe knowledge of sentences, and that the representation of such knowledge is possible only if the psychogrammar is constructive. According to the authors, the problems with this view are, first, that grammars cannot be psychogrammars, for reasons that they present in chapter 5, and second, that *nonconstructive* psychogrammars could still yield strictly constructive descriptions of the (finite) set of attestable sentences if they were combined with parsing and production mechanisms that were themselves computational in nature and hence constructive.[3] The second type of conceptualism, 'performance conceptualism', apparently runs into the same problems that follow from not maintaining the standard competence/performance distinction. The third sort of conceptualism, which L&P label 'radical', has been put forth more recently by Chomsky (1980, 1981). On this view, *grammars* are real because they are represented in the minds of speakers, while *languages* are at best derivative objects and hence do not warrant special theoretical attention. L&P direct most of their remarks in this chapter toward showing that this latter position leads to numerous undesirable consequences.

The final chapter is concerned with showing that the platonist position on natural languages is in fact the appropriate one to adopt. This view takes linguistic objects to be abstract but real entities on a par with sets, numbers, and other mathematical and logical objects and hence takes linguistics, whose fundamental goal is to characterize the nature of natural languages, to be an essentially mathematical enterprise.

As already mentioned, L&P seem to have covered all possible bases in their arguments, in the process leaving the reader with the impression that their conclusions are unavoidable. However, despite the seeming completeness of L&P's results, a number of objections can be raised about the assumptions that they use in reaching those conclusions. The main problems have to do with the Occam's razor arguments about sentence size laws in Chapter 3 and the size of natural languages in Chapter 4. In arguing that no size laws hold of natural languages, L&P state (p. 44),

> Since considerations internal to linguistics based on simplicity lead to [the] recognition [of transfinite sentences] and since Occam's razor forbids the incorporation of useless complications, the conclusion seems inescapable.
>
> Maximum simplicity is achieved not merely by assuming that NLs are infinite but by recognizing that their sentences are subject to no size bound at all, finite or transfinite.

However, the notion 'simplicity of theoretical statement' in a domain governed by considerations of empirical adequacy is a somewhat different one from that employed in mathematics. *Mathematical* simplicity involves concerns about elegance, compactness, lucidness, and ease of presentation of theoretical statements. On the other hand, while *scientific* simplicity also involves these notions, it relativizes them to the restrictiveness of the claims about the structure of the world made by the theoretical statements. In particular, it seems that scientific inquiry is governed by the following principles.

(4) In deciding between competing theories $A_1, A_2, \ldots, A_n$,
  a. if the empirical consequences of all the theories are compatible with known facts, choose the simplest theory;
  b. if there is a set of consequences in each theory such that no known facts at present decide whether the consequences are or are not true, choose the simplest theory whose claims place the greatest restrictions on the structure of the empirical domain in question.

(4b) helps to guarantee that when the future relevant facts are uncovered, the empirical constraints imposed by those facts will be capable in principle of falsifying the existing theory as straightforwardly as possible.

Since L&P still take linguistics to be bound by considerations of empirical adequacy at least in regard to native speakers' grammaticality judgments (their notions of 'attested sentence' and 'inductive basis for a language' seem to be grounded in just such judgments), it would appear that the principles in (4) are applicable in linguistics. More specifically, (4b) would seem to be applicable in the cases that L&P are concerned with. We know from the argument presented by Katz (1966) that the size bound for sentences must be at least $\aleph_0$. Furthermore, at this point (i.e., *before* L&P have presented their arguments about coordinate structures) there are no empirical arguments one way or the other about size bounds greater than $\aleph_0$. By (4b) then, we are compelled to choose a description which imposes the greatest restrictions on the domain in question.

L&P's remarks on p. 45 concerning Keenan's observation now become relevant. Recall that the observation was that the usual sorts of constructive systems entail a length bound of $\aleph_0$ as a consequence of defining 'derivation' to be a finite sequence of finite strings. L&P's reply – that this consequence does not come for free but is purchased at the cost of explicitly stipulating the finiteness limitation on derviations – while valid, seems somewhat beside the point in light of the fact that (4b) applies here. The choice, as L&P themselves see it, is between adopting a constructive system of one of the usual sorts or a nonconstructive system for defining natural language grammars. But the usual sorts of constructive systems impose far more restrictions on what can count as a possible grammar for a natural language than the unconstrained sorts of nonconstructive systems that the authors envision. Hence, by (4b) we should choose the usual sorts of *constructive* systems over the nonconstructive alternatives. Having adopted constructive grammars within linguistic theory on these grounds, we find that the size law (2b) above obtains, since these sorts of grammars only permit countably infinite numbers of sentences of finite length. This result may well change over time if it turns out that the class of constructive systems is inadequate on specific empirical grounds, but such is the way with empirical research.

To return to the quotes cited above then, while "maximizing simplicity" may be achieved "by recognizing ... no size bounds at all," maximal simplicity does not appear to be the relevant deciding factor in this situation. Furthermore, the "complications" required in the statement of the chosen theory of constructive grammars that their derivations be finite are not "useless' in the least, since those stipulations are among the ones which make such grammars more restrictive than nonconstructive grammars. Not all statements added to an empirical theory constitute unnecessary complications. In particular, a statement which is consistent with known facts and which contributes to the overall restrictiveness of the theory is far from being a theoretical appendage; under the principles of scientific research, we are in fact compelled to add such a statement to the theory.

The preceding comments appear to apply to the justification that L&P give in Chapter 4 for assuming that natural languages are closed under CC. Thus, they argue (pp. 62, 63),

> But any such choice [of a bound on the magnitude of the class closed under CC – SGL] leads to a complication, and thus will be rejected under Occam's razor, unless it can be argued that some basic justified ontological or methodological principles proper to linguistics justify the particular boundaries.
>
> General scientific principles demand that the projections from the small finite samples to the desired characterizations involve the maximally general laws (principles) projecting the regularities found in observed cases to the collections as wholes. Putting aside the ontological and methodological grounds for limiting projections discussed in Chapter 6, one can then never justifiably replace a more general projection by a less general one unless this is factually motivated.

It is important to point out that in Chapter 6 the authors consider several different ontological views about linguistic objects, as well as some methodological issues specific to linguistics which are closely related to those ontological positions, but they do *not* directly take up the restrictiveness issue or other general questions of scientific methodology. Nevertheless, it would seem that those more general considerations might well be as relevant in the type of case that L&P are considering as "principles proper to linguistics". Indeed, as we have just seen, there *is* a set of circumstances under which one can justifiably posit a less general projection from a small sample of data without factual motivation, namely, (a) when the principles in question are consistent with known facts, (b) when there are currently no empirical arguments for or against certain consequences of those principles, and (c) when the principles place strong constraints on the way that the domain in question is structured. For these reasons, as noted above, we should adopt some constructive notion of grammar for linguistic theory. Doing so leads to the conclusion that there is a bound on closure under CC, and that bound is $\aleph_0$. However, if *that* is the case, then the NL Vastness Theorem does not go through because its proof requires that natural languages be closed under CC *without bound*, and constructive grammars will not permit this.

It thus appears that there are good reasons for questioning the following conclusion which L&P reach at the end of their argument (pp. 67–68):

> Just as one cannot simply decide that rules are (or are not) structure-dependent, one cannot just decide that sentences are (or are not) all finite, or that the number of conjuncts in a coordinate compound is always finite. In both cases, arguments based on the nature of the attested part of the subject matter are required... Unfortunately for linguistics, the sentence finiteness decision has been arbitrarily made and maintained for nearly thirty years. But this mistake contains no justification for its continuation.

Contrary to L&P's remarks here, it seems that general scientific considerations *do* offer a nonarbitrary way to settle the issue in such cases, and in the particular case in question, they decide in favor of constructive systems of grammar. In fact, from the viewpoint of scientific methodology, the mere fact that we have learned as much about the structure and organization of language as we have over the past thirty years using constructive grammatical techniques indicates that we have been playing the game in the right ballpark, even if we still have not come to an agreement on what the set of rules should look like.

It is perhaps not too much of a surprise that the second author, P, would present arguments like that above which ignore the issue of restrictiveness in empirical theorizing. A dozen years before *VNL* appeared, P published a paper in a conference volume titled "The Best Theory" (Postal, 1972). In it, he argued that the Generative Semantics

(GS) descendant of the standard theory of transformational grammar was to be preferred over the theory's Interpretive Semantics (IS) offspring on the grounds that IS contained theoretical machinery not needed by GS, and hence, other things being equal, GS should be chosen over IS. However, as the above discussion suggests, things generally are *not* equal, especially when we are comparing theories some of whose claims cannot be directly decided by known facts. As we have seen, in such cases the restrictiveness requirement of (4b) comes into effect. Contrary to what P was arguing in the paper cited above, IS clearly made more restrictive claims about what counted as a natural language grammar as a result of assuming the existence of a D(eep) S(tructure) level at which all lexical insertion takes place. Hence, far from being an unnecessary complication, the adoption of a DS level would seem to make the *IS* approach the better of the two theories. Similarly, as already pointed out several times above, constructive systems are more restrictive than nonconstructive ones, and hence, in the absence of compelling evidence to the contrary, are to be preferred as the basis for natural language grammars. Apparently, P still refuses to recognize the methodological principle that underlies decisions of the sorts just mentioned. That P still holds this view, despite twelve years of methodological debates arguing the contrary, indicates that he is, at the very least, maintaining consistency with his earlier views of the nature of linguistic inquiry.

Those views are related to two more or less explicit ulterior motives driving the arguments in this volume. The first has to do with the theory of arc-pair grammars developed by Johnson and Postal (1980) as the formal basis for the Relational Grammar framework (Perlmutter, 1980). The latter is a system of grammatical description which grew out of several concerns raised by the GS research of Postal and Perlmutter. The important point for the present discussion is that arc-pair theory is basically the only current *nonconstructive* system used in linguistics. So, if L&P can show that the usual kinds of constructive grammatical systems are inadequate on *mathematical* grounds, i.e., that they are intrinsically incapable of adequately characterizing certain properties of natural languages, while nonconstructive systems do not suffer from the same defect, then arc-pair theory will be in an excellent position to fill the ensuing theoretical vacuum.

However, as we have seen, the assumptions used by L&P in rebutting potential counterarguments rest on highly debatable methodological grounds. Hence the status of arc-pair theory, and any other potential syntactic frameworks of a nonconstructive nature, reverts back to what it was *before* L&P undertook the *VNL* exercise – in an empirical and restrictiveness competition with constructive theories of grammar, a competition which the NL Vastness Theorem was intended to try to circumvent.

The second ulterior motive is to support Katz's (1981) view of natural languages as basically mathematical objects and hence of linguistics as a sub-field of mathematics. This position permeates the volume, in keeping with L&P's view that linguistics is the study of language *per se* without regard for its relation to psychology or any other field. Not only are set theoretical notions discussed at length, actual mathematical proofs given, and arguments set out in an intentionally deductive style, but nearly all of the analogies drawn between linguistics and other fields involve emphasizing the parallels between linguistics and mathematics or logic. Numerous illustrations are taken from the history of the latter two fields, but one is hard-pressed to find any from the history of science.

Notice though that the methodological objections about restrictiveness raised above are based on the premise that linguistics is a field of empirical inquiry, on a par with psychology, biology, etc. However, if linguistics is a *mathematical* field, then the earlier objections dissolve, L&P's arguments for *maximal* simplicity and generality become sound, and the NL Vastness Theorem will go through. Under such circumstances, we would have no more reason to exclude sentences like (1) from the collection of English sentences than we would in banning the set of natural numbers as a subset of the real numbers.

Nevertheless, it is not at all clear that platonism is the appropriate view to hold of linguistic objects. If linguistics is assumed to be a mathematical field, a serious question arises as to what criteria determine the subject matter of the domain to be studied. As already noted briefly, L&P take as the starting point for several of their arguments the notion of 'attested' sentences, the (necessarily small) finite set of sentences of a natural language that have actually been judged by real speakers to be well-formed. But speaker' judgments about the well-formedness of sentences would seem to constitute a set of *contingent* facts about the world. Theories in pure mathematics and logic are generally not bound to accommodate such apparently empirical facts, whereas scientific theories are.

As an illustration, consider one fact derived from speaker's judgments about English sentences:[4]

(5) Reflexive pronouns must be bound by their antecedents within the same clause in which they appear; regular (personal) pronouns must be bound by antecedents outside the clause in which they occur.

Why should this be the case? From a purely logical view, the situation would appear to be far simpler if there were only one type of pronoun whose antecedents could occur anywhere. Indeed, standard logics typically allow an open variable to be bound by an operator anywhere within the operator's scope. Nonetheless, native speakers simply do not accept sentences as grammatical if they contravene (5).

But why should *that* fact have any bearing on the *mathematical* study of string sets? There appears to be no reason why we cannot countenance a whole series of language types, one conforming to (5), the next conforming only to the first clause of (5), another conforming only to the second clause of (5), and so on, all of which would seem to be equally legitimate as objects of mathematical inquiry. These types of languages are not, however, all on an equal footing with respect to the study of linguistics; instead, only the first is a legitimate set to study, exactly because it represents a generalization derived from native speakers' intuitions. There are endless such facts about natural languages which seem quite arbitrary from the standpoint of mathematical study; yet, from the viewpoint of an empirical science, they form the kind of raw material out of which theories about the world are typically constructed.

Holding to the view that linguistics is a branch of mathematics, L&P tell us that,

> it is as arbitrary to claim that some structures have too many conjuncts to be proper coordinate compound sentences as it is to claim that some aggregates have too many elements to be (power) sets. (p.64)

What the authors do not tell us is why a constraint like (5) is less arbitrary from the standpoint of linguistics as a *mathematical* field than a constraint which had the consequence of limiting the number of conjuncts in a coordinate structure to being finite. They do inform us, at some length, that generalizations like (5) are less arbitrary because their truth is based on attested sentences whereas the finiteness constraint cannot be evaluated on the basis of *any* (necessarily finite) set of attested sentences. However, they present no arguments showing why conditions derived from attested sentences, which appear to be essentially empirical generalizations, should constrain *mathematical* inquiry, while conditions underivable from attested sentences should not. Yet L&P would apparently accept (5) as a defining feature of at least one natural language. Taking linguistics as an empirical science would seem to avoid this problem.

Much later, in Chapter 7, L&P worry about the question of where infinitely long sentences like (1) are to be studied if they are excluded from the domain of objects to be characterized by linguistics. If they are not included, then L&P claim that "some non-linguistic theory [must] incorporate *every valid linguistic law*" (p. 162; emphasis in original), since, size considerations aside, such sentences observe all other generalizations about natural language sentences, but this is just the sort of duplication that Occam's razor should eliminate.

However, under the assumption that such sentences *are* to be excluded from linguistics proper, it would seem that a natural place to study them would be in mathematical linguistics, the branch of applied mathematics devoted to studying the mathematical properties of linguistic systems. Such research does not really *duplicate* the assumptions of linguistics in an essential way, i.e., as part of the foundations of the field of mathematical linguistics; instead, it takes those assumptions as being among the objects whose mathematical features are to be examined. It would seem reasonable, furthermore, that that field would also study extensions, reductions, and other modifications in the set of principles underlying linguistic theory. Thus, it would seem legitimate for mathematical linguistics to study the types of languages meeting the several variants of (5) listed above, L&P's example of languages meeting the Coordinate Structure Constraint with sizes not necessarily restricted to being denumerably infinite, and the like. In fact, L&P's remarks can be viewed as an argument in favor of including the study of transfinite collections of sentences as part of mathematical linguistics, under the assumption that they are excluded from linguistic theory for the reasons mentioned earlier.

A few remarks need to be made about the arguments in Chapter 6. First, nothing said so far inpugns L&P's demonstration, perhaps the most solid in the book, that the standard conceptualist argument in favor of constructive grammars does not hold. As L&P summarize their argument,

> One way that a psychological interpretation of NLs could yield an ontological escape from the NL Vastness Theorem would be via a claim that psychogrammars are *necessarily* constructive . . . [this view] has no known justification. Even if psychogrammars are nonconstructive, they could provide constructive knowledge of individual (finite) sentences via their incorporation in constructive producers and parsers. Hence, even the view that one must provide a constructive account of speakers' knowledge of sentences offers no support for the doctrine that psychogrammars per se are constructive. (pp. 121–122)

While this argument may block the usual conceptualist justification for constructive grammars, it should be noted that the authors' conclusion is not especially strong. It does not specifically *bar* constructive systems from being psychogrammars, nor does it *require* nonconstructive systems as psychogrammars. The argument boils down to this: as things now stand, *any* assumptions about the nature of psychogrammars can be made compatible with our assumptions about parsing and production mechanisms. As a result, the argument would seem to revert back to the more basic question of whether the platonic view of linguistic objects should be adopted.

Next, as observed earlier, most of Chapter 6 is given over to attacking what the authors see as the deficiencies and inconsistencies in Chomsky's most recent position concerning the reality of grammars. It is not possible to take up a detailed discussion of L&P's criticisms here, and in any case, if Chomsky takes L&P's comments at all seriously, we can anticipate a lengthy response from him in the near future. However, there is one argument that the authors present against radical conceptualism which is worth considering since L&P claim that it is relevant for the standard view of conceptualism as well.

The argument rests on the distinction the authors draw between two types of grammars: 'grammar$_1$s', which are individual grammars defined by universal grammar (UG), the mechanism that Chomsky takes as the innate characterization of possible human grammars, and 'grammar$_2$s', which are psychogrammars, i.e., those actually represented in the minds of real individuals. They then use this distinction to uncover what they take to be an inconsistency in Chomsky's claim that grammars generate mental representations (MRs) of sentences rather than sentences themselves. L&P ask us to consider a grammar$_1$, one of the grammars defined by UG, which is not a grammar$_2$, that is, one which has never actually been learned by a human but which could potentially be learned. Call this grammar$_1$ G_x. The question then is, What sorts of things does G_x generate? L&P respond by saying, "[the] answer cannot under any sensible assumptions be mental representations . . . [its] outputs must be sentences" (p. 130). They go on to note that because all of the grammar$_2$s are also grammar$_1$s (grammar$_2$s are just those grammar$_1$s which have actually been learned), grammar$_2$s must also generate sentences rather than MRs of sentences.

Apparently, L&P are taking the notion 'generate a mental representation' to mean 'actually compute a MR' (and perhaps store it); that is, they seem to be taking the verb *generate* in its ordinary language sense of 'produce' rather than in its technical sense of 'characterize'.

But why can't we take *generate* in this technical sense here? If G_x above is a grammar$_1$ which could potentially be learned, i.e., is a *potential* grammar$_2$, why can't we simply say that it generates 'potential' MRs, under L&P's sense of MRs being actually computed (and possibly stored) objects? The authors counter that this is not possible, because the

> claim that the grammar,s characterized by UG describe 'potential knowledge' is untenable in the face of unlearnable NLs, for which there is not even any potential human knowledge (of the sort in question) (p. 131),

and they point out that this difficulty holds for standard conceptualism as well as the more radical sort.

Although L&P mention problems with demonstrations based on learnability claims at several points in the volume, they really only give two clear arguments against the use of such claims. First, having shown that there are infinitely many finitely specifiable grammars for natural languages, they conclude (pp. 149–151) that there are infinitely many *unlearnable* grammars, since infinitely many of them are so large that, even though they are finite, there are not enough electrons in the known universe to put into one-to-one correspondence with their elements. This result, though, does not seem to square with the work of learnability theorists like Wexler and Culicover (1980), who have shown that any grammar in a particular suitably constrained version of standard transformational theory can be learned 'in the limit'.[5] Since the grammatical theory that Wexler and Culicover adopt would seem to allow infinitely many finitely specifiable grammars, how can their results be reconciled with L&P's conclusion?

At least part of the solution to this puzzle seems to rest in the fact that the two sets of researchers are dealing with separate types of grammars. Wexler and Culicover are mainly concerned with characterizing *potentially* learnable grammars, those that could *possibly* be learned if there were no constraints of time or memory involved, and hence their interest in learnability in the limit (see Note 4). On the other hand, L&P focus their attention on *actually* learnable grammars, just as they focus on *actual* grammars and MRs in the quotes above, for which time and memory constraints are of crucial importance. However, there is nothing preventing researchers interested in abstract learnability results from agreeing that the *actually* learnable grammars are those learnable within the time and space limits imposed by other psychological and biological systems that interact with UG in the acquisition of actual grammars. Since this is essentially what L&P suggest (p. 151) from the viewpoint of their platonist conception of language (minus the bit about the role of UG), there does not appear to be much of an argument against conceptualism here.

The second argument against learnability considerations, given in Chapter 7, contends that there are (transfinitely many) natural languages that are not even learnable in the limit. Unfortunately, this conclusion is based on the earlier NL Vastness Theorem, and so to the extent that there are doubts about the assumptions used in proving that theorem, there are doubts about this conclusion concerning learnability.

It seems, therefore, that a conceptualist (of either the standard or the radical variety) could reasonably respond to the quote above from p. 131 along the following lines. (1) UG defines the class of grammars$_1$s which are *potentially* learnable. (2) Grammar$_1$s generate *potential* sentences (or MRs of sentences). (3) Psychological and biological systems interacting with UG limit the class of grammar$_1$s to the set of *actually* learnable grammars, which we might call 'grammar$_a$s'. (4) Grammar$_2$s form the subset of grammar$_a$s which *have actually been learned*; these generate actual sentences (or actual MRs of sentences). (5) The goal of linguistics is to characterize UG by studying (among others) speakers' judgments based on their grammar$_2$s' characterizations of real sentences (or real MRs of sentences). Since the potential vs. actual grammar distinction is relevant only in situations where it is necessary to keep these learnability issues straight, and since (conceptualist) linguistics is concerned with specifying the class of *potential* grammars, for all practical purposes, conceptualist linguists can drop the potential/actual labels and continue talking as they typically have in terms of UG characterizing grammars which generate sentences (or MRs of sentences).[6]

Despite the preceding somewhat negative comments, there are several positive points about *VNL* which should be stressed.[7] First, the logical rigor and clarity with which the arguments are presented is quite refreshing and should make the text accessible even to the least mathematically inclined readers. Next, the authors do the field a service by continually emphasizing how mind-bogglingly large a finite sentence generated by a constructive grammar could be. We tend to think of sentences as strings which contain, at the outside, perhaps a half dozen clauses and in any case generally do not run on for more than a few inches on the printed page; Joycean sentences that continue for pages at a time strike us as either deviant or funny, depending on our sense of humor. But if we are to take the usual assumptions about infinite sets of finite sentences seriously, we are going to have to get used to the idea that sentences containing $10^{10^{10}}$ words are included in those sets. In addition, L&P's demonstration that the standard conceptualist argument for constructive grammars is invalid drives home the often overlooked logical point that constructive systems are not the only finite means of specifying knowledge about objects in an infinite domain.

These positive aspects notwithstanding, the reader is apt to find L&P's rhetorical style at times a bit hard to take. Conclusions are strongly worded, are rarely qualified or hedged, and are presented as always following as a matter of necessity. The authors reserve their most combative language for criticizing Chomsky's views. Thus, in the early chapters, we find Chomsky's position concerning finite constructive grammars "obviously insupportable" (p. 17) and "extraordinarily enticing because it has fallaciously been promulgated" (p. 19); later, we find this view described as a "pervasive myth about linguistic knowledge" (p. 100). Chapter 6 is especially peppered with inflammatory wording. There, the conceptualist position is characterized in light of Katz's (1981) work as "an extensively socially institutionalized, but intellectually unjustified approach" (p. 112). Chomsky's recent radical conceptualism is described as "a totally inadequate conceptual framework" (p. 126) and "so confused and so divorced from reality that it is out of the question to take [it] as providing a rational ontological framework for linguistics" (p. 152). These examples, which represent only some of the more blatantly antagonistic passages scattered throughout *VNL*, are reminiscent of the acrimonious style used during the GS debate of some years ago. Just as the rhetoric then acted as an unnecessary distraction from the substantive points that the opponents in the debate were trying to make, so too does the rhetorical style in *VNL* get in the way of what L&P are trying to say.

So, the bottom line on *VNL* seems to come down to this. If the reader buys Katz's platonic view of linguistic objects, as L&P do, then s/he will accept the authors' conclusions at face value. On the other hand, if the reader holds to a position closer to standard conceptualism, the results simply will not follow. But those readers who take the platonic position will find themselves, in the company of L&P, with several important questions concerning the relation between empirical generalizations and the study of supposedly mathematical entities still to be answered.

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Notes

[1] L&P employ the terms 'collection' and 'megacollection' rather than 'class' and 'proper class' because common usage tends to blur the technical distinction between 'class' and 'set' which L&P are concerned with maintaining. Thus, L&P take a *collection* to be the extension of an arbitrary open sentence, a *set* to be a collection which is a member of some other collection, and a *megacollection* to be a collection which is not a set and which has the magnitude of the collection of all sets.

[2] Notice that since S_0 is itself a subset of S_0, the coordinate structure for the infinitely long sentence in (1) will be a member of S_1.

[3] It is interesting to note that Postal observed some years ago (1972: Note 2, p. 163) that problems with (i) below render logically invalid "the argument usually given to show that each speaker must have internalized within him a generative grammar."

(i) The *only* way a finite object can know (in the specific sense) an infinite set of objects is to have represented in it a finite device which recursively enumerates this set.

This point is relevant again in the discussion of conceptualism below. It is also interesting to note that the main line of argumentation in the same paper plays a crucial role in the reasoning presented by L&P to back up their results; see the discussion of the restrictiveness issue below.

[4] Or, if the reader prefers, in terms of the Government-Binding theory (Chomsky, 1981), reflexives and other anaphors must be bound within their governing categories, while regular pronouns must be free in theirs.

[5] A term due to Gold (1967) which indicates that a language can be identified upon exposure to a finite number of sentences presented serially. The notion abstracts away from the time and order of presentation constraints associated with actual language learning.

[6] L&P clearly take a hostile attitude toward Chomsky's notion that grammars generate MRs of sentences rather than sentences themselves. A more sympathetic interpretation of Chomsky's admittedly general comments on the matter might take them as an attempt to stress the *difference* between the mathematical study of languages (which L&P advocate) and the psychological study of them (which Chomsky has consistently supported). For instance, by saying that grammars generate MRs of sentences, Chomsky may be trying to emphasize the fact that he is primarily interested in studying properites of (potential) psychogrammars. Thus, a more sympathetic reading might actually take Chomsky's remarks to be stressing the difference between 'sentence' and 'knowledge of sentence', something L&P accuse him of failing to do, under the assumption that the knowledge of a sentence is encoded in the MR of that sentence generated by a (potential) psychogrammar.

This is not to say that Chomsky's comments are not without their problems. Even under this sympathetic view, as least the following questions are left unresolved. What *are* sentences and languages then under radical conceptualism? How are they related to MRs? What does it mean to say that if something is characterized by a grammar, it is epiphenomenal? If languages are epiphenomena, does that actually make them not real, as Chomsky (1981) indicates? Until these issues are satisfactorily clarified, radical conceptualism is not likely to supplant standard conceptualism as the dominant view in linguistics of linguistic objects.

[7] A few typos and technical problems should be noted. In Chapter 3, L&P construct an abstract language consisting of *a*'s and *b*'s with the constraint (among others) that no sentence can have three *b*'s in a row. However, one of the example strings that they give for this language ((10 g), p. 40) is *abbabbb*; apparently, the last *b* should have been left off. On p. 43, L&P show that the strings in this language can be put into one-to-one correspondence with the binary expansion of the real numbers between 0 and 1. They start by taking *ababb* = .0 = 0, but then they say *ababb* = .1. From the rest of the passage, it should be clear that .1 should have been *abbab*. Finally, on pp. 52–53, the authors present an argument that every subset of a collection of constituents has a coordinate projection, a fact that they need in order to make the proof of the vastness theorem work. However, the argument as stated does not go through. Basically, all L&P do is take a subset of constituents, U, assumed to have *k* elements, take an arbitrary coordinate structure based on the constituents of U also containing *k* conjuncts, and note that there is a one-to-one correspondence between the conjuncts and the elements in the subset U. But that fact alone will not guarantee that the uniqueness and existence conditions on coordinate projections in (6) on p. 52 will be met by this coordinate structure; in constructing the

conjuncts, we could, for example, have left out one of the members of U and used another member twice, thus still leaving the coordinate structure with *k* conjuncts but also contravening (6b,c). Apparently, what we need to do is to construct the coordinate structure from the members of U in such a way that it will reflect the conditions given on p. 52. This is quite straightforward to do, however.

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