Chapter III

Two Proposals Concerning the Role of Meaning in Linguistic Analysis and the Justification of Grammars

3.1 Introduction. In the last chapter it was shown that substantial controversy and misunderstanding arose within structural linguistics at roughly mid-century concerning the role of meaning in linguistic analysis and over the issue of justification of linguistic theories, i.e., how a 'best' theory was to be selected. This chapter examines two influential programmatic proposals for linguistic theory conceived within this nexus; that of Quine, originating in his essay, "The Problem of Meaning in Linguistics" (PML), and that of Chomsky's massive typescript dated June, 1955, The Logical Structure of Linguistic Theory (LSLT). Both parallel and indeed may be viewed as spawned by the contemporary debates in structural linguistics regarding the role of meaning and how and whether 'reliance on meaning' impugns an 'objective' science of language. Both proposals are united by an effort to confront 'mentalist semantics' head-on; in so many words, the 'problem of meaning' is their joint raison d'être. Both are concerned with what Sapir, in 1929, referred to as "The Status of Linguistics as a Science", i.e., with the possibility of a science of language structure. Given the murky state of understanding surrounding notions of meaning, such an inquiry, both agree, should seek to avoid reliance on meaning. And both are interested in how and whether a 'best' theory of this kind can be chosen.

At this point there is a divergence. Quine's program seeks to show just where semantic notions do enter linguistics (taken as comprising grammar and lexicography) and how and to what extent the work done by these notions can be performed instead by operationally definable behavioral notions. In the different domains of linguistics, the success

of this replacement program varies. In grammatical theory, the fundamental notion of 'significant sequence of phonemes' may, pending a non-semantic definition of 'phoneme', be operationally reconstructed according to whether the utterance of a given phoneme sequence occasions a "bizarreness reaction" from native speakers. On the other hand, in lexicography, where the fundamental problem is with the pairing of synonyms, the notion of synonymy (which holds that syntax may vary as semantic content remains fixed, in Quine's construal) remains impervious to adequate reconstruction in the justificatory terms of observable behavioral correlates. Due to linguistic relativity, the determination of 'sameness of meaning' cannot, in principle, be reconstituted in the terms of observable behavior in such a way as to warrant the claim of a uniquely correct pairing of synonyms. In lexicography, at least, the evident lack of success in purely formal reconstructions of meaning leads to the conclusion that there is, then, "no fact of the matter", nothing for the lexicographer to be right or wrong about in his posited pairings of expressions. Later developed and amplified as "the doctrine of indeterminacy of translation", this position has, as a consequence, that linguistics is in a rather different situation from the other sciences where theories, though admittedly not uniquely determined by a range of data, nonetheless relate to data where there is, after all, something to be right or wrong about. The thesis of indeterminacy, arising from a suitably reconstructed and hence scientifically respectable linguistics, is the Quinian analogue of (and may be viewed as a partial response to) concerns in structural linguistics

posed by the so-called "non-uniqueness" of theories. It is this latter issue which is the focus of Chomsky's LSLT, in turn, a partial response to Quine.

For Quine, the status of linguistic theories as science, ~~i.e.,~~ (that is, the extent to which ~~insofar as~~ linguistic theories are reconstructable in terms of behavioral analogues of the theory of meaning, is clouded by the specter of indeterminacy. The program of LSLT, in full accord with the Quinian attack on 'the theory of meaning', reaches a different assessment of the standing of linguistic theory, understanding now by this term exclusively the theory of grammar. While in agreement with Quine as to the regretable vagueness and imprecision of semantic notions, Chomsky argues that the particular replacement program called for by Quine is, in fact, unnecessary since, he argues, semantic notions are irrelevant to grammatical theory. The irrelevance of 'synonymy' in phonemic analysis is demonstrated, it is claimed, by the sufficiency of a purely operational and non-semantic test for phonemic distinctness, Harris' paired utterance test. In syntax, the irrelevance of the notion of 'significance' is shown, Chomsky argues, by the existence of sentences like Colorless green ideas sleep furiously which are, apparently, not significant at all but whose intuitive well-formedness is operationally attestable. The fundamental notion for the grammarian is not, consequently, 'significant sequence of phonemes' but 'intuitively well-formed sequence of phonemes'. As a result, grammar may be seen to be a theory of a native speaker's "intuitions of linguistic form", intuitions which have often, mistakenly, been held to be semantic. A different replacement

program is advocated: to reconstruct "intuitions of linguistic form" in non-semantic and, where possible (as in phonemics), operational terms. Thus firmly setting grammatical theory on a purely formal basis of primitives does not suffice, however, as a solution to the problem of the choice of a 'best' grammar. There are two central issues here. The first has to do with the notion of 'projection': how can a grammar of a language, i.e., of the in principle infinite set of intuitively well-formed sentences, be justified? The second concerns the insufficiency of any purely formal distributional procedures to justify setting up a particular class of grammatical elements. Since many other classes of elements, equally distributionally justifiable, might be set up instead, why is this particular class chosen? And how is the linguist to choose from among the conceivably many different formally-based grammars, each of which is in accord with the available empirical data, the "intuitions of linguistic form" of the native speaker? Taking a page from Quine and Goodman, Chomsky's answer is a grammatical metatheory based upon the notion of simplicity. With Quine, simplicity provides the means of a solution to the key methodological problem of corpus-based descriptive linguistics, that of taking an 'inductive step', of projecting a grammar of the language as a whole from a finite corpus of sentences. With Quine and Goodman, simplicity is seen as a solely system-internal consideration; it is not adequate as a basis for choice among opposing conceptual schemes or systems. With Goodman, choice among competing system-internal theories can be made by specifying a numerical measure of simplicity. In LSLT, these ideas are developed into the conception of a two-fold

program of linguistic research. On the one hand, to construct an explicit "general theory of linguistic structure", a metagrammar, in which are defined various "levels of analysis" (e.g., phoneme, morpheme, word, syntactic category, sentence, phrase structure, transformational) where 'higher' levels are motivated by the reduction in complexity they achieve in restating and reformulating the results attained at 'lower' levels. On the other hand, there is the important goal of constructing, in conformity with the theory of levels of the general theory, empirically adequate grammars of particular languages. The problem of non-uniqueness, i.e., of selecting from among empirically indistinguishable grammars of a language, is conceptually resolved by the requirement that the metagrammar be constructed by "literally defining simplicity" for grammars and that it incorporate a purely formal, in fact, mechanical evaluation procedure which is to select the (notationally) simplest of the candidate grammars. Thus the LSLT program for the justification and validation of grammars has a two-tiered structure of criteria of adequacy: "external" (empirical) adequacy and "internal" (notational simplicity) adequacy. Non-uniqueness is avoided and the standing of linguistic theory (i.e., grammatical theory) as a science is secured.

These differing assessments of the prospects for linguistic theory, laid down in the early 1950's, will reverberate throughout much of the next two decades, coming face-to-face, however, only at the end of the 1960's and thereafter at least as far as Chomsky (1980a). In this debate,[1]

[1] Chomsky (1969b), (1975c:198 ff) and (1980a); Quine (1969c) and (1972).

to summarize, Chomsky argues against Quine that the doctrine of indeterminacy is "bifurcationist",[1] unwarrantedly setting linguistic theory (which Chomsky includes as part of cognitive psychology) a double standard which restricts the kinds of evidence that may count for the correctness of one theory or hypothesis as against another. Yet as may be seen from the above, the respective programs of Quine and LSLT are each bifurcationist in their own way. Because of 'the problem of meaning', each singles out linguistic theory as facing special obstacles regarding the justification of theories. For Quine maintains that an unavoidable indeterminacy ~~infuses~~ pervades lexicography (and translation) due to an in principle absence of any objective criterion for determining a correct synonym pairing. And Chomsky holds that a 'best' grammar can be arrived at only ~~via~~ by means of a mechanical evaluation procedure.

There is, of course, ~~under the onus~~ in the context of accounting for child language acquisition, a substantial reinterpretation of, or "shift of focus" regarding, the conception of linguistic structure put forward in LSLT. The details of this reorientation are many, requiring a full-scale piece of work in themselves, and can only be alluded to here. ~~while~~ Additional discussion may be found on these matters in Chapter 4 §§ 2 and 3. In this chapter we limit our concern to a rather general presentation of the program for linguistic theory set out in PML and LSLT in which the replacement or the elimination of meaning from linguistic analysis is a major focus.

[1] Chomsky (1980a:16 et seq.); this sense of the term is taken from Hockney (1975).

In §3.2 it is shown that Quine's rational reconstruction of linguistic theory undergoes several changes yet never succeeds in presenting an argument which shows, as Quine later asserts, that indeterminacy arises for the grammarian in his stated task of demarcating all and only the well-formed (or 'significant') phoneme sequences of a language. And where an argument for indeterminacy is presented, in the case of the lexicographer become field linguist engaged in "radical translation", it may be replied that a doctrine of indeterminacy is perhaps only a lingering vestige of the very essentialist conception of meaning it is designed to combat. In §3.3 we examine the LSLT motivation for the approach to justification of grammars which involves a metagrammar based on the criterion of simplicity. As noted above, such a general theory of language structure is later to be viewed as an "innate schematism" or "universal grammar" which restricts the class of possible grammars available to the child language learner, thereby constituting an explanation for the uniformity, ease of acquisition and specificity of structure of the grammar attained by a child who has acquired a language. In §3.31 we consider the arguments presented for the irrelevance of semantic notions in grammatical theory according to the revision of "distributional analysis" proposed in LSLT. Here we find that the case for irrelevance goes through neither in phonemics nor in syntax, in the former due to degeneracies with the results of the pair test, in the latter because of a petitio principii. And when the irrelevance of semantic notions to grammar cannot be sustained, the character of a metagrammar, as defining simplicity, and the accompanying purely formal schema of justification lose their point.

In 3.4 . . .

3.2 Quine's Program for Linguistics. Quine's paper, "The Problem of Meaning in Linguistics" (PML), originally delivered to an audience of linguists at Ann Arbor in 1951, is an assessment of the prospects for a program of reconstructing linguistic theory so as to eliminate reliance on the notions of the theory of meaning. It attempts a parallel rational reconstruction of linguistics (comprised ~~of~~ the two domains of grammar and lexicography); the theme is to examine whether and at what points notions of meaning enter these domains, with an eye towards removing this reliance, where possible, through reconstruction in overtly operational and behavioral terms. And, where notions of meaning (in particular 'synonymy') do not admit of exact reconstruction, the aim is to point out the unwelcome but unavoidable consequences for the standing of linguistics as a discipline.

For Quine, the "sorry state of the theory of meaning" [1] (once detached conceptually from matters properly of reference and naming) boils down to problems with the two remaining aspects of meaning: what is it for a linguistic form to be significant (to have meaning) and what it is for two linguistic forms to be synonymous (to have the same meaning). [2] The problem with meaning then becomes the difficulty in explaining -- "preferably in terms of behavior" -- the notions of significance and synonymy without appeal to a realm

[1] (1953:132).

[2] (1948:11), (1951:48).

of shadowy and irreducible intermediary entities called meanings, which, in any event give only the illusion of an explanation.[1] To these twin aspects ("offspring") of the problem of meaning, there correspond two areas of linguistic inquiry: to the aspect of significance, there is the grammarian's task of devising "a recursive definition of a class K of forms which will comprise all and only those sequences of phonemes which are in fact significant".[2] To the aspect of synonymy corresponds the lexicographer's task of correlating synonyms either in one language or between languages. But the respective tasks are not really as distinct as might appear since, Quine suggests, the grammarian's concern with significance is a disguised concern with synonymy. This is because the grammarian's job is stated to involve the demarcation of all and only the significant sequences of phonemes of the language, and the general definition of the phoneme, unfortunately, invokes sameness or difference of meaning. And this, Quine concludes, is to invoke synonymy:

> Two subtly different sounds count as the same phoneme unless it is possible, by putting one for the other in some utterance, to change the meaning of the utterance. (2) Now the notion of phoneme, thus formulated, depends obviously and notoriously on the notion of sameness of meaning, or synonymy. 3

The statement of what the grammarian is about hence not only requires the notion of 'having meaning' but also implicates that

[1] (1948:12):"The problem of explaining these adjectives 'significant' and 'synonymous' with some degree of clarity and rigor -- preferably, as I see it, in terms of behavior-- is as difficult as it is important. But the explanatory value of special and irreducible intermediary entities called meanings is surely illusory."

[2] (1951:51).

[3] (1951:50). Footnote 2 cites Bloch and Trager (1942:38-52) and Bloomfield (1933:74-92) as linguistic authorities; see the discussion below in §3.3. Quine's inference, that recognition of sameness of meaning is recognition of synonymy, may be questioned along lines suggested by Hiż (1964): to say that two sentences are paraphrases does not imply that there is some thing that they both express; see the discussion of paraphrase in Chapter 5 §3.

of 'sameness of meaning'. The grammarian seems doubly compromised. [1] However, in order to pursue further his rational reconstruction of linguistic theory, drawing parallels between the grammarian and the lexicographer, Quine indulges in "the unrealistic assumption" that some non-semantical definition of the phoneme is at hand. [2] One parallel is that each can accomplish their respective ends only indirectly by enumerating the short or "atomic" forms and then displaying how these systematically combine to yield the longer forms. A more direct approach is impractical, if not impossible, given the size of the respective classes to be reconstructed, and, in this respect, the grammarian's enterprise is, contrary to standard assumptions, no more 'formal' than the lexicographer's:

[1] By parity of reasoning, it might also be maintained that the lexicographer is doubly compromised since a necessary condition for two forms to have the same meaning is that each has a meaning, i.e., each is significant.

[2] (1951:51). Quine shows no awareness here of the operational test for phonemic distinctness proposed in Harris (1951:32-33), although in later writings he alludes to it, however inaccurately, as implicit in the definition of the phoneme he proposes in terms of "stimulus meaning"; see (1969c:329-30), (1972:450) and cf. (1979:130) and (1981:44-45). On the limitations of the paired utterance test, see §3 below. Quine does refer here to Bühler's suggestions for a purely acoustical definition of phonemes, though noting that "there are abundant reasons to suspect that neither this oversimplified account nor anything remotely resembling it can possibly provide an adequate definition of the phoneme; and phonologists have not neglected to adduce such reasons." The difficulties in achieving an acoustical definition of the phoneme are more recently reviewed in Liberman and Cooper (1972) who observe: "The segmentation of the acoustic signal does not correspond to the phoneme segments; the acoustic cues for particular phonemes are not, in general, the same in different contexts; and the most important cues are sometimes among the least prominent parts of the acoustic signal (331)."

> The invidious use of the word 'formal', to favor grammar as against lexicography, is thus misleading. Both the lexicographer and the grammarian would simply list the membership of the respective classes in which they are interested, were it not for the vastness, the infinitude even, of the numbers involved. 1

Another parallel is that the class that each can be thought of as attempting to formally reconstruct can itself only be antecedently characterized in the unsavory idiom of meaning, of 'significance' and 'synonymy':

> Just as the grammarian needs over and above his formal constructions a prior notion of significant sequence for the setting of his problem, so the lexicographer needs a prior notion of synonymy for the setting of his. 2

The required appeal to notions of meaning in characterizing what both the grammarian and the lexicographer are about, shows that they "draw equally on our heritage from the old notion of meaning". 3

At this point, however, the parallelism comes to an end. For although the statement of the task of the grammarian includes a compulsory reference to 'significant sequence', nonetheless this notion is

> describable without appeal to meanings as such, as denoting any sequence which could be uttered in the society under consideration without reactions suggesting bizarreness of idiom. 4

That is, it is claimed that the meaningful sequences of phonemes can demarcated by the fact of their having clear correlates in the observable

[1] (1951:59).

[2] ibid. Quine's use of 'formal' seems ambiguous between notions of concatenation theory (though he nowhere defines the central notion of "linguistic form") and the sense given as "purely formal, that is, free of semantics" (52). Later he elaborates upon the first sense (1969d:328):"The syntactician's product is...a formal demarcation. By this I mean that it can be couched in a notation consisting only of names of phonemes, a sign of concatenation, and the notations of logic."

[3] (1951:60).

[4] (1951:54).

behavior of language users. In effect, this is to say that the class K of significant phoneme sequences is extensionally identical to the class of acceptable phoneme sequences. Now the "basic point of view" which Quine adopts has it that "the class K is objectively determinate before the grammatical research is begun";[1] the grammarian's task is that of reproducing formally (i.e., in non-semantic terms) and recursively, this class, of defining necessary and sufficient conditions for membership in this class. The objective predetermination of K is a necessary requirement to ensure that the grammarian's task is an empirical and objective one.[2] Surely, however, this assumption seems a rather strong one, for how can K be considered "objectively predetermined" in advance of grammatical research? It appears either that one must assume K is predetermined by considerations of meaning (in which case the grammarian's formal reconstruction of K is reliant on meaning for its objectivity) or K is to be predetermined in the purely behavioral terms of something like the "bizarreness reactions" the grammarian is to use in formally reproducing K, a tact which is viciously circular.[3] However, since Quine will later attempt to amend this flaw in characterizing the grammarian (see below), we will refrain from pursuing the matter further here.

But there is also a large hurdle to be overcome in Quine's attempted methodological assimilation of a semantic property ('significance') to

[1] (1951:51).

[2] (1951:52):"(The grammarian) is an empirical scientist, and his result will be right or wrong according as he reproduces that objectively predetermined class K or some other."

[3] A similar point is made by Swanson (1969) in criticizing generative grammar:"There is something curiously circular about a program that attempts to construct a grammar on the basis of intuitions informed by that very grammar (131)."

a behavioral one (eliciting "bizarreness reactions")-- namely, the class K is stipulated to contain all significant sequences, not only those observed or even observable, but all which could occur:

> What are wanted as significant sequences include not just those uttered but also those which could be uttered without reactions suggesting bizarreness of idiom. The joker here is 'could'; we cannot substitute 'will'. The significant sequences, being subject to no length limit, are infinite in variety;.... [1]

Adepts will recognize this is the problem of 'taking the inductive step' from a closed corpus to the language as a whole which Chomsky has, on several occasions, recalled so troubled him as a student and young worker in structural linguistics.[2] Others may see here the setting of a "language acquisition device", the formal analogue of the child language learner in the situation of the "poverty of the stimulus".[3] What is wanted is a characterization of what can be in the language on the basis of what is observed to be in the language. According to Quine, a solution can be found by appeal to the notion of simplicity of theory:

> I expect we must leave the 'could' unreduced. It has some operational import, indeed, but only in a partial way. It does require our grammarian to bring into his formal reconstruction of K all of the actually observed cases,.... Now what more does the 'could' cover? What is the rationale behind that infinite additional membership of K, over and above the finite part...? ...Our basis for saying what 'could' be generally consists, I suggest, in what is plus simplicity of the laws whereby we describe and extrapolate what is. I see no more objective way of construing the conditio irrealis. [4]

[1] (1951:53).

[2] E.g., Chomsky (1975a:30-31), (1979b:115 and 131); Mehta (1971:65).

[3] The "language acquisition device" analogue of the child language learner is introduced in Chomsky (1960); on the origin and subsequent role played by this concept in generative grammar, see Levelt (1975).

[4] (1951:53-54).

In PML, Quine does not explicate or further indicate just how considerations of simplicity are to aid the grammarian in reconstructing K. However, some suggestive remarks concerning simplicity are made in his "On What There Is" (1948), reprinted in the volume in which PML appears. In this essay Quine argues that choice of ontology (conceptual scheme) is

> similar in principle to our acceptance of a scientific theory, say a system of physics: we adopt, at least insofar as we are reasonable, the simplest conceptual scheme into which the disordered fragments of raw experience can be fitted and arranged. 1

Despite the similarity between the respective use of simplicity considerations in choice of theory and in choice of conceptual scheme, it appears to be Quine's intent to single out the employment of simplicity in the latter endeavor as inherently non-determinative. For he subsequently remarks:

> But simplicity, as a guiding principle in constructing conceptual schemes is not a clear and unambiguous idea; and it is quite capable of presenting a double or multiple standard. 2

As an illustration of his point, Quine offers the example of the counterposing phenomenalistic and physicalistic conceptual schemes. Simplicity here is of little avail in determining which of these schemes is superior. The implication is that, as opposed to simplicity as a criterion of theory choice (e.g., choice of a "system of physics"), it is perhaps not possible to invoke simplicity as a criterion for

[1] (1948:16). The context of these remarks is that of Quine's on-going dialectic with Carnap, in particular, with Carnapian toleration of opposing linguistic frameworks; see also Chapter 5 § 1 below. Davidson (1974) has rebuked the assumption of "conceptual schemes" as a "third dogma of empiricism".

[2] (1948:17).

selecting among rival ontologies:

> Which should prevail? Each has its own advantages; each has its special simplicity in its own way. Each, I suggest, deserves to be developed. Each may be said, indeed, to be the more fundamental, though in different senses: the one is epistemologically, the other physically, fundamental. [1]

Simplicity is a conceptual scheme-internal criterion; it provides a means of choosing from among theories framed within a given conceptual scheme or metatheory, whereas it is not an objective or non-conventional criterion on which to base a choice from among opposing metatheories, conceptual schemes or ontologies.[2]

The lexicographer, as well, "comes also to turn increasingly to that last refuge of all scientists, the appeal to internal simplicity of his growing system". [3] But, Quine argues, the work of the lexicographer -- unlike that of the grammarian -- cannot even be described without invoking one of the notions of the theory of meaning, viz., that of synonymy. Though the lexicographer parallels the grammarian in ostensibly being concerned with linguistic forms, his particular task is the correlation of forms that are synonymous. The problem is: How can the lexicographer legitimately speak of synonymy given the difficulties, previously surveyed in "Two Dogmas of Empiricism", encountered in trying to define 'synonymy'? [4]

[1] (1948:17).

[2] Similarly, Goodman's (1951:60ff)"formal simplicity of bases" is an explicitly system-internal criterion; see the discussion in §3 below.

[3] (1951:63).

[4] "Two Dogmas" is the immediately preceding essay to PML in Quine (1953). The discussion there undertook to show that the question of what is preserved under substitution of synonyms has no ready answer: interchangeability salva veritate is too weak for synonymy in purely extensional languages, while for non-extensional languages, specifying what is preserved under substitution of synonyms is held to circularly involve the notion of synonymy.

A provisional and pragmatic resolution to the practical task of correlating synonyms might lie in turning attention away from thinking of synonymy solely in terms of interchangeability of short forms ('words')--since the question of salva qua? cannot be answered -- to a "retreat to longer segments of discourse". Thus

> We may continue to characterize the lexicographer's domain squarely as synonymy, but only by recognizing synonymy as primarily a relation of sufficiently long segments of discourse. [1]

Still, even as amended by consideration only of forms which "are long enough to be pretty clean-cut about their synonymy connections", the notion of synonymy remains intractable. For it remains doubtful that it makes sense, even in principle, to think of pairs of linguistic forms related by a relation of synonymy. [2]

We saw above that the objectivity of the grammarian's enterprise is assumed to require that the class K of significant sequences be somehow predetermined: this amounts -- we observed above --either to a vicious circularity or to a hidden reliance on meaning. The obviously corresponding move for the lexicographer is to assume a predetermined class of synonymously paired expressions, call it M, between, say, English and Kalaba. The lexicographer's efforts could then be objectively assessed by ascertaining the success with which he formally reproduces M, correlating utterances with situations of utterance, by retreating to longer segments of discourse, etc. What prevents making this move, thus completing the parallel with the grammarian? Quine's answer is:

[1] (1951:58).

[2] (1951:60):"...I want to stress what a baffling problem this remaining problem of synonymy, even relatively clean-cut and well-behaved synonymy,is."

linguistic relativity, the notion that "there is no separating language from the rest of the world, at least as conceived by the speaker" -- a notion for which Quine cites the authority of Cassirer and Whorf. An objective predetermination of M cannot be legitimately assumed since

> It is not clear even in principle that it makes sense to think of words and syntax as varying from language to language while the content stays fixed; yet precisely this fiction is involved in speaking of synonymy. [1]

Unlike the situation in which the grammarian is involved, for the lexicographer there is no fact of the matter, nothing to be right or wrong about:

> In the case of the lexicon, pending some definition of synonymy, we have no statement of the problem; we have nothing for the lexicographer to be right or wrong about. [2]

The indeterminism lurking in the lexicographer's path arises from Quine's contention that -- comparable to the grammarian -- there is no "objectively predetermined" pairing of synonyms, even in principle. And, though Quine does not explicitly draw this inference, in as much as the grammarian is wedded to a definition of 'phoneme' which invokes synonymy, indeterminism lurks here as well. And in contrast to the celebrated argument for indeterminacy presented in Word and Object, the argument in PML does not proceed from the assertion of the inability of the native's observable dispositions to verbal behavior -- the only admissible class of evidence -- to uniquely specify synonymy pairings

[1] (1951:61).

[2] (1951:63).

(a.k.a. a "translation manual"). According to PML, what counts as an objective standard of lexicographic success -- a canonical list of synonym pairs -- is probably (i.e., pending a definition of synonymy) a figment of the lexicographic imagination. It seems to follow that either lexicography is impossible (a position refuted by the fact that lexicography, for better and for worse, exists), or that lexicographers are deceiving themselves about the scientific standing of their enterprise, since there is nothing to be <u>really</u> right or wrong about.

It goes beyond the confines of our discussion to show in detail how the PML argument for indeterminacy is refurbished in the famous chapter two of <u>Word and Object</u> (WO). Yet we may call attention to three points of contact. The first is that the lexicographer is transposed into a "field linguist" doing "radical translation" between a language hitherto completely unknown and English. This change signals a heightened concern that 'hidden' (i.e., not identifiable by observable "dispositions to overt behavior", hence, "subjective") considerations of meaning do not intrude or play a role in framing the field linguist's "analytical hypotheses" pairing expressions of the jungle language with expressions of English.[1]

[1] Of what avail to the linguist is familiarity with the native tongue? Clearly, a good deal. Quine's <u>Gedankenexperiment</u> of "radical translation" not only postulates no a priori familiarity with the investigated language, but seems also to proscribe that the linguist acquires any ensuing understanding as may be reasonably expected in an actual situation. To be sure, the field linguist is allowed to adopt conventions of simplicity, but these must be sharply distinguished as not involving any knowledge of meaning in order to preserve the integrity of the central theoretical notion of "stimulus meaning". This view of the field linguist (akin to that of a "formal learning device") is certainly unrealistic (Cf. Hockett (1955:147) who notes that it is necessary "the analyst...to some extent learn the language with which he is working"). How reasonable it is may be gauged by the pertinence of Quine's rational reconstruction to the actual practice of writing grammars.

Second, the task of the grammarian is completely put aside in WO and the problematic status of the phoneme is not even mentioned.[1] Indeterminacy is adduced, as in the earlier paper, only regarding considerations of 'synonymy' and not over those of 'significance' (or well-formedness). Finally, the vague suggestion in PML that an account of synonymy might be attempted in behavioral terms, correlating sameness of utterance with sameness of situation of utterance, is in WO developed into the central theoretical notion of "stimulus meaning". The stimulus meaning of a sentence is identified with the set of stimulations of a native speaker's nerve endings which would either prompt assent (sameness of stimulus meaning, i.e., "stimulus synonymy") or dissent to the linguist's utterance of the sentence in question.[2] The doctrine of indeterminacy therefore has two parts: that the totality of dispositions to speech behavior, as these can be assessed by establishing correlations between assent and dissent reactions to utterances and the native speaker's sensory stimulations, in principle do not suffice to establish a uniquely correct translation between English and the jungle language (or, since indeterminacy is held to arise in the "home" language, to determine uniquely correct pairings of synonyms among expressions

[1] The truncated discussion of the phoneme (89-90) is rather surprising, alluding to none of the problems broached in PML.

[2] (1960:34): "The stimulus meaning of a sentence for a subject sums up his dispositions to assent to or dissent from the sentence in response to present stimulation." As Quine later acknowledges (1969b:312), the notion of stimulus meaning requires the prior notions of assent and dissent to be behaviorally specified; however, somewhat puzzlingly, he then urges that the behavioral identification of assent and dissent introduces an "initial indeterminacy" which "carries over into the identification of the stimulus meanings." No argument is provided to show that the behavioral identification of assent and dissent is indeterminate, as opposed to merely underdetermined, by observable response.

of a single language), and secondly, that the totality of speech dispositions, as "summed up" in terms of stimulus meanings, is the sole source of admissible evidence concerning translational correctness.[1]

In fact, it is only some years later, in the course of an exchange with Chomsky, that Quine returns to a consideration of "the grammarian's classical task".[2] Speaking now of the "well-formed" sequences, rather than of the "significant" ones, the grammarian, according to Quine, faces the problem of "demarcating, recursively and in formal (i.e., non-semantic) terms, the infinite totality of the well-formed strings of phonemes of the chosen language".[3] But putting the matter in quite this way, Quine now allows, illegitimately "presupposes some prior behavioral standard of what, in general to aspire to include under the head of well-formed strings for a given community." That is, the "objective predetermination" of the class K can no longer be invoked; lacking such, the grammarian would appear to be in the same boat as the lexicographer, without an objective criterion or standard of success. As before, behavioral data provide some headway for a corpus of test sentences but, as before, the problem arises of how behavioral data can be the criterion for the well-formedness of an <u>infinite</u> set of sentences.

[1] (1960:72)."There can be no doubt that rival systems of analytical hypotheses can fit the totality of speech behavior to perfection, and can fit the totality of dispositions to speech behavior as well, and still specify mutually incompatible translations of countless sentences insusceptible of independent control." Also, "stimulus meaning...may be properly looked upon...as the objective reality that the linguist has to probe when he undertakes radical translation(39)."

[2] Quine (1972).

[3] (1972:445).

The methodology urged on the grammarian in PML is now rejected as unworkable [1] since it does not provide allowance for obviously well-formed sequences such as Carnap's This stone is thinking about Vienna [2] (or Chomsky's Colorless green ideas sleep furiously, see below)which do or may evoke reactions of bizarreness of idiom from native speaker informants. Hence Quine no longer speaks of the grammarian as concerned with the meaningful ('significant') sequences, but of his concern with those that are well-formed. This requires "a more realistic characterization of the grammarian's classical task", a "somewhat melancholy version" which is "an open-ended one".[3] Since there is "no prior behavioral citerion for well-formedness", the grammarian has only

> some sufficient behavioral conditions. Strings heard from natives count as well-formed, at least provisionally. So do sentences which, when tried on an informant, elicit casual and unbewildered responses. What I then picture the grammarian as doing is to devise as simple and formal recursion as he can which takes in all these comfortably well-formed strings and excludes all strings that would bring really excessive bizarreness reactions. He rounds out and rounds off his data. Sometimes of course he will even reject a heard string as ill-formed, thus rejecting a datum, if he can appreciably simplify his system in so doing; but it would be regretable to do much of this. 4

[1] (1972:445):"Passive observation of chance utterances is a beginning. The grammarian can extrapolate this corpus by analogical construction, and he can test these conjectures on an informant to see if they elicit a manifestation of bewilderment. But of course the grammarian settles for no such criterion."

[2] Carnap (1937:5):"This stone is now thinking about Vienna."

[3] (1972:445-446).

[4] ibid.

We are perhaps entitled to wonder just how different this account is from the earlier one Quine intends to modify, for it turns upon spelling out in some non-question begging terms the operational significance of "sufficient behavioral conditions" and "really excessive bizarreness reactions". But we can readily accept Quine's emendation that it is unwarranted to suppose the class K of well-formed sequences is "objectively predetermined". Thus Quine can attempt to restore the parallelism with the lexicographer, arguing that there is no sense to be made of speaking of a uniquely correct grammatical theory. This argument (directed against Chomsky) proceeds from a consideration of the distinction between the notions of "fitting" and "guiding", i.e., between a rule or rule system correctly describing (fitting) some domain of behavior and the behavior in question being guided by these rules. Of course, behavior can fit or be in conformity with rules or rule systems of ostensibly very different kinds, e.g., a dynamical system may be alternatively and equivalently characterized by either the equations of Lagrange or those of Hamilton, the difference being merely a matter of convenience for the purpose at hand.[1] In speaking of "internalized grammars", however, Chomsky has it that the grammarian seeks rules which are uniquely correct because they are alleged to guide or otherwise be "involved in" the production of the relevant

[1] E.g., Yourgrau and Mandelstam (1968:43):"For the actual solution of problems, the equations of Lagrange are more convenient than those of Hamilton, since the first step in integrating Hamilton's equations would amount to reducing their number by half, an operation which would lead us back to our original Lagrange equations. In purely theoretical inquiries, on the other hand, Hamilton's equations are often more useful."

linguistic behavior.[1] Thus the difference between 'fitting' and 'guiding' seems to be one between correct and uniquely correct grammatical rules.

Quine poses the fitting and guiding issue in the form of asking whether there is a principled and non-conventional (i.e., not having to do with "simplicity" or "convenience" [2]) means of choosing among extensionally equivalent grammars. Now two grammars are extensionally equivalent iff both "determine, recursively, the same infinite set of well-formed (e.g.) English sentences". [3] In this regard, both grammars fit the behavior of all native speakers of English; in this lies the criterion of their correctness. But, we may well question whether Quine is now entitled to phrase the problem in quite these terms. After all, he has made a lavish point of establishing there is "no prior behavioral criterion for well-formedness", yet stipulating extensional equivalence of grammars over the admittedly infinite set of well-formed English sentences would appear to involve just such an assumption. Moreover, Quine speaks of the infinite set of well-formed English sentences, whereas it is quite unclear that the set of sentences of any language is well-defined by acceptability or behavioral criteria: what is well-defined are the sentences characterized ('generated','accepted') by a particular grammar. [4] In thus ~~situating~~ the problem of choice ~~from~~

[1] See the discussion at the end of Chapter 4 §1 and §3 passim.

[2] (1972:451).

[3] (1972:442).

[4] See the discussion in Chapter 4 §2.

among empirically equivalent grammars as ~~that~~ of choosing from extensionally equivalent grammars, Quine appears to be smuggling in controversial assumptions that amount to stacking the deck against his opponent.[1]

But further, the behavior which the two extensionally equivalent grammars "fit" is behavior of a particular kind i.e., some subset of "the native's dispositions to behave in observable ways in observable circumstances".[2] The choice between the two, if there is one to be made, is therefore to be based on a difference between the respective dispositions to behavior to which the two theories are linked. Such a difference, ex hypothesi, is not to be found among dispositions attesting to well-formedness since in this consists the claim of extensional equivalence. So, choice among extensionally equivalent grammars must be based upon differences in dispositions to other sorts of behavior. It remains to be determined just what is or are the other kinds of behavior in which the grammarian seeks to ground his choice. Quine notes:

> It could be a question of dispositions to make or accept certain transformations and not others; or certain inferences and not others. 3

[1] A reasonable requirement for any grammar is that it parsimoniously (i.e., avoiding "class cleavage") account for the range of distribution of an element, showing the "the same" linguistic form can occur in apparently different grammatical environments. As we show in Chapter 5 §3, a transformational treatment may involve extending the set of sentences of the language to include "regularized" or "regularizing" sentences which can not be considered as attested sentences of the language but must be considered "grammatically possible". The claim of extensional equivalence seems therefore vastly misleading.

[2] (1972:444).

[3] ibid.

41.

However, Quine does not pursue this suggestion, preferring instead to consider an "unimaginative suggestion" for resolving the question of non-uniqueness: "ask the natives".[1] For it is often supposed that native responses can provide a principled means of choosing between alternative grammatical proposals. Which responses are these? This Quine does not say, save to remind us again that "it could be a matter of dispositions to make or accept certain transformations or inferences".[2] But the notion of asking the natives provides an occasion to "take off on a tangent, leaving at last this whole question of a native bias toward one of two extensionally equivalent grammars". This tangent is the familiar attack on synonymy:

> The unimaginative suggestion was: ask the natives. The same question and the same warped circle or one very much like it, are encountered from time to time in semantics. People like me challenge the notion of synonymy and ask for a criterion. What is synonymy? How do you tell whether two expressions are synonymous? Ask the natives. 3

We have been led to expect an argument to the effect that the grammarian, like the lexicographer - field linguist, runs up against the wall of "no fact of the matter". But no such argument is forthcoming. At the crucial juncture where it is incumbent upon Quine to attempt to show that the choice among competing grammars is <u>indeterminate</u> with respect to some specified class of relevant behavioral evidence, just as he provided such an argument in WO that the permissible behavioral evidence ("stimulus

[1] (1972:448)

[2] <u>ibid</u>.

[3] <u>ibid</u>.

meaning") shows there is "no fact of the matter" regarding choice among rival and incompatible "analytical hypotheses" and "translation manuals", Quine is unwilling to even be definite as to what this class of relevant behavioral evidence may be. And not specifying such a class, no argument is presented that, with respect to it, there is "no fact of the matter" on which to base a choice among otherwise equivalent grammars. Quine has simply failed to provide an argument that indeterminacy afflicts the grammarian. ~~And~~ he has neither proposed that the characterization of the grammarian's task needs to be revised to include a concern with synonymy where, to be sure, he has an indeterminacy argument lying in wait. The most that can be (charitably, in the light of the tendentious assumptions in Quine's argument, noted above) gathered from Quine's account is that choice among competing grammars is underdetermined with respect to one class of evidence, the native's dispositions attesting to the well-formedness of particular strings. But indeterminacy is, Quine himself has urged, additional and not reducible to underdetermination of theory by evidence. [1] In order to establish the existence of indeterminacy, Quine requires a further step, analogous to that made in WO in the case of the field linguist's key notion of "stimulus meaning", namely, that there is no other legitimate objective means of evidencing the notion (here, well-formedness, in WO, synonymy) in question. Only then would Quine have an argument that there is "no fact of the matter" regarding choice of grammars.

[1] E.g.,(1960:75):"May we conclude that translational synonymy at its worst is no worse off than truth in physics? To be thus reassured is to misjudge the parallel." See especially his (1970a), discussed briefly in Chapter 5 §1 below.

Though Quine has not shown how indeterminacy afflicts the grammarian in his task of demarcating the well-formed sequences of a language, the prospects for linguistics are nonetheless still viewed dimly since it is held that there is, in principle, "no fact of the matter" regarding the other fundamental task of linguistics, that of determining whether two expressions are synonyms, that is, have the same meaning. For Quine requires that the only objective evidence (hence the only admissible evidence) for correctness in positing synonym pairings is observable behavioral evidence of an explicit and highly restricted kind, of dispositions to assent to or dissent from test utterances in response to present sensory stimulation. And even all possible evidence of this kind, Quine maintains, does and cannot suffice to establish uniquely correct translations or synonym pairings. Hence indeterminacy of translation.

We may marvel at just how tightly woven is the net Quine has cast in setting up indeterminacy with respect to all possible (admissible) observational evidence. Overlooking for the moment[1] the by-now familiar objections which may be raised as to the legitimacy of restricting evidence in the manner Quine demands, another, more conceptual, objection may be raised. For Quine's indeterminacy doctrine reveals a tendency, especially perceptible in the earliest version of the argument in PML where the central notion of "stimulus meaning" is not to be found, to be taken as following merely from the denial, based upon linguistic

[1] See the concluding paragraph of this chapter and Chapter 5 §1.

relativity, of essentialist views of meaning held by "mentalist semantics". This argument seems to hold that the fact of linguistic relativity refutes or casts doubt upon a lingering and uncritical notion, institutionalized almost beyond awareness in our everyday talk about meaning and deriving from a naively essentialist metaphysics of common sense, that there is a uniquely correct pairing of expressions according to their common meaning. But from this, Quine concludes not only that there is no unique pairing of expressions but as well that there is "no fact of the matter" regarding such pairings. This certainly is to conclude too much. There seems to be no reason to single out linguistics on the grounds that there uniquely correct theories are not, in principle, attainable, especially since no case has been made (nor presumably would Quine care to make one) that uniquely correct theories can be attained anywhere in science.[1] "Indeterminacy" or "no fact of the matter" does not follow from the non-existence or alleged non-existence of uniquely correct theories. To be sure, if there is a residual belief in the existence of uniquely correct theories, it is undoubtedly laced with vestiges of essentialism, an essentialism from which the 'advanced' sciences have, slowly and in fits and starts, labored over many centuries to extricate themselves. The considerable success they have thus achieved in these efforts indeed

[1] This is not to say that, though being good fallibilists, scientists may not believe even in the truth of a particular theory. But presumably few would care to be identified with the claim that a theory is uniquely true in the timeless sense characteristic of essentialism.

comprises, in large measure, the grounds for their standing as 'advanced' sciences. Yet ironically, to promulgate a doctrine of indeterminacy in linguistics as a caution against age-old essentialist views of meaning, perhaps falls prey to the very essentialism it purports to eradicate. If "determinacy" and "having a fact of the matter" in linguistics are to be understood, per impossible, only as construed by the "myth of the museum", the doctrine of indeterminacy amounts to nothing more than a rejection of the essentialist myth of a uniquely correct theory. But if taken as demonstrating that in linguistics there is "no fact of the matter" in the sense in which there is a "fact of the matter" in physical theories about, say, the electron, then the doctrine of indeterminacy is curiously tantamount to assessing linguistics from the forbidden perspective of essentialism. [1] Another, and preferable, avenue of attack on "mentalist semantics" is to abandon talk of indeterminacy altogether, recognizing that an in toto rejection of essentialism requires a thorough housecleaning of the conceptual terrain it has so stubbornly and lastingly occupied. And this means severing the notion of 'determinacy' or 'fact of the matter' from their tired essentialist mooring. [2]

[1] Michael Gottfried has reminded me that Putnam (1974a) presents an argument which eventuates in a similar conclusion, that Quine's (in at least one sense of 'Quine') indeterminacy argument, being conventionalist in character, has the form of an essentialist (in particular, "negative essentialist" (227)) claim.

[2] See Chapter 5 §1.

3.3 LSLT: A Metagrammatical Approach to the Justification of Formal Grammars. The central concern and primary motivation for LSLT lies in considering the problem of theory (i.e., grammar) choice in linguistics. For there are particular difficulties which linguistics must face in justifying grammars. These difficulties are of two kinds. On the one hand, grammars are required to have no semantic terms among their primitives; in this sense, they are to be formal theories. On the other hand, grammars are theories of "intuitions of linguistic form" (which are mistakenly thought of as semantic). Now a theory is justified by relating it to data and the empirical data of grammars are the native speaker's "intuitions of linguistic form". A grammar is accounted empirically adequate if it is in accord with these data. As such, it may be said to meet the criterion of "external adequacy".[1] But because of "the nature of the data"[2] for grammars, which have implications extending beyond any given corpus of sentences, a problem appears in selecting a particular grammar from among others, each equally empirically adequate:

> We...face the problem of choosing among the vast number of different grammars, each giving a different structure, and all meeting these vague and incomplete external criteria.[3]

The special problem that the linguist faces in justifying a grammar is, then, that there is ostensibly no non-conventional basis upon which to choose one of these "externally adequate" grammars as presenting the

[1] (1955a:I-10/11). The requirement of "external adequacy" holds that "the generated sentences be acceptable to the native speaker, that the elements of the language as constructed in the grammar have certain observable correlates, etc. (I-11)."

[2] (I-10); see further below.

[3] (I-11).

structure of a given language. It is precisely this problem that LSLT proposes to address:

> This is the facet of the problem of justification which is most interesting at the present stage of linguistic research, and to which we will devote out primary attention in this study. 1

However, since "we can scarcely describe a language at all, except in terms of some previously assumed theory of linguistic structure",[2] a conceptual solution to the problem of choosing a particular grammar can be found in the requirement that empirically equivalent grammars be comparable according to "internal" criteria. This is to require that they be couched in the terms of a general theory of language structure, a metagrammar. The grammarian is therefore necessarily engaged in a two-fold program of linguistic research, of constructing a general theory and of constructing particular grammars.[3] These goals are interdependent and one cannot be pursued without reference to the other, though the apparent circularity of this characterization of the goals of linguistic theory is not vicious. At any stage of research, a non-circular account can be given, presenting the general theory as an abstract formal system and showing how each particular grammar is an exemplification of the general theory.[4] The adequacy of the general theory depends on the demonstration that all the grammars to which it leads are empirically adequate; thus an "indispensible aspect" of validating a grammar of a particular language is the construction,

[1] (1955a:I-11).

[2] (I-7/8).

[3] (I-8) and (I-40).

[4] (I-8).

in terms of a common general theory, of empirically adequate grammars in other languages.[1] Accordingly, there are two factors necessarily involved in the validation of a grammar of a language, an "external" and an "internal" aspect, and it is the second which plays the decisive role in selecting a correct grammar:

> (T)here are two factors involved in determining the validity of a grammar, the necessity to meet the external conditions of adequacy, and to conform to the general theory. The first factor cannot be eliminated, or there are no restraints whatsoever on grammar construction; the simplest grammar for L will simply identify a grammatical sentence in L as any phone sequence. Elimination of the second factor leaves us free to choose at will among a vast number of mutually conflicting grammars. 2

The problem of justification is therefore intimately tied up with the relation of a grammar to its metagrammar.

There are, however, various ways in which this relationship can be construed. For it may be required that the general theory provide "a practical means for literally constructing the (particular) grammar out of the raw data". This is a requirement (see Chapter 2 §4) that the grammar of a particular language be mechanically derivable from the application of the general theory to a sufficiently large corpus, without any knowledge of the language on the part of the linguist, and indeed, even without any ingenuity:

> Let us call such a theory procedural. Thus given a sufficient corpus, a procedural theory will lead us directly to a grammatical description of the language, requiring, in principle, no ingenuity or intuition on the part of the linguist. A procedural theory gives what might be called a 'practical discovery procedure' for grammars. 3

[1] (1955a:I-11).

[2] (I-12).

[3] (I-9).

Or, the relation of the general theory to a particular grammar may be conceived along somewhat weaker lines than that of a literal discovery procedure. For example, it may only be required

> that given a grammar, the theory must provide a practical mechanical way of validating it, i.e., of showing that it is in fact the best grammar of the language. 1

As an example of this latter view, Chomsky cites Harris' Methods in Structural Linguistics. [2] There is yet a still weaker construal of the relation between the general theory and a particular grammar, namely, that

> the (general) theory provide a method of evaluating any proposed grammar, so that, given two proposed grammars, there would be a practical and mechanical way for determining which is the better of the two. 3

This last is the approach to be followed in LSLT. It is, Chomsky observes, though the weakest of the three approaches to justification surveyed, still far too strong a requirement to impose upon theories in natural science. But such is required in linguistics, "given the nature of the data"

> (T)his last is still a strong requirement, much stronger than those imposed in natural science, where no one would seriously consider the possibility of a general, practical, mechanical method for deciding between two theories, each compatible with the available evidence. But in linguistics, given the nature of the data, it seems natural that our sights should be set at least that high. 4

[1] (1955a:I-9).

[2] ibid.,"This would seem to be the proper interpretation for the kind of theory that Harris is interested in building in his Methods of(sic) Structural Linguistics". Thus LSLT presents a different assessment of this work that that in the celebrated argument against "mechanical discovery procedures" in Syntactic Structures; see Chapter 2 §4.

[3] (I-10).

[4] ibid.

What are the practical implications of this view of the relationship between the general theory of language structure and particular grammars for the justification of grammars? Most prominently, it implies that a purely formal statement of the observable distribution of elements in a corpus does not suffice to justifiy setting up these particular elements in the grammar and not others. Any disparate set of linguistic forms can be gathered under a particular heading by listing and "listing is as precise and formal procedure as we can find". [1] The statement that an element has such and such a formal property (distribution) is legitimate

> but not...as an objective means for setting up these elements in the first place, or as a significant and objective formal means of demonstrating that these and not other elements should be constructed. 2

Every element will have some formal property, but to define the element in terms of a particular formal property fails to address the issue of why this property was chosen as criterial. [3] The justification of a grammar requires more than that its elements are constructed by formal (i.e., distributional) procedures and consequently, merely distributional procedures "give no support to the program of developing an objective and operational linguistics". [4] Accordingly, there is but one way in which

[1] (1955a:I-13/4)

[2] (I-14).

[3] (I-15).

[4] ibid.

the characterization of elements of a grammar can meet the criteria posed for its validation. And this is to ask:

> in accordance with what general theory are the elements in question set up? Is this theory a rigorously constructed one, framed in terms of clear and applicable notions? Can this theory be applied to other languages giving satisfactory results? 1

Noteworthy in this earlier version of the argument against so-called "mechanical discovery procedures" (MDPs), as opposed to the more familiar rendition in Syntactic Structures, is a significant shift in emphasis underlying the charge that MDPs are too strong a requirement to place on theory (grammar) construction. We may recall from Chapter 2 §4 that in Syntactic Structures Chomsky argued that MDPs were unworkable, that they involve more and more complex analytic procedures which fail to answer "many important questions about the nature of language structure".[2] But here the argument has a focus instead on the insufficiency of distributional procedures to provide a basis for the justification of grammars. With these remarks a new and, to our knowledge, unanticipated perspective enters the discussion of linguistic metatheory, that the justification of grammars fundamentally requires that the construction of grammars of particular languages be constrained by an explicitly formulated metagrammar or general theory of language structure, and not merely by a particular methodology[3] or the (implicit) claim of the general applicability of a set of procedures[4] to arbitrary languages. The rationale for the new

[1] (1955a:I-15).

[2] (1957a:53).

[3] E.g., behaviorism or operationalism.

[4] As in Harris (1951a).

requirement is clearly stated: it is forced upon the grammarian by "the nature of the data" he seeks to account for, the open-ended ability of speakers to recognize 'new' word sequences as belonging or not to the language in a manner which is reasonably uniform from speaker to speaker. As we saw in Chapter 2 §6 above, the issue of the predictiveness of grammars was not (as much subsequent historiography of linguistics has it) ~~exactly~~ at all(?) absent from the discussions of linguistic metatheory of structural and anthropological linguistics. And other linguists had previously articulated a goal for linguistic theory to be an axiomatic grammar of the sentences of a language [1] or had written of "operational parallels" between the linguist's task in constructing a grammar of a language and the ability of speakers to 'project' from their previous linguistic experience to new utterances. The decisive point posed in the discussion here is that such grammars cannot be considered adequately justified simply on the grounds that their elements are set up according to distributional criteria. For the question is not faced of why these elements, and not others which might be equally justified on distributional grounds, were chosen.[2] Reasons for proceeding in one way rather than another must be recognized and explicitly stated; the testability of grammars (as might be determined by submitting 'new' utterances to analysis) inherently depends on this. The argument against MDPs in Chomsky (1957a),[3] which attacks

[1] See also Chapter 5 §3 below.

[2] Of course, in a program for which semantic notions are "irrelevant", there is no scope allowed for the view (Harris (1951a:188); see Chapter 2 §3 above) that distributional regularities are sought which establish "elements which will correlate with meanings".

[3] Chapter 2 §4.

a straw man, is but an abridged, even misleading, replay of this prima facie more persuasive argument for a new conception of metagrammatical justification of grammars. Although it is easy with hindsight to view the demand for an explicit metagrammar in the justification of particular grammars as presaging what is later to be termed "universal grammar", it is clear in the discussion here and throughout LSLT that the notion of a metagrammar is raised solely in this context of justification.[1]

In LSLT Chomsky conceives a general theory of language structure, as, first of all, specifying a common structural form to grammars constructed in accordance with it.[2] Beyond this, it is the assigned function of a metagrammar to enable a choice to be made from among candidate grammars so constructed and which are empirically equivalent over some range of data. To do this, the basis for choice among grammars must be built into the actual definition of elements of the various grammars.[3] And, in the opening remarks of a chapter entitled "Simplicity and the Form of Grammars" a broader theme, notably associated with Goodman, is struck: the notion of 'simplicity' is requisite

[1] E.g., (1955a:I-18):...(T)he problem of justification and that of constructing a general theory of linguistic structure are, in part at least, essentially the same."

[2] (I-18):"The general theory will ultimately assume the form of a system of definitions, in which 'phoneme','word', 'sentence', etc. are defined, and their general properties and interrelations specified."

[3] (III-71):"We want linguistic theory to enable us to choose among proposed grammars. Every consideration that is relevant to this choice must be built into linguistic theory, into the actual definition of linguistic elements."

to the very possibility of systemization.[1] Referring to the essays in Quine (1953) for a discussion of "the role of simplicity in setting up scientific theories" [2] Chomsky announces that the task to be achieved in a general theory of linguistic structure is that of "literally defining simplicity" for grammars, and, subsequently, to construct a mechanical ("effective") evaluation procedure for grammars in terms of the criterion of simplicity.

> In linguistic theory, where the material under investigation is relatively clear and limited, we may hope to carry out in an effective way the task of literally defining simplicity for the theories in question, namely grammars, and of setting up an effective evaluation procedure for these theories in terms of simplicity. 3

Actually defining simplicity for theories must be distinguished from the sense of 'simplicity' in which simplicity is an ideal

[1] (1955a:72/3):"It has been recognized of philosophical systems, and it is, I think, no less true of grammatical systems, that the motives behind the demand for economy are in many ways the same as those behind the demand that there be a system at all". As Chomsky notes, here he paraphrases from Goodman (1943) (cf. "The motives for seeking economy in the basis of a system are much the same as the motives for constructing the system itself (1943:107).") "where the reference is to a special sense of simplicity, namely, economy in the basis of primitives". It is, above all, Goodman (1951) that provides an impetus for the idea that the formal simplicity of a theory might be measured and thus serve as a criterion for theory choice; see the discussion of the "formal simplicity of bases -- simplicity, that is, only in so far as it is affected by those differences among predicates that are expressible with the use of the basic logical terms alone (in addition to the predicates themselves)" where methods of "measuring simplicity...of assigning to every basis a numeral indicating its degree of complexity" and judging between these methods are proposed (60 ff). Goodman is careful to point out, however, that "many other less measurable factors...enter into any choice of basis for a system"(85).

[2] (III-73 fn 1). In the published version of LSLT (1975), this passage (114: fn 2) refers to Quine (1953) "for recent discussion of the role of simplicity in the choice of scientific theories" (our emphasis). Recall from the discussion in §2 above that for Quine simplicity also functions as a systems- or conceptual scheme-internal criterion for theory choice.

[3] (III-83).

for any science" including linguistic theory, ~~and~~ which is a notion to be analyzed "in the general philosophy of science" and (2) the sense indicated by speaking of "simplicity of grammars" which "is a notion to be defined within linguistic theory." [1]

To define simplicity of grammars within general linguistic theory is to provide a metagrammatical scale of notation where symbols are weighted in terms of 'simplicity' conventions [2] and on which symbols an evaluation measure may be defined to select the notationally simplest of rival candidate grammars. This is to give "a general schematic account of the form of grammars" and "a general definition of simplicity for grammars of the proper form".[3]

on the symbols of which ??

[1] (1955a:III-83).

[2] For example, Chomsky suggests simplicity may be defined in terms of length of grammars, in terms of notation which permits the coalescence of similar grammatical statements as with the use of brackets(III-85):

$$\ldots \left\{ \begin{matrix} a_1 \\ a_2 \\ \cdot \\ \cdot \\ \cdot \\ a_n \end{matrix} \right\} \ldots$$

abbreviates the ordered set of statements

$$\begin{matrix} (i)\ldots a_1 \ldots \\ (ii)\ldots a_2 \ldots \\ \cdot \\ \cdot \\ \cdot \\ (n)\ldots a_n \ldots \end{matrix}$$

Simplicity may also be defined in terms of an ordering of rules of the form $\alpha \rightarrow \beta$ (III-84). The objective is to "define simplicity so that, in certain clear cases, simplest grammars are in fact the correct ones (III-81). Chomsky observes, however, that his proposals to define simplicity of grammars are only tentative since "the determination of correct notations will involve detailed study of the effects of various claims on actual grammars (III-83)". Similarly, he also notes that in his discussion, "we have not really stated an evaluation procedure, but only indicated how one might be stated(III-106)".

[3] (III-97):"if we wish to take the simplicity of grammars seriously as a means of validating grammars(,)(W)e must develop in an abstract manner, a general schematic account of the form of grammars, and we must give a general definition of simplicity for grammars of the proper form."

Little interest attaches here to further consideration of the attempt to literally define simplicity of grammars in order that a mechanical evaluation procedure might select the notationally simplest candidate grammar.[1] Of course the notion of a mechanical method of theory choice in an empirical science is naturally a target for obvious objections [2] and in any case, evaluation procedures have been abandoned in the most recent models of generative grammar.[3] But the peculiar complexion -- the notion of a mechanical evaluation procedure for theory choice -- given the conception of justification of grammars in terms of a metagrammar in LSLT exhibits the same thorough-going formalism which, inspired by Quine's attack on "the theory of meaning", dismisses semantical considerations as irrelevant to the determination of linguistic form. There is to be no reliance on meaning in the definition of the primitives of linguistic theory nor in the justification of grammars constructed in these formal terms.

Our remaining discussion is restricted to an examination of this general orientation to formalism in LSLT, an orientation which maintains that linguistic form (i.e., 'syntax') is methodologically and theoretically independent of meaning ('semantics) and which rests upon a purported

[1] Further discussion of this approach to simplicity and evaluation measures as employed in Chomsky and Halle (1968) may be found in Sober (1975), Chapter 3.

[2] E.g. Suppes (1975) and Putnam (1974b:268).

[3] As recently as 1973, Chomsky argued that, though it is a "logical possibility" that "evaluation procedures are not necessary", this possibility is nonetheless an "unlikely one" (1975a:27-8). On the disappearance of evaluation procedures in "modular" models of generative grammar, see Williams (1984), discussed below in Chapter 4 §3.

demonstration that semantic notions are strictly irrelevant to either defining the phoneme or to specifying the native speaker's "intuitive sense of grammaticalness". We shall not here be directly concerned to trace how this doctrine of "autonomy of form", which links up with the ancient tendency to view language as 'form with meaning', has been preserved, though with modifications, throughout the thirty-odd year trajectory of generative grammar. [1] Some discussion of these matters may be found in Chapter 4 §3. It is, however, manifestly evident that the motivation for the formalist program of linguistic theory and the justification of grammars proposed in LSLT has changed rather dramatically over the intervening period. In particular, the on-going concerns of linguistic metatheory in the early 1950's -- the attack on the theory of meaning, the problem of selecting a 'best' grammar (the "nonuniqueness problem") -- are hardly recognizable or, as with the latter, are stood on their heads (to apply Marx's metaphor for the relation of his to Hegel's philosophy)

1 Thus Chomsky, in Aspects of the Theory of Syntax, argues against "the widely voiced (but, for the moment, totally empty) claim that semantic considerations somehow determine syntactic structure or distributional properties" (1965a:229 fn 13). In Chapters 4 §2, 5, and 6, below, we attempt to give some substance to the claim here found "totally empty". The ancient pedigree of the autonomy doctrine is pointed to in Chomsky (1981c:4):"We might think of language, following Aristotle's familiar phrase, as sound with a meaning. The English language, then, would be regarded as a set of pairs (s,m) where s is a certain real world object, a physical sound, and m its meaning." Chomsky's (1975b) homage to Jespersen elaborates on this theme, and cites De Anima 420^{b} as the source for the attribution (25 fn 2).

in the transition to models of grammars incorporating levels of "semantic representation" and where, under a metaphilosophical perspective of "realism" -- according to which biological and genetic evidence will ultimately testify to the unique correctness of claims about language structure -- the goal of linguistic research is held to be the revelation of "the biological basis of language capacities" (see Chapter 4 §3). Still, there are strikingly discernable connections between the motifs of LSLT and the more recent doctrines of generative grammar: the doctrine of "autonomy of syntax", mentioned above, the view of grammar as a "system of autonomous components" (and a view of mind as comprised of a system of "modules" which "interact") corresponding to the separate linguistic levels of analysis in LSLT; in the conception of "universal grammar" ("UG") as constraining "the class of possible grammars available to the child language-learner" corresponding to the "general theory of language structure" of LSLT whose sole stated motivation is to provide a principled means for resolving the problem of empirically indistinguishable formal grammars. Indeed, until quite recently, the LSLT proposal for theory selection by a formal evaluation algorithm was retained in the form of the "little linguist" model of child language acquisition, a model which posits a mechanical algorithm that selects the formally simplest grammar compatible with the "primary linguistic data" of the child's ambient experience.[1] At present, however, the precise

[1] On the "little linguist" acquisition model, see Valian et al (1981) and Levelt (1975).

character of the relationship of LSLT to the subsequent explicit concern of generative grammar with explanation of language acquisition remains to be drawn and is, at present, a matter of some controversy.[1]

[1] The controversy concerns whether and to what extent the later psychological ("mentalist") and "realist" interpretation of generative grammar are prefigured in LSLT (see Katz (1981:33ff) and Steinberg (1975)). For his part, Chomsky has maintained that there is an essential continuity, that LSLT assumes implicitly what was later to be made explicit:

> LSLT is an attempt to develop a theory of transformational generative grammar. The "realist interpretation" (see below-TR) of linguistic theory is assumed throughout, and it is argued that the competence attained by the normal speaker-hearer is represented by a transformational generative grammar....The principles of this theory specify the schematism brought to bear by the child in language acquisition. They define the linguistic universals that constitute "the essence of language"(...), and thus can be taken as one fundamental element in the characterization of the innate "language faculty". Work by many investigators since has enriched and modified many of the notions developed here and developed the framework that is only implicit in LSLT and has placed it in a rich tradition that was entirely unknown to me at the time. (1975a:45)

> In LSLT...the emphasis was on I(nternalized)-language, though the term was not used. (1984:11 fn14).

Two points regarding these quotations: by "realist interpretation" in this context, Chomsky refers to the claim that "a grammar determined by a linguistic theory (given data) consititutes a hypothesis concerning the speaker-hearer's knowledge of language"(1975a:37); "I-language" is defined as"something in the mind of the person who knows the language, acquired by the learner, and used by the speaker-hearer"(1984:7) and as "things in the world in particular mind/brains"(1984:10). Katz and Steinberg, on the other hand, point to the explicitly anti-mentalist and operationalist cast of LSLT. The dispute is complicated by the fact that there are three versions of LSLT. The "first and most widely circulated" (Chomsky (1975a:2) version is the typescript of June, 1955. This is the text on whichour discussion is based. In addition, there is a "partially edited and revised January 1956 version"(1975a:3), deposited at the Harvard and MIT libraries on microfilm. A microfilm copy of LSLT was obtained by interlibrary loan from the University of California at Berkeley library; its designation was"MIT Libraries" and was dated 1961. An exhaustive comparison with the June, 1955 typescript revealed only few and superficial differences (omission of appendices, correction of typographical errors). A few minor technical changes were found inChapter VIII "Transformational Analysis"(pages VIII -377,-388,-389,-422 and -465). One other minor change was found in Chapter IX "Transformational Analysis of English" (IX-548). The third version was published as The Logical Structure of Linguistic Theory by Plenum in 1975 (a softcover edition appeared in 1985 from University of Chicago Press). According to Chomsky (unpublished correspondence with Jerrold J. Katz, dated November 6, 1982) this text is "an edited version of the 1956 version of LSLT, deposited at Harvard and MIT libraries in

In any event, LSLT is a work that can, and may, be dealt with on its

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microfilm, and that is quite different from the 1955 version". Although this formulation has two interpretations, it is clearly the published 1975 version, and not the January 1956 version, which is "quite different" from the 1955 version. For there are literally hundreds of changes, totaling thousands of words, and primarily in Chapters I-V (= Chapters X and I-IV of the 1955 and 1956 versions)which deal with methodological and conceptual rather than technical issues. While many of these changes are rewordings or alterations that do not substantively change content, there is a perceptible difference of nuance between the 1955 and 1975 versions. This may be seen in comparison of similar passages; in the following, differences (substitutions, rewordings, additions) between the 1955 and 1975 versions are noted by enclosing the different 1975 formulation in square brackets and underlining the corresponding 1955 text:

> The form of theory that we have just described, where every notion appearing in the theory is completely analyzed in terms of a set of operational primitives, is a very strong one....But it seems to me that this is a correct way to state the goal of that aspect of linguistic theory that we are here considering.
>
> Wells has pointed out recently that philosophers have, by and large, rejected as a general criterion of significance, the strong kind of reductionism that we are suggesting as necessary [appropriate] for our particular purposes. He offers this in criticism of Bloomfield's program of avoiding mentalistic foundations for linguistic theory. It is true that many philosophers have given up a certain form of reductionism, of which Bloomfield's program (and our restatement of it)is an instance, as a general (ital.) criterion for significance,... [as a general criterion for significance, the kind of reduction that our restatement of Bloomfield's program has as its goal...] However I do not believe that this is relevant to Bloomfield's antimentalism. [, or to the approach to linguistic theory that we have outlined.] (1955a:I-19/20;1975:85/6)
>
> At present it seems to me proper to say that whereas we know of many grammatical notions that have no semantic basis, we know of none for which a significant and general semantic analysis is forthcoming. And for the present at least, this justifies the tentative identification of grammar with distributional analysis. [This justifies the tentative assertion that the theory of linguistic form does not have semantic foundations.] (1955a:I-45;1975:97)

Similarly, many references to the "operational" character of the primitive notions of linguistic theory have been deleted from the 1975 version (e.g., at I-20/21 corresponding to 1975:86, at I-15 corresponding to 1975:83, at X-714 corresponding to 1975:61, at I-24 corresponding to 1975:87) although, as can be seen in the above quotation, some remain. In addition, the 1975 version contains at least one reference to "universal grammar",

> The program of developing a general linguistic theory is reminescent, in certain respects of much earlier attempts to develop a universal grammar (108)

whereas neither this term nor "language universals" occurs in(1955a).Nor is there a discussion of language acquisition; as Chomsky notes in his "Introduction"

own terms, not indeed as a proposed theory of language structure but as

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to the 1975 version (1975a:13), "these matters are not discussed in LSLT, but the issues lie in the immediate background of this work and have been the subject of considerable discussion and controversy since". It is therefore somewhat peculiar to read in this same "Introduction":

> it is suggested in LSLT that linguistic theory characterizes a system of levels, a class of potential grammars, and an evaluation procedure with the following property: given data from language L and several grammars with the properties required by the linguistic theory, the procedure of evaluation selects the highest-valued of these. It is thus suggested that the language learner (analogously, the linguist) approaches the problem of language acquisition (grammar construction) with a schematism that determines in advance the general properties of human language and the general properties of the grammars that may be constructed to account for linguistic phenomena. His task is to select the highest-valued grammar....Having done so, he knows the language it generates. (1975a:12, emphasis added).

The inference to the second suggestion is clearly ex post facto (or, perhaps, implicit, given later understandings); it is not explicitly made in either version of LSLT. The reference to language learning in LSLT occurs in the context of a statement that the goal of linguistic theory is to "formally reconstruct" the native's speaker's ability to produce and recognize new sentences, what is later termed "linguistic creativity":

> in learning a language, the native speaker has done much more than merely absorb a large set of sentences which he can now reproduce. He has also abstracted from this set of sentences, somehow, and learned a certain structural pattern to which these sentences conform. And he can add new elements to his linguistic stock by constructing new sentences conforming to this structural pattern.
>
> Is it possible to reconstruct this ability within linguistic theory? That is, can we develop a method of analysis which will enable us to observe a corpus of sentences, to abstract a certain structural pattern from this corpus, and to construct, from the old materials, new sentences confroming to the pattern? This is a question of fundamental importance. Our working hypothesis is that we can give an account of this process of generation or projection within the limits of distributional analysis....It is by no means obvious that an even partially adequate reconstruction of this behavior can be given in distributional terms, i.e.,in terms of the structural characteristics of observed utterances. It might be the case that many other factors in the particular history and development of the individuals concerned may be responsible for this ability. ...The program of developing methods of linguistic analysis, or, in our terms, a theory of linguistic structure, might be interpreted as being basically an attempt to reconstruct this ability to speak and recognize new grammatical sentences. (1955a:IV-113/4)

These remarks are fully consistent with the "operational parallel" Hockett (1948) and (1952b),(1954) points to between the child's production of 'new'utterances and the linguist's grammar (see Chapter 2 §5 above for discussion). In particular, there is no allusion here to "schematisms"

a novel and elaborate investigation of the possibility of constructing adequate

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> that "determine in advance the general properties of human language",etc. The substantive difference between LSLT and what are clearly Chomsky's later views is that LSLT is concerned with the so-called problem of non-uniqueness, of the validation and justification of empirically equivalent formal grammars, whereas, as he writes in the "Introduction" cited above, Chomsky comes to view "the fundamental problem of linguistic theory" to be "the problem of determining how it is possible for a child to acquire knowledge of a language" (1975a:12). The tendency to identify these two prima facie different problems, thus conflating the problem of language structure with the problem of language acquisition, reflects only a subsequent (it may be argued)revision of the goals of linguistic theory. There may be an understandable willingness to interpret the past in the light of what followed but a close scrutiny of the LSLT text, considered in itself, provides substantial grounds for questioning the claimed continuity of conception and for situating this work within the considerably different problematic of the justification of formally-based grammars that are required to project the infinitely many remaining well-formed sentences of a language from a corpus of observed sentences. To be sure, there is a formal analogy here with the later construal of the "projection problem" as "the problem of providing a general scheme which specifies the grammar (or grammars) that can be acquired by a human upon exposure to a possible set of basic data" (Peters (1972:172)), but completely missing from LSLT is the required "mentalist" or "realist" perspective which identifies the linguist's linguistic theory, explicitly based on considerations of simplicity (see below), and highest-valued grammar selected by a formal evaluation measure, with "universal grammar" and the "internalized grammar" (or, "I-language") acquired by the language-learner. This perspective is not only not present in LSLT, but is also in conflict with the identifiably "instrumentalist" views of theories expressed there and in other writings of this period: e.g.,
>
> > There has been some discussion recently as to whether the linguist 'plays mathematical games' or 'describes reality' in linguistic analysis of particular languages, where the phrase 'playing mathematical games' refers, apparently, to the conscious development of a theory of linguistic structure for use in constructing and validating grammars. ... the linguist's goal can only be to construct for each language a simple grammar which relates to the grammars constructed for other languages in such a way as to lead to a revealing general theory of which all are exemplifications. (1955a:I-12/13)
>
> Equal in importance to the problem of non-uniqueness, in the authors' view, is the question (...) 'whether the dichotomous scale is the pivotal principle which the analyst can profitably impose upon the linguistic code or whether this scale is inherent in the structure of the language'(p.47). They consider that 'there are several weighty arguments in favor of the latter solution.' Phrased in this way, this statement is at worst pointless, -- at best, misleading. If we take it literally, it seems to raise a pseudo-issue. It is to imagine that possible evidence could count for one of these positions and against the other. It is not clear what could be meant by saying that the

grammars of particular languages within the constraints of a purely formal general theory of language structure.[1] And, as ~~alluded to~~ above, our examination of LSLT has more than historical interest. For Chomsky has maintained, subsequently, and on several occasions, that the arguments presented in LSLT (and, in abridged form in Syntactic Structures and in Chomsky (1955b)) for the thesis of "autonomy of syntax" or "autonomy of linguistic form" are, in large measure correct, and continue to provide a basis for the "autonomy" doctrine as a fruitful working hypothesis.[2] Our discussion proceeds then to scrutinize the LSLT case for the irrelevance of semantical considerations to grammar, considered as a theory of linguistic form.

3.31 The Irrelevance of Semantic Notions to Grammar. In the "Introduction" to LSLT, Chomsky describes it as a study of linguistic form, as pertaining to the arrangement of words and morphemes in sentences. It is therefore a syntactic study as distinct from phonology

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> dichotomous scale is 'inherent in the structure of language' other than that this scale is the one that can most profitably be imposed upon the linguistic code. These are just two ways of describing the same thing; the choice between them is only a matter of temperament. (Chomsky (1957b:240)).

[1] Cf. Introduction":"It would be misleading, then, to describe this as a proposed theory of language structure. Rather it is an attempt to sum up and organize a certain set of theoretical investigations into linguistic structure, and to examine the implications of these constructions for syntactic description of actual linguistic material. Since these constructions are, necessarily, so tentative and incomplete, the motivation for the construction is often more important than the actual construction (O-v)."

[2] E.g., Chomsky (1979b:139):"I think, in fact, that the thesis of the autonomy of syntax, in the form proposed in the fifties and since then, is probably correct." Cf. (1969a:198-9), (1975a:21) and (1975b) for two different formulations, an "absolute" and a "weaker" version of the autonomy thesis. See the discussion of the autonomy thesis in Ch. 4 §3.

and, in particular, as distinct from semantics. As such, no reliance is placed on the meaning of "linguistic expressions". The exclusion of semantical notions from the study of linguistic form is both desirable and principled: desirable because these notions do not meet "certain minimum requirements of objectivity and operational verifiability", and principled since these notions appear to be "quite irrelevant" to problems of linguistic form. [1] In Chapter I ("The Nature of Linguistic Theory") a number of arguments are advanced in support of this assessment. There it is noted that the issue of "the role of meaning in linguistic analysis has been the subject of much debate in recent years". However, considerable "inconclusiveness" has been attached to this debate, in part, Chomsky suggests, because "the question has been argued on the wrong basis". [2] For the usual manner in which the issue of the role of meaning has been posed, viz., "how can you construct a grammar with no appeal to meaning?" assumes, by implication, that one can construct a grammar <u>with</u> appeal to meaning. [3] But there is no warrant for this assumption. It is by no means clear that a grammar can be constructed if there is "as much knowledge of meaning as you please, including synonymy". [4] Drawing upon Quine's

[1] "Introduction":"This is basically a study of the arrangement of words and morphemes in sentences, hence a study of linguistic form. Thus it is a syntactic study in both the narrow sense (as opposed to phonology) and in the broader sense (as opposed to semantics). In particular, no reliance is placed on the meaning of linguistic expressions in this study, in part, because it is felt that the theory of meaning fails to meet certain minimum requirements of objectivity and operational verifiability, but more importantly, because semantic notions, if taken seriously, appear to be quite irrelevant to the problems being investigated here (0-ii)".

[2] (I-22).

[3] (I-24); cf. Chomsky (1955b) and (1957a:93).

[4] (I-25).

partitioning of semantics into the theory of meaning and the theory of reference, [1] Chomsky argues that the obscurity of notions of the theory of meaning (i.e., of significance and synonymy) alone is sufficient reason for banning them from linguistic theory,[2] while the theory of reference does not appear to be applicable to the problems facing the linguist.[3] Yet a stronger case against the notions of the theory of meaning can be made than that based upon their obscurity. In fact, Chomsky argues for the strongest possible case refusing to admit these notions into linguistic theory: semantic notions are literally irrelevant to the determination of formal (i.e., grammatical) structure.[4] And this permits a "tentative identification" of grammar and "distributional analysis":

> At present it seems to me proper to say that whereas we know of many grammatical notions that have no semantic basis, we know of none for which a significant and general semantic analysis is forthcoming. And for the present at least, this justifies the tentative identification of grammar with distributional analysis. 5

[1] Chomsky (1955a:I-23) cites Quine (1953); see especially (1953:130).

[2] (1955a:I-22)

[3] (1955a:I-25 fn 18):"It can be argued that the theory of reference as it exists today offers little help to the linguist given his particular problem." A different assessment is later adopted; see §4.3 end.

[4] (1955a:I-23):"...in fact there is a deeper motivation for refusing to base the theory of linguistic form on semantic notions than merely the obscurity of such a foundation. What I would like to argue here is that semantic notions are quite irrelevant to problems of formal structure, that only their unclarity disguises their irrelevance, and that when the claim is put forward that linguistic analysis cannot be carried out without the use of meaning, what is really expressed is that it cannot be carried out without intuition."

[5] (1955a:I-45). Chomsky notes here that he gives an "extended sense"

The claim of the irrelevance of semantic notions to grammar indeed is a very strong one. There are, it would seem, just two ways in which one could be confident that a particular notion is "irrelevant" to a given analysis. First, if the notion in question can be reasonably precisely defined, ~~it may be shown~~ one might show that the notion nowhere enters into the analysis. But 'synonymy' at least, according to Quine and seconded by Chomsky,[1] admits of no definition more precise than ~~that~~ the essentialist construal traditionally given by an uncritical mentalist semantics. Second, ~~it~~ might ~~be~~ shown that all the notions ~~bearing~~ on which the analysis is based can be defined without any ~~reasonable suspicion being raised that a~~ hidden reliance ~~is not made~~ on the notion in question, however it is to be construed, and that the analysis, as so delimited, succeeds or may be reasonably thought capable of succeeding in the task for which it is claimed to be adequate. The arguments of LSLT appear to be of this latter variety. Chomsky argues (against Quine and unnamed "descriptive linguists") that phonemic distinctiveness (i.e., determining which utterances are phonemically distinct) can be sufficiently defined in terms of a non-semantic operational test due to Harris,[2] and

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to "this rather vague term" (i.e., 'distributional analysis'); elsewhere, he observes he has borrowed the term from Harris (X-717 fn 1), citing Harris (1951a). The "extended sense" is given in full at (III-107/8):

> The notions that enter into linguistic theory are those concerned with the physical properties of utterances, the formal arrangement of parts of utterances, conformity of utterance tokens (as determined by the pair test), and finally formal properties of systems of representation and of grammars. ...We will refer to linguistic analysis carried out in these terms as 'distributional analysis'. This usage seems to me to correspond to the practice of what has been called distributional analysis.

See below for how Chomsky's understanding of the term is a substantial departure from the practice of what Harris termed 'distributional analysis'.

[1] E.g., (1955a:I-25 and I-36f).

[2]

hence that no reliance upon synonymy is made in the definition of the phoneme. And he argues, exclusively against Quine, that the notion of interest to the syntactician is not that of 'significant sequence of phonemes' but that of 'intuitively well-formed sequence of phonemes'. We consider these arguments in turn.

Given the concern to demonstrate that "the theory of linguistic form does not have semantic foundations" [1], there is a remarkable disproportion of argument devoted to showing that notions of meaning ('synonymy') are irrelevant in phonemic theory (roughly, 15 pp. [3]) as opposed to showing that such notions are irrelevant to 'higher' levels of linguistic analysis (ca. 4 pp. [4]). The heightened emphasis on phonemics may be explicable as a response to what is viewed as an apparently widespread misconception about the nature of phonemic theory, in particular, concerning the role of meaning in phonemic analysis:

> It is almost a cliché, even among those linguists who consciously attempt to avoid meaning in their descriptive work, that in order to construct a phonemic system, while we do not need to know the meaning of expressions, we must certainly know whether or not expressions are different in meaning.5

[1] (1955a:I-44).

[2] Cf. in fn 1, p. 148.

[3] I-25 to I-36g.

[4] I-37 to I-40.

[5] I-25; in a footnote Chomsky observes,"Almost every descriptive linguist concerned with phonemics has on some occasion maintained this position, and this view has been reiterated by representatives of neighboring fields."

Here, Chomsky follows Quine's formulation in PML in glossing the linguist's determination of a phonemic difference between two utterances as a determination that they are different in meaning, spelling out Quine's inference that this is an "obvious" and "notorious" reliance on 'synonymy':

> But if we know exactly which expressions of a given corpus differ in meaning, we also know exactly which expressions are the same in meaning. To know difference in meaning is also to know synonymy, and this is the central term of the theory of meaning. ...(which is) precisely the most dubious part of semantic theory. [1]

The claim under attack is that phonemic analysis must appeal to notions of 'sameness' or 'difference' of meaning (so-called "differential meaning") in establishing phonemic distinctiveness, that "we can only find out which pairs of utterance tokens are phonemically distinct (are in contrast, form oppositions) by determining which pairs are different in meaning." [2] This claim is explicitly framed as a biconditional statement; given two utterance tokens, U_1 and U_2,

> (1) U_1 is phonemically distinct from U_2 iff U_1 differs in meaning from U_2. [3]

Chomsky proceeds to show the falsity of (1) in both directions. That (1) is false from right-to- left is established by the existence of homonymous pairs of utterances (i.e., phonemically identical utterances which do differ in meaning); e.g., I gave him a pair and I gave him a pear differ in meaning but are phonemically identical. So, "it is not the case that if U_1 and U_2 differ in meaning, then they must be phonemi-

[1] (1955a:I-25).

[2] (I-27).

[3] ibid., cf. (1957a:94-6).

cally distinct".[1] From left-to-right the inference fails because there are utterances which are phonemically distinct yet do not differ in meaning. For example, bachelor and unmarried male, or

> if one is inclined to deny the existence of absolute synonyms, consider such pairs as /ekənamiks/ and /iykənamiks/ ('economics'), ádult and adúlt, /ræšən/ and /reyšən/('ration'), /rædiyetər/ and /reydiyeytər/ ('radiator'), advértisement and advertísement etc. which often coexist in one person's speech and are clearly synonyms. Such pairs have the same meanings but are phonemically distinct. 2

Thus (1) is also false in this direction: "it is not the case that if two utterances are phonemically distinct, then they must differ in meaning".

For Chomsky, Harris' paired utterance test,[3] "a thoroughly non-semantic operational device",[4] provides a sufficient means of determining phonemic contrast, which is "the intuitive sense of distinctness of utterances that we are attempting to reconstruct in linguistic theory".[5] The identification of the phonemes of a

[1] (1955a:27).

[2] (I-28).

[3] Harris (1951a:32-33). The procedure may be summarized as follows: Two (preferably short) utterances U_1 and U_2 are selected. A speaker of the language is then asked to pronounce, randomly intermixed with other utterances $U_3,\ldots,U_n$, a number of his repetitions of U_1 and U_2. Another speaker of the language indicates which of these utterances are repetitions. If the hearer identifies the speaker's repetitions in close to 100% of the cases, then the two sets of repetitions are phonemically distinct: $U_1 \neq U_2$ (e.g., heart ≠ hearth). If the hearer can distinguish U_1 from U_2 with no more than 'random' accuracy (e.g., heart and hart), no phonemic distinction can be posited. See Harris (1968:21).

[4] (I-36e)

[5] (I-31).

language (although Chomsky's examples are from English, his argument is clearly intended to have general import) requires knowledge of difference of meaning (or synonymy), nor does such knowledge coincide with phonemic distictions discernible by the thoroughly non-semantic paired utterance test. The relation between phonemic contrast and meaning remains to be established. Any connection between the two "would be an interesting correlation between independent notions".[1] To those who maintain the necessity of 'appeal to meaning' in phonemic analysis, Chomsky replies:

> the responses to language marked by such operational devices as the pair test are not meanings. Calling them 'meanings' can only be explained as the result of an all-too-prevalent compulsion to introduce the word 'meaning' into the statement of linguistic method no matter what violence is done to the ordinary sense of this term in the process. 2

In general, claims that meaning plays an ineliminable role in linguistic analysis can often be seen to be the result of a confusion of meaning with "intuition of linguistic form", a tendency which is no doubt due to the obscurity which surrounds both of these notions.[3] Now the remedy for reliance on obscure notions, as Quine had shown, was a replacement program, reconstructing, wherever possible, obscure notions in behavioral and operationally-definable terms. Here the target of the reconstruction has

[1] (1955a:I-31).

[2] (I-32).

[3] (I-33):"The only thing that meaning and intuition have in common is their obscurity."

been changed -- not the notions of the theory of meaning, since these are irrelevant to linguistic theory, but the notion of "intuition of linguistic form":

> (T)he major goal of methodological work in linguistics is to enable us to avoid intuition about linguistic form wherever we find it, replacing it by some explicit and systematic account. [1]

Phonemic theory is the paradigm example where this replacement program succeeds; success is due to the pair test which is "the most important operational test that we have at present for any linguistic notion", and, as such, "one of the operational cornerstones of linguistic theory". [2] And, while it seems likely that "our intuitions about grammar may be useful in the actual process of gathering and organizing data", the same cannot be said for our intuitions about meaning.[3] Going yet further, Chomsky suggests that it is doubtful meaning serves <u>any</u> useful role in linguistic analysis, even as a heuristic or shortcut and where reliance on meaning may be supported and supplanted by grammatical (i.e., distributional) statement:

> (W)e can afford to be quite skeptical about the often-voiced claim that even if we can proceed without meaning, it is much easier to proceed with reliance on meaning, using this as a heuristic device to be eliminated in our careful reconstruction and validation of grammatical results. [4]

[1] (1955a:I-33).

[2] (I-35);"(I)n the case of phonemic distinctness the pair test enables us to avoid this reliance on intuition (I-33)."

[3] (I-34):"But this is not to say that our intuitions about meaning serve the same purpose."

[4] (I-34).

The strident methodological formalism expressed in these remarks contrasts quite directly with the "distributional analysis" of Harris, where the use of meaning as a "shortcut" to a distributional differentiation is advocated and where, in principle, meaning is involved in determining what sound or word occurrences are repetitions.[1]

Certainly the standing of these contrasting global assessments about the use or irrelevance of meaning in setting up the elements of linguistic description is completely dependent upon whether and how 'appeals to meaning' are made, or are required, in actually resolving problems of linguistic analysis. This alone determines the 'fact of the matter' regarding the role of meaning. But in addition, and not completely incidently, there may be certain 'internal' difficulties with a position which can detract from, or reduce altogether, its standing as argument. Thus what appears as an argument may, in reality, be unsupported assertion, ~~though admittedly~~ however based on higher-order meta-philosophical or meta-scientific considerations. The force of such considerations in guiding or directing a research program cannot be denied and can hardly be overestimated, ~~being, as~~ because they tend to be, relatively immune to the demonstrated inadequacies of particular arguments advanced on their behalf and often ~~incoucient~~ impervious to examples or counter-arguments which imply their falsity. The question before us here, as to the role of meaning in linguistic analysis, is an especially difficult case of

[1] E.g., Harris (1951a:7 fn 4):"Objection might be raised...to the effect that meaning considerations too, are involved in the determination of elements, since, for example, when sounds (or sound-features) x and y occur in identical environments they are assigned to different phonemes if the complexes containing them constitute different morphemes (e.g., [l] and [r] in the environment /-ayf/: life, rife). However, this differentiation of life and rife on the basis of meaning is only the linguist's and the layman's shortcut to a distributional differentiation. In principle, meaning need be involved only to the extent of determining what is repetition."

of this kind, especially difficult because of an antecedent unclarity concerning what may be considered to be 'purely formal' and what constitutes or is involved in an 'appeal to meaning'. Fortunately, however, a phonemic analysis, due to the agreed-upon operational efficacy of the pair test, presents us with a reasonably clear-cut example of how linguistic elements -- phonemes -- may be set up on a purely formal and non-semantic basis. As we shall see, however, the issue is not so cleanly posed when it comes to determining the grammaticality or well-formedness of word sequences because of a lack of a correspondingly clear operational test.

Returning then to the argument Chomsky has given for the falsity of the bi-conditional (1), we may at once notice that this argument requires the assumption that "we have as much knowledge of meaning and synonymy as we please." [1] Obviously, only by assuming knowledge of synonymy can Chomsky hope to show its irrelevance in determining phonemic distinctness. However, given the problems involved in trying to specify or define synonymy, this certainly is to assume "far too much":[2]

[1] (1955a:I-25).

[2] (I-36e/f):"(T)he possibility of a semantic approach to phonemic distinctness was based on the assumption that all semantic information is available, and that it is possible to assign a meaning to each utterance to be compared with other meanings. But clearly in granting this assumption, without which the discussion could not even begin, we have given away far too much. Not only is such an assignment impossible, by any means known to us, but there is a conceptual difficulty that seems to undermine the whole approach in a much more fundamental manner. We have not asked how we can determine whether the meanings assigned to utterance tokens are the same or different."

It seems that the plausibility of Chomsky's argument for the irrelevance of semantic notions in phonemic analysis cannot rest on the mere assumption of "as much knowledge of meaning and synonymy as we please" but rather requires some adequate prior means of determining whether the meanings of two expressions are the same. Possibly some specification of meaning might be gleaned from trying to conceive of meaning in terms of the (proper) use of expressions, but this suggestion circularly invokes the notion of expression for which meaning is alleged the required criterion. [1] At this point, Chomsky concludes his discussion of the role of meaning in phonemic analysis, admitting that the obscurity of the notion of meaning makes it difficult to evaluate other proposals concerning its role in grammar:

> It is difficult to evaluate many other suggestions about the role of meaning in grammar, largely because it is difficult to pin down the notion of meaning. However, I think that within the limits posed by the obscurity of these notions, it is reasonable to suggest 'intuition about linguistic form' as a more proper locution than 'meaning', wherever such suggestions are made. 2

That is, by self admission, the argument for the irrelevance of semantic notions, pending some as yet to-be-demonstrated manner of individuating meanings, reduces to the charge that the obscurity of the notion of

[1] (1955a:I-36f/g):"We must provide a method for determining when two slightly different meanings are sufficiently similar. If, on the other hand, we try to maintain the position that the meanings are identical, that the meaning is a fixed and unchanging component of each occurrence, then a charge of circularity seems warranted. It seems that the only way to uphold such a position would be to conceive of the 'meaning' of an expression (a token) as 'the way in which tokens of this type are (or can be) properly used', the class of situations in which they could be used, or something of this sort. But it is difficult to make sense at all out of such a position without the prior establishment of utterance types (...). The degree of unclarity in this discussion makes the attempt to define phonemic distinctions in such terms appear somewhat ludicrous."

[2] (I-36g).

meaning is sufficient reason to avoid an 'appeal to meaning' in grammar. Ironically, however, it is also the proclaimed obscurity of the notion of meaning which prevents the irrelevance argument from going through.

What has Chomsky's argument actually shown? That contrast, as determined by the pair test, and difference of meaning do not always coincide. The assertion that the pair test suffices to determine the phonemes of a language is an assertion which, though almost true, is not quite true. It is important to see just where this assertion cannot be sustained by looking at some of the non-neglible cruces of phonemic analysis and therefore to stake the limits of the claim that the phonemes of a language admit of a purely formal determination. These are cases where the results of the pair test are problematic and do not provide a clear sufficient basis for a decision as to whether a phonemic distinction exists.[1] And it is meaning which both immediately allows these cases to be characterized as "problematic" and which guides a resolution in terms of a statement of distributional regularities of 'higher' level (morphemes, words) elements. Thus meaning, through its correlation with distributional regularities, plays a central role in phonemic analysis. This is shown in the case of phonemic overlapping, not only in examples such as the /ˌekəˈnamiks/ (ˌiykəˈnamiks/ expression pair already cited by Chomsky, but also

[1] Cf. Harris (1968:21-2):"In some cases the results of the pair test are problematic, and in some cases the decision as to whether a phonemic distinction exists, and of what kind, is adjusted on the ground of later grammatical considerations. But the direct results of the pair test furnish a starting point, a first approximation to a set of ultimate elements adequate for characterization of language." For the sense of the term 'ultimate' adopted here, see the end of §2 of Chapter 5.

That the existence of contrast does not necessarily indicate a phonemic distinction can be seen in the case of partial phonemic overlapping. Here, one uses meaning (knowledge of morphemic differences) in order to determine what is a phonemic repetition, supporting the postulate of a meaning difference by showing a corresponding difference in morphemic distribution. For example, in Moroccan Arabic [1] [bgər'] and [bqər'] ('cow') occur as repetitions of each other (i.e., do not contrast). So it might be inferred that [g] = [q], that is, [g] and [q] are freely substitutable (are free variants) occurrences of a single phoneme. But in some environments they are not freely substitutable: [gr' a'] ('squash') and [qr' a'] ('ringworm'), [ɣrəg] ('he was parched') and [ɣrəq] ('it sank'). Here a solution is to say that there is a partial overlapping in the first ('cow') environment, that [g] is a free variant of the /q/ phoneme here whereas in the second ('squash'), [g] is a member of the /g/ phoneme; on the other hand, the /q/ phoneme has [g] and [q] in the first environment and only [q] in the second. As this example makes clear, the determination of a partial overlapping, where two distinct (to the linguist!) sounds do not contrast in one environment but do in others, requires reference to a morpheme list, i.e., to elements which differ in meaning. And the linguist must, in some sense, know these morpheme differences; he must know that [gr'a'] and [qr' a'] are different in meaning. To be sure, he can find and state a distributional correlate for this

[1] The example and solution are from Harris (1951a:36 fn 11) and (1951a:65 fn 14), respectively. Square brackets enclose what may be termed "impressionistic" (or broad, as opposed to narrow) "phonetic transcriptions"; see Harris (1951a:15 fn 16). Where the enclosed segment is submorphemic, the segments are sometimes termed 'phones' or, as pairwise compared, 'allophones'. However, since these terms are defined differently by diffent linguists (some linguists, e.g., Hockett (1958:107) maintaining, e.g., that two contrasting sounds in free variation are not allophones of the same phoneme), the simpler terminology adopted here is preferable for purposes of illustration.

morphemic difference, but unless he suspects a difference in meaning, he can not be sure where to seek a distributional difference.

Another example, closer to home, shows again how knowledge of sameness or difference of meaning may sometimes be called upon in order to decide whether a given contrast is phonemic. Here, we may see, ceteris paribus, how much conventionality is involved in maintaining that a phonemic distinction coincides with a difference in meaning. In the case of /ˌekə'namiks/, /ˌiykə'namiks/ (cited above by Chomsky, somewhat misleadingly, as clear cases of synonyms), one faces a series of choices. If (1) it is desired that the principle identifying phonemic contrast with a difference of meaning be preserved, it might either be argued (a) that, as there is no appreciable difference of meaning between them, the contrast between /ˌekə'namiks/ and /ˌiykə'namiks/ is not phonemic, i.e., that here [e] and [iy] are free variants of a single phoneme, say /e/. The claim that there is no difference of meaning between them requires that no difference in distribution can be shown to distinguish them. Another view of the matter (b), associated most notably with Bloch,[1] maintains that the [e] and [iy] are not here in completely free variation since there is a different connotation of elegance or learnedness attached to the two forms. Hence, it is maintained, the contrast is phonemic, say between phonemes /e/ and /i/, again preserving the principle. This argument as well requires that the claim of meaning difference (as different morphemes) be distributionally supported. Alternately, (2), one could simply say

[1] E.g., Bloch (1947), where the principle is stated in the form:"Phonemically different forms that occur in the same environment, and are not in completely free variation with each other, are morphemically different (247 fn 13)."

that since non-contrasting forms may be morphemically different (the case of homonyms), there is really no reason to preserve the principle that phonemically contrasting forms must be different in meaning. So, it might be argued, /ˌekə'namiks/ and /ˌiykə'namiks/ are phonemically contrasting but nonetheless do not differ in meaning. And, once again, distributional support is required for this assertion. These different choices do not exhaust the list of options. And they all do presuppose that a difference in meaning correlates with a difference in distribution, an assumption which requires qualification.[1]

[1] Cf. Harris (1951a:7 fn 4):"It may be presumed that any two morphemes A and B having different meanings also differ somewhere in distribution: there are some environments in which one occurs and the other does not." To critics of distributionalism like Bar-Hillel(1954), this principle appears vacuous since there seems no way of saying a priori in what environments a form may occur and thus no way of surveying all the environments of occurrence of a given morpheme (or word). Thus Bar-Hillel argues that oculist and eye-doctor cannot be distributionally distinguished when a language is viewed as the totality of all possible sentence-types, whereas if viewed as the totality of sentence-tokens (i.e., occurrences), the principle is trivial. But Bar-Hillel construes the situation too narrowly. Linguists (as Quine's prototypical grammarian recognizes) would have very little to go on if some reasonable assessment of what 'can occur' could not be made. It may be that, with respect to the language as a whole (the totality of sentence-types), distribution can legitimate only statements about degree of synonymy. On this point, see Hoenigswald (1965) passim and

> Intuitively, it seems right to judge the degree of synonymy, that is, so-called nearness of meaning, by the effort needed to make the search (i.e., for a discourse environment fitting one but not the other of the two forms in question - TR) successful. In this indirect and pragmatic, but centrally relevant way, attempts are constantly made (...) to account for the distribution of particular elements 'in terms of the totality of their environments' -- the speaker can consult his own potential as to what occurs and what does not (192).

The grammar of partially-ordered word dependences (see Chapter 5) is a theoretical attempt to account for actual word occurrences where the domain of constraints is restricted to sentence boundaries. And, it may be argued that most or virtually all synonyms are local in the sense that synonymy, as sameness of distribution, can actually be demonstrated only in sublanguage or discourse, due to the additional restrictions upon word cooccurrences; see the discussion of the status of members of sublanguage word classes and subclasses in Chapter 6 §3.

Overlooking the internal difficulties, noted above, with Chomsky's argument for the irrelevance of semantic notions in grammar, we have seen here that the insufficiency of the pair test to determine a phonemic solution for a language, in tandem with the 'appeal to meaning' made in resolving these insufficiencies, invalidates the irrelevance argument, as it pertains to phonemic analysis, altogether. Chomsky's argument has, however, succeeded in showing, against Quine (and others [1]), that phonemic contrast and difference of meaning need not always coincide. Quine, it may be recalled, citing Bloomfield and Bloch and Trager as authorities, [2] ruefully acquiesced in an apparently

[1] E.g., Benveniste (1967:35):"Etre distinctif,être significatif, c'est la même chose."

[2] Quine's attribution of this position to Bloomfield is not quite correct. In a passage within the pagination cited by Quine, Bloomfield writes:

> The study of significant speech-sounds is phonology. ... Phonology involves the consideration of meaning. The meaning of speech-forms could be scientifically defined only if all branches of science including especially, psychology and physiology, were close to perfection. Until that time, phonology and, with it, all the semantic phase of language study, rests upon an assumption, the fundamental assumption of linguistics; we must assume that in every speech community some utterances are alike in form and meaning (1933:78).

In a passage not included within the bounds of Quine's citation, Bloomfield returns to his "fundamental assumption":

> Our fundamental assumption implies that each linguistic form has a constant and specific meaning. If the forms are phonemically different, we suppose that their meanings also are different -- for instance, that each one of a set of forms like quick, fast, swift, rapid, speedy, differs from all the others in some constant and conventional feature of meaning. We suppose, in short, that there are no actual synonyms. On the other hand, our assumption implies also that if the forms are semantically different(...), they are not 'the same', even though they may be alike as to phonetic form. ...Different linguistic forms which have the same phonetic form (and differ, therefore, only as to meaning) are known as homonyms. Since we cannot with certainty define meanings, we cannot always decide whether a given phonetic form in its various uses has always the same meaning or represents a set of homonyms. ...All this shows, of course, that our basic assumption is true only within limits, even though its general truth is presupposed not only in linguistic study, but by all our actual use of language (145).

These remarks show that whereas Bloomfield viewed meaning as a necessary

unavoidable definition of the phoneme in terms of meaning. The existence of homonyms, pointed to by Bloomfield and seconded by Chomsky, show the limitations of such a definition. To be sure, Chomsky follows Quine's suspect inference that the linguist's appeal to sameness or difference of meaning is an appeal to synonymy. However, in as much as ('local') synonymy can be explicitly characterized in grammatical terms (see Chapter 6), there need be no in principle objection to speaking of synonymy, if the adequacy of such a characterization is clearly evaluable. Yet the case of homonyms likewise presents difficulties for the irrelevance argument proposed by Chomsky. For without some sort of 'reliance on meaning', how is the linguist to determine whether two non-contrasting word occurrences are occurrences of 'the same' word, or are homonyms? As we saw above, Chomsky's argument for irrelevance of semantical notions simply begs this issue by assuming the existence of homonyms. Here again, it is instructive to see how considerations of meaning, supported by distributional statement, enter into grammatical analysis in the resolution of the problem of homonyms.

Consider the phonemic sequence /tuw/. [1] One could say that all

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condition (wrongly, in view of our argument above) for phonemic distinctness, he did not maintain that meaning was a sufficient condition, given the difficulties occasioned by homonymity, for phonemic distinctness, contrary to Quine's allegation.

[1] An example taken from Harris (1951a:199-200).

occurrences of this sequence, as non-contrasting, were repetitions of the same morpheme, i.e., were synonyms. However, ~~in this case~~ a morphemic element ~~would be~~ set up ~~which had only a quite complexly~~ statable environment of occurrence (distribution). ~~However~~ since some morphemes have some environments of occurrence that are the same or quite similar to other morphemes (though it may still be maintained that no two morphemes share all environments of occurrence). it is desirable to partition the occurrences of /tuw/ into different morphemes according to how the range of environments it occupies is similar to that occupied by other morphemes. The result of this procedure might be to establish /tuw/ as a member of at least three recognized distinct classes of environments, e.g., those where three, six, etc., occur, where also occurs, where with, from, at etc. occur. Thus it is possible to base a multimorphemic partitioning of the occurrences of /tuw/ on formal, distributional grounds: a different meaning (alternately, a morphemic difference) may be assigned to those occurrences where it is replacable by different sets of morphemes. But this hardly shows that meaning is irrelevant to the attainment of this formal solution. Note, first of all, the distributional solution does not resolve all homonymities, i.e., where the different morphemes share the same environments of occurrence.[1] More importantly, the notion of 'replacable' requires amplification: replacable salva qua? And here, if the case for irrelevance is to be sustained, it must

[1] E.g., We were afraid Max couldn't bear to; We were afraid Max couldn't bear two; We were afraid Max couldn't bear, too.

be shown that semantic notions do not enter into the determination of 'grammaticality' or 'well-formedness' or similar notions (see below). Finally, and most obviously, many elements have distributions which are only complexly statable (perhaps only as a listing) in non-transformational terms. In this regard, many elements for which a multimorphemic solution is not sought (e.g., expect, before, etc.) cannot be distinguished from those where it is. Clearly, it is the indication of a difference in meaning which motivates the search for distributional regularities with other morphemes. [1] The formal solution to the problem of homonymity may therefore be said to be a reconstruction in explicit terms of a perceived difference in meaning. It cannot be viewed as serving in lieu of, or without such a perceived difference. The data of meaning are, in consequence, neither irrelevant to determining phonemic distinctness nor to resolving homonymity, as Chomsky's argument alleges. It remains, at this point, to consider whether Chomsky's argument for the irrelevance of semantic notions in the determination of grammaticality or well-formedness (Quine's "significant sequences of phonemes") fares any better.

As is well-known, the (in)famous sentence Colorless green ideas sleep furiously is a purported counter-example to claims (which, we have seen, are associated with Quine [2]) that a definition of 'grammaticalness' must be based upon semantic notions. The case, as presented in LSLT

[1] Moreover, the transformational demonstration that, e.g., two occurrences of expect in apparently different environments, are repetitions requires the semantical notion of paraphrase, reconstructed in terms of a partial order of word dependences; see Chapter 4 §2 and Chapter 5 §3.

[2] The notoriety of the example stems from the published work, Syntactic Structures, where Quine is not identified as propounding this position.

is initially set up as follows:

> Quine distinguishes two major notions in the theory of meaning, 'synonymy' and 'significance', and suggests that grammar relies on both for the determination of the subject matter of a linguistic description. ...Is it correct to identify 'grammaticalness' with 'significance'? I think that it is not. If we take 'meaningfulness' or 'significance' seriously, I think we must admit that
>
> (2) I noticed a round square
>
> or (3) colorless green ideas sleep furiously
>
> are thoroughly meaningless and non-significant, but it seems to me that as a speaker of English, I would regard these as in some sense 'grammatical' sentences, and it can certainly be argued that the establishment of their non-significance falls outside the domain of grammar. 1

Chomsky proceeds to note that a speaker of English will normally read (3) with the standard intonation pattern of an English sentence, whereas a sentence like furiously sleep ideas green colorless (=4), where the word order is permuted back-to-front, will be read with "the intonation pattern characteristic of a sequence of unrelated words, each word with a falling intonation". [2] It is only later, in Chapter IV ("Grammaticalness"), that an explanation is proposed for these descriptive 'facts'. Here what is to be explained is a distinction between what is termed the "grammatical nonsense" of (3) and the "ungrammatical nonsense" of (4):

> (A)ny speaker of English will recognize at once that (17 (=3)) is an absurd English sentence(25), while (18 (=4)) is no English sentence at all, and he will consequently give the normal intonation pattern of an English sentence to (17 (=3)), but not to (18 (=4)). ...This distinction can be made...by developing a notion of sentence form, and demonstrating that (17 (=3)) is an instance of the grammatical sentence form Adjective--Adjective--Noun--Verb --Adverb, which is grammatical by virtue of such sentences as
>
> (19) revolutionary new ideas appear infrequently
>
> that might well occur in normal English. 3

[1] (1955a:I-37).

[2] (I-38).

[3] (IV-147); footnote (25) observes:"More properly, an absurd semi-English sentence, when we have set up degrees of grammaticalness." The issue of

There are two contentions here. The first is simply a matter of empirical fact: that (3) will be read with the "normal" intonation pattern of English sentences whereas (4) will not. The second is that (3), for which intuitive well-formedness is claimed, is grammatical in virtue of its being an instance of the Adj-Adj-N-V-Adv "grammatical sentence form". From this second claim, it follows immediately, though is not asserted, that (4), for which intuitive ill-formedness is alleged, is ungrammatical, there being no grammatical sentence form of the type Adv-V-N-Adj-Adj of which it is an instance. Thus the notion of a grammatical sentence form is invoked as a formal reconstruction of the intuition of native speakers that a particular word sequence is well-formed. Note that this sense of 'formal reconstruction' is different from the formal reconstruction of intuitions of phonemic distinctness for which the paired utterance test, a purely non-semantic and operational procedure, was claimed to be sufficient.

Whether (3) as a matter of fact has the characteristic intonation contour of a normally asserted English sentence (and (4) does not) is a problem for careful psycholinguistic investigation. Certainly those to whom the example is familiar cannot be viewed as non-biased informants capable of rendering a 'typical' response. The more important claim is the second one which may be stated in the following way: (a) if a word sequence S has the structural form Adj-Adj-N-V-Adv, which is a grammatical

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degrees of grammaticalness , which obviously relies on a corresponding notion of degrees of acceptability or some other empirical control, has a long and chequered history in generative grammar; see Newmeyer (1980: 30 ff) and the early paper of Katz (1964b) for some of the issues involved.

sentence form, then S is intuitively well-formed in the judgements of native speakers of English (presumably, Chomsky's argument has a more general intended import, but the examples given are cited as instances of English sentence forms). And, as pointed to above, a consequence of (a) is (b): if a word sequence S has the structural form Adv-V-N-Adj-Adj, which is not a grammatical sentence form, then S is not intuitively well-formed.

Leaving aside for the moment the obvious query concerning the status of the presumed categories, Adj, V, N, etc., it is at once apparent[1] that there are counterexamples to both of these claims ready to hand. Against (a) there are sequences of the form Adj-Adj-N-V-Adv which are easily imaginable as being read with the list intonation which Chomsky holds a characteristic of ungrammatical sequences, final unordered sauces pick almost, political musty pebbles are apparently, etc. On the other hand, against (b) there are sequences of the form Adv-V-N-Adj-Adj which do seem intuitively well-formed and capable of satisfying the intonational criterion proposed by Chomsky: always dye shirts greenish blue,[2] never classify me old conservative, etc. Admittedly, pending some definitive test for intuitive well-formedness, the status of counterexamples to (3) and (4) is no better, but neither worse, than the claimed status of (3) and (4). However, the counterexamples do point up that Chomsky's argument, based on (3) and (4) for the irrelevance of semantic notions to the determination

[1] An expanded version of the following argument is given by Moore and Carling (1982:76-83).

[2] Moore and Carling's (1982:80) example.

of well-formedness, is a very weak one.

Note also that Chomsky's argument concerning (3) and (4) appears to assume that the native English speaker has intuitions of linguistic form which are explicable (reconstructable) in terms of strings of formal syntactic categories (sentence forms), for as it stands, it entails that no matter which words preassigned to a given category are substituted into the corresponding place in a "grammatical sentence form", the result will be an intuitively attestable sentence. We have already indicated how one species of counterexample to this claim may be generated. There is still another. Since very many (virtually all) words may 'belong' in their various occurrences to different ones of the traditional grammatical categories, basing claims of grammaticality upon conformity to specified strings of categories would appear to simply beg the question of how words, in particular occurrences are assigned to a given category. For example, how is one to know that (4) furiously sleep ideas green colorless has the structure Adv-V-N-Adj-Adj and not that of Adv-N-N-Adj-Adj, since we have Max needs some sleep, e.g., which can be taken as an instance of N-V-Adj-N ? Furthermore, since the establishing of sentence forms is intended as a means of projecting beyond a corpus (see below), it is clear that the account given must be modified to prevent 'overgeneration' along the following lines. As an example of the N-V-Adv grammatical sentence form (since we have John runs quickly) we may have both Hope springs eternally

and Spring hopes eternally . Since N therefore includes hope, spring and V includes springs, hopes, we can generate both hope hopes eternally and spring springs eternally as instances of N-V-Adv, yet it is highly doubtful that these can be considered intuitively well-formed in any demonstrable sense. These objections are very elementary but they do, again, point to the weakness of Chomsky's argument as presented, showing that the case for the irrelevance of semantic notions to grammar requires additional details (such as a notion of 'being in construction with' as might be defined in terms of phrase structure, or a notion of 'degrees of grammaticality', see below).

There are deeper, and more fundmental, objections to Chomsky's case for irrelevance. Let us return to the question of the status of the categories assumed by examples (3) and (4). Since Chomsky's argument purports to demonstrate the irrelevance of 'significance' (or other semantic notions) to the determination of grammatical form, then it must be required, first, that a system of categories can be specified in a purely formal way, and, second, that the assignment of words to the particular categories may also be shown to be without any reliance on semantic considerations. How can a system of categories, adequate for the reconstruction of the intuitive sense of grammaticalness, be purely formally constructed? This problem, Chomsky recognized, [1] is not separable from another: that of specifying what 'could' be in the language on the basis on what is observed to be in the language, i.e.,

[1] (1955a:IV-115/6): "(W)e might say that a speaker projects his finite and somewhat accidental linguistic experience to a set of more and more comprehensive extensions. ...The most reasonable model for explaining and reconstructing this projectibility seems to be based on the notion of syntactic category."

the problem of projecting from a given corpus to the remaining sentences of the language. There are, Chomsky argues, two immediate difficulties facing so-called "procedural" attempts to formally set up a system of syntactic categories distributionally, i.e., directly by substitution tests.[1] In any given corpus, it is likely that there is not enough regularity -- no two words will share exactly the same set of contexts -- and too much: there are some environments where almost any word can occur.[2] What one ends up with is a somewhat arbitrary class of 'diagnostic' environments and a list of exceptions and this is not a result which has any demonstrated validity for sentences not in the corpus. It appears highly unlikely that a set of categories having applicability over the entire language can be constructed on the basis of formal substitution techniques applied to a corpus. In view of the difficulties with "procedural" approaches to the problem of syntactic categories, another approach to the matter is available which requires, however, a certain "lowering

[1] (1955a:IV-133):"A substitution technique would be procedural in the sense that it would lead from the data directly to the correct grammar, that is, it would offer a practical and mechanical discovery procedure for grammars. It would tell us how to actually go about building the classes."

[2] (IV-120):"In any sample of linguistic material, no two words can be expected to have exactly the same set of contexts. On the other hand, many words which should be in different categories will have the same context in common....Thus substitution is either too narrow, if we require complete mutual substitutability for comembership in a syntactic category..., or too broad, if we require only that some context be shared."

of aims" regarding the justification of grammars.[1] This approach involves the postulation of a "completed syntactic solution" which may then be contrastively evaluated vis-a-vis another proposed solution, in a purely formal, indeed mechanical, manner:

> I think it can prove interesting to lower our aims to the weaker correspondence between theory and particular grammars, and to try to construct a definition of syntactic category that begins not with a distributional characteristic of words, but with a certain measureable characteristic of completed syntactic solutions; that is, a definition that merely enables us to assign a value, say a number, to each proposed analysis, as to which is the better, with no concern as to how, in fact, these analyses were constructed. 2

Adequacy requires that the syntactic categories in a grammar of a particular language be set up in accordance with a general definition of syntactic category given by a general theory of language structure. Proceeding from a definition of a system of categories, a "completed solution" may be sought in a methodology in which the notion of 'projection' is fundamental. Following an assignment of the words of a presented set S of sentences to syntactic categories, a set of sentence forms may be constructed (as the Adj-Adj-N-V-Adv "grammatical sentence form" of example (3)). To do this, as Chomsky remarks, is already to project beyond the given corpus S; a sentence form characterizes a finite but (depending on the size of the vocabulary) arbitrarily large set of sentences.[3] A string of categories (a sentence

[1] Recall the discussion in §3.3 above.

[2] (1955a:IV-134); Chomsky notes, however, that substitution procedures may not altogether be dispensed with: "Even though the substitution procedure will not lead directly to the system C (of syntactic categories), it may reduce significantly the number of alternative analyses that have to be evaluated. Hence if we do have an effective evaluation procedure, it becomes quite important to develop substitution procedures (...) even if these prove to be only partially effective in themselves (IV-156/7)."

[3] (IV-148).

form) generates ~~any~~ a set of sentences satisfying its categorial configuration. [1] For various n, the set S is to be analyzed in terms of a system C of n syntactic categories ~~and~~ together with the sentence forms framed in terms of C. Thus S is projected to another set S* which serves as the basis for study at ~~the~~ a 'higher' levels of analysis than the "syntactic analysis" conducted in terms of the n category analysis C.[2] In this way a set of sentences is constructed which will be a closer and closer approximation to the target set of all and only the sentences of a language, which is assumed to be somehow given in advance. [3]

At the level C, the problem is to determine (a) the best analysis of the corpus S in terms of a system of n categories and (b) to specify an evaluation procedure which selects a minimal n such that an n category analysis compares favorably with both an n+1 and an n-1 category analysis.[4] As n increases, the word members of the various n categories become smaller in number and projection will be more limited and selective. [5]

[1] (1955a:IV-118/9).

[2] "Syntactic analysis" is defined in terms of C at (IV-118/9), see further below; "projection" for study at 'higher' levels is summarized at (IV-170/1).

[3] (IV-147/8); cf. Chomsky (1957a:85).

[4] (IV-138): "Our aim here is to select a certain n such that the n-category analysis compares very favorably with the n-1 category analysis, but is not much worse than the n+1 category analysis, that is, such that there is a large drop in the number of sentences generated when we move from the n-1 category analysis to the n-category analysis, but only a small drop in moving from the n-category analysis to the n+1 category analysis."

[5] ibid.

However, in line with the explicitly stated schematic and programmatic character of LSLT, which is not to be described as a "proposed theory of linguistic structure" but rather to be understood as a "sketch of a theory ...suggesting a program of research, i.e., a specific model for syntactic description to be tested and elaborated", [1] regarding (b), no details are forthcoming as to how an evaluation measure which selects a minimal n at the level C is to be defined [2] and, with respect to (a), only a rough outline is given of how a system of categories C may be set up for English. Essentially, a syntactic analysis assigns words to membership in various major classes and subclasses which are previously assumed.[3] For English, four basic categories are assumed: N, V, Adj, and X = "everything else". [4] In addition to these, ~~there are~~ basic categories numerous subclasses ~~which~~ are posited in order to account for the apparent fact that speakers of a language can ~~order~~ rank utterances never previously encountered in terms of "their degree of 'belongingness' to the language". For example,

[1] (1955a:Introduction"):"It would be misleading,then, to describe this as a proposed theory of linguistic structure (0-v)."; "The resulting sketch of a theory should be understood,..., as suggesting a program for research, i.e., a specific model for syntactic description to be tested and elaborated (0-iv)."

[2] (IV-139):"At this point we can only speculate about which function should be chosen for minimization. There are several possible choices, and at this point there seems to be no compelling reason for making a choice one way or another. This decision turns upon the empirical consequences of the various choices, and we simply haven't the requisite data at this stage of our knowledge."

[3] (IV-116):"Let us assume that we have a finite set of sentences, the corpus, with word division marked. The corpus might contain, for instance, 12(a) John came (b) Bill ate (c) John saw Bill etc. We assign these words to classes. Let us call this assignment a syntactic analysis of the words of the language, ...We can now associate with each sequence of words a sequence of classes, replacing each word by the class to which it belongs. Thus if we assign 'John', 'Bill' to the class N and 'came', 'ate', 'saw' to the class V, we will have NV, NV, and NVN as sequences of classes corresponding, respectively, to (12a), (12b), and (12c)."

[4] (IV-2^{Ap}).

> the following sentences might all be new in English:
>
> (9) look at the cross-eyed elephant
>
> (10) " " " " kindness
>
> (11) " " " " from
>
> but I think it is clear that any native would arrange them in this order with respect to 'belongingness' to English. 1

Data of this type show that the goal of reconstruction of the native speaker's ability to project from his limited linguistic experience to new utterances is the notion of "degree of grammaticalness". [2] Towards this end, if it is assumed that a sentence like look at the cross-eyed man does occur in the corpus, then (9) will have a high degree of grammaticalness

> since 'man' and elephant' are presumably co-members of the small subclass of animate common noun, and thus (9) conforms to the selective sentence form stated in terms of this small class. (10) is less grammatical, since 'man' and 'kindness' are co-members of no class smaller than the larger class Noun, and (11) is still less grammatical, since the only class containing both 'man' and 'from' is presumably the class of all words. 3

Positing a subclass of N, "animate common noun", thus accounts for (9)'s apparently high degree of 'belongingness' to English which may be attested to by native English speakers. The larger the number of subclasses, set up on these grounds, the smaller the number of members of each subclass. The proliferation of subclasses in turn means a corresponding increase in the number of sentence types and a corresponding decrease in the number of sentences projected from the presented corpus.

[1] (1955a:IV-115). Since (11) is presumably not a sentence, Chomsky should preferably refer to (9) - (11) as "utterances...new to English".

[2] (IV-116).

[3] (IV-118).

As noted above, the restrictiveness of an n-category analysis is to be assessed with respect to both an n-1 and an n+1 category analysis and a function is to be defined which selects the optimally minimal n.

While our summary here is not exhaustive, it nonetheless may suffice to show how Chomsky's account of 'projection' in terms of syntactic categories is intended to formally reconstruct the native speaker's projection from "his finite and somewhat accidental linguistic experience" to wider and wider sets of sentences. But in what sense of 'formal' is this a formal reconstruction? Certainly, nothing has been shown pertaining to the proclaimed irrelevance of semantic notions to the determination of 'grammaticalness'. On the contrary, precisely where such a demonstration is required -- in setting up the syntactic categories and in the partitioning of the vocabulary of the corpus (or the language) among them -- we find only that a "completed solution" is assumed and we are reprieved from any "concern as to how, in fact, these analyses were constructed". [1] To be sure, the criterion of formality is upheld when it comes to evaluating "completed solutions"; this is to be the outcome of an assumed purely formal, indeed mechanical, procedure. But whatever the merits (which seem exceedingly doubtful in an empirical science) of such a scheme for theory comparison and justification, the provision of a formal method of theory selection does not ~~comprise~~ constitute an argument for the irrelevance of semantics in specifying the

[1] (1955a:IV-134), cited above.

the fundamental notions in terms of which a theory is framed. And one might well question the claimed "irrelevance" of semantics to a "completed solution" where subclasses such as "animate common noun" are set up. Is the word virus a member of this subclass? The word cutlet, or enzyme? What is the criterion? It is hardly conceivable that the extension of this subclass admits of a purely formal and non-semantic specification, unless of course, questions are begged by speaking of 'syntactic' and 'semantic'.[1]

[1] Thus Chomsky in his (1965a) says that "lexical formative rules" associate the lexical entry boy with the syntactic features (+Common), (+Human), etc. while no formal criteria are mentioned which justify this assessment. Similarly, selectional rules are considered syntactic though the features stated by the rule might more ususally be considered semantic. For instance, the selectional rule for the ategory V (verb) is

$$(+V) \rightarrow CS/\left\{\begin{array}{l}(+\text{Abstract})\ \text{Aux}\ - \\ (-\text{Abstract})\ \text{Aux}\ - \\ -\ \text{Det}\ (+\text{Animate}) \\ -\ \text{Det}\ (-\text{Animate})\end{array}\right\}$$

where CS abbreviates "complex symbol", the bracketed expression of specified syntactic features (p.95). Elsewhere, however, Chomsky considers the possibility that selectional rules such as those specify boy as (+Human) and frighten as permitting an Abstract Subject and Animate Object might actually be taken over by "the semantic component" of the model of generative grammar proposed here. This would be a change which, he asserts, would do "little violence to the structure of the grammar" (p.153). The criterion for what is 'syntactic' and what is 'semantic' is clearly stated: "to call a feature of a lexical entry a 'syntactic feature' when it is involved in a strictly syntactic rule" (154). Of course, a "strictly syntactic rule" belongs to the 'syntactic component' of the grammar. McCawley(1973b:1) voices a criticism of ASPECT'S broad construal of syntax to include selectional restrictions.

3.4 The Autonomy Thesis. Since 1955, Chomsky has consistently urged a view which has become known as the thesis of the "autonomy of syntax" (a.k.a. the "autonomous systems view"). Amidst the manifold changes and reformulations made within generative grammar during this period, the autonomy thesis stands forth intact, remaining the most visible continuity linking the various models of grammars proposed since 1955, grammars which have otherwise differed both as to specifics and ~~to~~ metatheory (see Chapter 4 §3). The persistence of the autonomy thesis may be seen in examining various expressions of it and allusions to it made throughout this period.

> The study of meaning is an essential task of linguistics; it is certainly important to find some way of describing language in use. But this is not the study of grammatical structure. ...The theory of linguistic form does not have semantic foundations (1955a:I 43-4).
>
> Grammar is best formulated as a self-contained study independent of semantics (1957a:106).
>
> ...the widely voiced (but, for the moment, totally empty) claim that semantic considerations somehow determine syntactic structure or distributional properties (1965a: 229, fn 13).
>
> I tried to show that every clear formulation of a hypothesis concerning the alleged necessity to define syntactic notions in semantic terms led to incorrect results. Thinking about these questions led to what was later termed the hypothesis of autonomy of syntax. The more I think about it, the more it seems to me that this thesis is quite natural....I also know of no substantial argument that it is incorrect.... It seems to me that the elements of syntax are not established on a semantic basis, and that the mechanisms of syntax...function independently of the other components of the grammar, which are interpretive components (1979b:138-9).

Explicitly challenged by a development within generative grammar (roughly 1965 - 1975) known as "generative semantics", the autonomy thesis was reiterated and emphasized as a "working hypothesis":

> A central idea in much of structural linguistics was that the formal devices of language should be studied independently of their use. The earliest work in transformational generative grammar took over a version of this thesis, as a working hypothesis. It seems that grammars contain a substructure of perfectly formal rules operating on phrase markers in narrowly circumscribed ways. Not only are these rules independent of meaning or sound in their function, but it may also be that the choice of these devices by the language learner (...) may be independent, to a significant extent, of conditions of meaning and use. If we could specify the extent precisely, the working hypothesis would become a true empirical hypothesis. ...(T)he extensive studies of meaning and use that have been undertaken in recent years have not given any serious indication that questions of meaning and use are involved in the functioning or choice of grammars in ways beyond those considered in the earliest speculations about these matters, say in Chomsky (1957 = our 1957a) (1969b:198-9).
>
> (T)he theory of formal grammar has an internal integrity and has its distinct structures and properties....It seems to me reasonable to adopt the working hypothesis that the structures of formal grammar are generated independently, and that these structures are associated with semantic interpretations by principles and rules of a broader semiotic theory (1975b:57).

But from the above, it appears that the autonomy thesis may be taken as making several, <u>prima facie</u> different, sorts of claims: (1) that semantic notions are not involved in the definition of the primitives of the (consequently, formal) syntactical or grammatical theory; (2) that syntax may be studied separately and independently of considerations of meaning and the use of language; (3) that the <u>functioning</u> of syntactic rules is independent of these considerations; and (4) that the language learner's "choice" of a grammar occurs largely

independently of conditions of meaning and use. We have seen in this chapter that a large portion of LSLT is devoted (unsuccessfully, we argued) to establishing (1). (2) is not quite as straightforward as it might appear since, over the years, the definition of a grammar has changed from a "self-contained study independent of semantics" to one where an explicit semantic component is included (see below and Chapter 4 §3). On several occasions, Chomsky has responded to unnamed critics of the autonomy thesis that the thesis does not entail that the study of meaning is not a concern of linguistics, or that "semantic considerations" are not relevant for linguistic theory.[1] The intent of this version of the autonomy thesis clearly is not to enjoin against 'the-study of meaning' in linguistics, or to deny that semantic facts constitute an important part of the domain of relevant data for a candidate grammar. (3) may be illustrated by the contention that certain kinds of syntactic rules, called "transformations" (such as the passive) "appl(y) blindly to any phrase-marker of the proper form, caring nothing about meanings or grammatical relations",[2] whereas (4)

[1] E.g., (1975b:44): "To show this strong (autonomy thesis to be false, it will not suffice, then, to show that there are systematic relations between semantic and syntactic notions. This assumption is not and has never been in question;.... It would be surprising indeed to find important formal elements that are devoid of semantic import." Cf. (1969b:199) and (1979b:138).

[2] (1969b:197); preceding this is the statement that "Each transformation applies to a phrase-marker on the basis of the formal configurations expressed in it, and quite independently of the meanings or grammatical relations expressed by these formal configurations."

may be taken as reflecting the metatheoretical requirement, examined above, that choice among roughly empirically equivalent grammars is to be made by a formal evaluation metric that mechanically selects the highest-valued ("simplest") candidate grammar among those "externally" (or "descriptively") adequate. As we shall see in Chapter 4 §3, this requirement of formal evaluation of candidate grammars has apparently been surrendered in the most recent models of generative grammar.

There is, in addition, a certain lack of commonality regarding the standing intended for the autonomy thesis, some interpreting it as an idealization which, if fruitful, may prove itself a useful working hypothesis,[1] while Chomsky himself, as the quotations above illustrate, inclines to the non-conditional statement that it is a working hypothesis but perhaps not yet an empirical hypothesis. Perhaps the major difficulties to be encountered in attempting to evaluate the autonomy thesis, however, lie not so much in determining just what it maintains, but rather in the fact that any evaluation presupposes a clear line of demarcation between "syntactic" and "semantic" notions. Thus, the thesis of autonomy of syntax becomes an interesting one only subsequent to a responsible and non-question begging delimitation of what is "formal" and what is not, together with a demonstration of the non-relevance of non-formal and non-syntactic considerations in treating problems properly of the auto-

[1] In their "Introduction" to a volume appropriately titled Formal Syntax, Culicover, Wasow, and Akamajian (1977) write:

> Any science is founded on certain idealizations. The legitimacy of such idealizations is measured by the fruitfulness of the theories they lead to. Insofar as an idealization contributes to advancing our understanding, it is a reasonable working hypothesis. The autonomous systems view is an idealization; thus the question is not whether there are autonomous systems in some absolute sense, but rather the assumption that there are (sic) leads to significant insights into the nature of language."

nomous domain thus demarcated.[1] As we have seen, the account of 'grammaticalness' presented in LSLT does not support, nor really constitute an argument for the proclaimed doctrine of the irrelevance of semantic notions to problems of linguistic form, or that semantic considerations (including Quine's "significance") do not play a role in the reconstruction within linguistic theory of the native speaker's intuitions of well-formedness. Nor is an argument to this effect to be found in the abridged presentation of the doctrines of LSLT contained in Syntactic Structures. Despite this, the quotations above show that Chomsky has subsequently made repeated reference to the sufficiency of the purported case offered in these early works on behalf of the thesis of autonomy, a sufficiency meriting its maintainence as a "reasonable working hypothesis".

[1] After LSLT, the autonomy thesis is raised again in detail only in Chomsky (1975b); here Chomsky outlines how such a demarcation might be made: "Suppose that among the primitive notions of linguistic theory we can distinguish some that are 'semantic' and others that are 'formal'. Thus we might take such notions as 'synonymous', 'significant', 'denotes', 'satisfies', 'refers to concrete objects', to be core notions of semantics,..., primitive in our linguistic theory; while the primitives of phonetic theory, or 'is an utterance of a corpus' (possibly idealized), or those of footnotes 16 and 22 (dealing with (a) the availability of "as much mathematical apparatus...as needed for the construction of theoretical notions", and (b) the supposition that " 'word' and 'deviant' (i.e., corrected by the linguistic community) are primitives", respectively -- TR), may be taken to be formal notions. Given a bifurcation of the primitive notions into 'formal' and 'semantic', we can ask, for each defined concept, whether terms of one or the other category appear in its definition (our version (1) of the autonomy thesis -- TR). ...Consider the purely formal concepts. We may refer to the theory concerning these as 'the theory of linguistic form'. We might discover that this theory -- which excludes the core notions of semantics -- is virtually null, or quite uninteresting. Or, at the other extreme, we might find that it includes an interesting concept of 'grammar' and 'structure', perhaps all linguistic levels apart from semantic representation (41)." Attention may be called to the subjunctive mood of this statement, as well as to the fact that the listing presented can by no means be considered exhaustive.

How might the autonomy thesis for syntactic (as opposed to other grammatical concepts, such as the phoneme) concepts be contested? Since syntax, by definition, has strictly to do with the formal arrangement of words in sentences, without concern for their meanings, or to a speaker's use of these words, it seems that the legitimizing presupposition of at least versions (1-3) of the autonomy thesis -- the presupposition that meaning has never been shown to be both useful and empirically controlled in determining grammatical structure -- would be undermined by a demonstration that a word's distribution (i.e., its range of occurrences) could be accounted for (and thus 'explained') by a grammatical theory employing transformations, one of the conditions of which is the semantic condition of paraphrase, i.e., a speaker's recognition that two sentences A and B, both containing occurrences of the word W, 'say the same'. A theory using transformations with this condition, ~~such a theory~~ must derivationally relate the different occurrences of a word to a canonical or "base" grammatical environment, thus ~~in so doing~~ showing that indeed it is "the same" word in these different occurrences. A theory of this kind would thereby exemplify the contention that meaning is needed in order to determine which occurrences of the language are repetitions, i.e., to even <u>define</u> a classification of the elements of the language.

But there also appear to be empirical grounds for challenging the autonomy thesis itself. For there are rather strong indications

that insuperable difficulties face any attempt to account for the observed distributions of a sizeable number of words via a system (and not a listing!) of purely formal rules.[1] Moreover, as Chomsky has recognized, the "gravest defect" facing grammars whose syntactic rules function independently of semantic considerations is that they massively overgenerate:

> The gravest defect of the theory of transformational grammar (i.e., transformational generative grammar -- TR) is its enormous latitude and descriptive power. Virtually anything can be expressed as a phrase-marker,.... Virtually any imaginable rule can be described in transformational terms. 2

On balance, it surely seems no less plausible to abandon the assumption of formal autonomy as to try to restrict the formalism so that it describes only occurrences of the language. And, upon the de facto surrender of the goal of accounting for the distribution of the elements of the language, linguistic theory finds itself -- since the word co-occurrences and speaker's judgements informed by the patterns of word co-occurrence in his linguistic community are, after all, the observables for linguistic theory -- launched on a new course pursuing vastly different goals and employing explanatory constructs with less clearly understood ties to empirical observation (see the discussion in Chapter 4 §3).

In this context, the few remarks in LSLT pertaining to "underlying form" and "underlying structure" are of special interest. For here Chomsky notes that it is the character of the relevant linguistic

[1] The reference here is to the results of Gross et al discussed at the end of Chapter 4 §2.

[2] Chomsky (1969b:124-5).

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[2] Chomsky (1969b:124-5).

behavior, i.e., presumably that behavior evidenced in "intuitions of linguistic form", that forces upon the linguist an account of this behavior in terms of underlying structure or form:

> Describing a corpus in terms of C automatically produces a certain projection of the corpus. Further projections will be discussed below in terms of other structures. ... We see, then, that the linguist is led to the study of underlying form, and to the formulation of principles of classification in terms of substitutability, simplicity of function, similarity of formal features, etc. ...This emphasis on underlying structure does not arise from any desire to impose a rigid and simplified system on the actual variety of the real language....(T)his emphasis is forced on the linguist by the nature of the behavior he wishes to investigate. 1

But as the quotation in fact shows, it is actually the ~~actual~~ character of the methods proposed in LSLT for the 'formal' reconstruction of intuitions of grammaticalness which may be viewed as the primary source of the enduring belief that explanatory appeal must be made to some notion of structure or representation underlying that of words and their relations of co-occurrence, a view which becomes a major article of faith in the subsequent mentalist and overtly psychological interpretations of generative grammar. Otherwise put, the roots of the doctrine of "abstract underlying levels of representation" may be traced to the position assumed in LSLT that a native speaker's intuitions of linguistic form (i.e., 'grammaticalness') are intuitions of, or are explicable in terms of strings of antecedently specified syntactic categories (i.e., sentence forms). In this way, the demand, in LSLT, for a formal reconstruction

[1] (1955a:IV-148/9).

of intuitions of linguistic form leads by an agreeably natural progression to later, avowedly mentalist, views of linguistic theory positing the existence of "psychologically real" underlying levels of representation 'guiding' or otherwise involved in the production and understanding of language.[1] It follows that formalism is a jumping off point for mentalism, once the restrictive fetters of operational definition, so characteristic of the anti-mentalistic program of LSLT, are cast aside.[2]

The explanans of grammatical theory may be taken as the native speaker's 'intuitions of grammaticalness'. However, there appears to be no way of isolating (a structure for) this intuition apart from how particular intuitions are evidenced with respect to particular word sequences. Accordingly, it is the pattern of

[1] The familiar argument for the necessity of postulating "deep structures" based on transformational relations between sentences (see, e.g., Chomsky (1972:16-7)) is, in fact, only a particular instance of the more general case for underlying form, deriving (as we have seen) from a concern to reconstruct intuition in purely formal and non-semantic terms. On the transformational argument for "deep structure" and the accompanying empirical difficulties facing such approaches which 'insert' lexical items into formally generated structures, see Chapter 4 §2 (end) and the references cited there.

[2] Some evidence of the 'sea-change' involved in the transition of generative grammar from formalism to mentalism may be gathered by contrasting the following:

> The form of theory that we have just described, where every notion appearing in the theory is completely analyzed in terms of a set of operational primitives, is a very strong one. A weaker conception of scientific theory can be given. But it seems to me that this is a correct way to state the goal of that aspect of linguistic theory that we are here considering (1955a: I-19).
>
> It is sometimes assumed that operational criteria have a special and priviledged position..., but this is surely a mistake. For

word co-occurrences informing these intuitions (a pattern which indeed projects beyond a fixed corpus) for which a grammatical principle of composition is sought. Should the compositional principle be specified, not in terms of abstract underlying categories -- for which no compelling justification is presented, to our knowledge -- but in terms of properties of words, then, inasmuch as words cannot be formally concatenated, like beads on a string, into 'new' sentences with any significant degree of

(continued from previous page)

> one thing, we can be fairly certain that there will be no operational criteria for any but the most elementary notions (1964:56)
>
> ...(I)t appears that the 'antimentalistic' arguments that have been characteristically proposed would, were they correct, apply as well against any attempt to construct explanatory theories. They would, in other words, simply eliminate science as an intellectually significant enterprise (1963:328).
>
> Wells has pointed out recently that philosophers have, by and large, rejected, as a general criterion of significance, the strong kind of reductionism that we are suggesting as necessary for our particular purposes. He offers this in criticism of Bloomfield's program of avoiding mentalistic foundations for linguistic theory. It is true that many philosophers have given up a certain form of reductionism, of which Bloomfield's program (and our restatement of it) is an instance, as a general criterion for significance, ...However, I do not believe that this is relevant to Bloomfield's anti-mentalism. The fact that a certain general criterion of significance has been abandoned does not mean that the bars are down, and that 'ideas' and 'meanings' become proper terms for linguistics....If this rejection of an old criterion is not followed by construction of a new one, then it simply has no bearing on the selection of legitimate terms for a scientific theory. Where it is followed by some new sense of 'significance', then if this new sense is at all adequate, it seems to me that it will rule out mentalism for what were essentially Bloomfield's reasons, ~~its~~ its obscurity and general uselessness in linguistic theory (1955a:I-19/20).

empirical adequacy, such a principle is unlikely to be found which is purely formal. To give only one example of how semantical considerations may be seen to enter into the determination of linguistic form, consider that such a compositional principle must account for the occurrence of what is semantically recognizable as 'the same word' in apparently differing grammatical environments. For instance, there are apparently 'transitive' verb occurrences of walk (They walk Max's spaniel nightly) by the side of 'intransitive' (They walk nightly) and 'noun' occurrences (They take a walk nightly). No adequate theory of syntactic (grammatical) categories will merely list these occurrences as belonging to different categories; to do so ignores an important datum -- that these different occurrences share a common factor of meaning or are semantically relatable (e.g., the first has the second and third -- which are paraphrastic -- as consequences). What has to be shown is how this common factor of meaning accrues to each of the different occurrences. And to do so involves no less than showing how these apparently different occurrences may be derivationally (i.e., transformationally) related, the assigned grammatical category of walk remaining invariant, where the empirical condition of transformation is a semantic condition of paraphrase or consequence. [1]

[1] In Chapter 5 §3 and in Chapter 6 we examine such a conception of transformation, whose necessary condition is paraphrase and sufficient condition is preservation of a partially ordered word dependence relation (grammatical category), and discuss how transformations are employed in 'regularizing' linguistic description by eliminating variant forms that 'say the same'. See Harris (1982:212 ff) for an account along these lines of 'derived nouns' such as walk in the third example above.

To the difficulties, surveyed above, encountered in attempting to evaluate the autonomy thesis, can be added another: counterexamples raised against specific grammatical proposals determined to be in conformity with one or another version of the thesis can at most only indicate an inductive unlikelihood that the general thesis is true. It may still be that the autonomy thesis is susceptible to empirical test, yet the most feasible means of challenging the thesis is perhaps to actually construct an empirically adequate grammar in which meaning, or "semantic considerations", operating under specified empirical constraints, does play a role in the definitions of the primitives of linguistic theory or in the "functioning" of grammatical "rules". [1] But in any event there are other, methodological and philosophical, objections that can be raised against the autonomy thesis which are suggestive, if not compelling. Let us first briefly consider one of the consequences of adopting the autonomy thesis from the point of view of linguistic (and general scientific) methodology. It would seem that the autonomy thesis is attendant upon a general, and very ancient, conception of the nature of language, revitalized by generative grammar, which holds that language is an association of sound and meaning. [2] Accordingly, generative grammars have the overall structure

[1] This seems to be the course suggested in LSLT:

> In place of the customary challenge "how can you carry out linguistic analysis without meaning," it is perfectly proper to ask "how can you carry out linguistic analysis with meaning?" It is not at all evident that there is any way to meet this challenge (I-34)."

[2] (1981c:4) and (1975b:25 fn 2) cited above, p. 141 fn 1; Cf. (1972a:115).

of systems of rules expressing a correspondence between representations of sound and representations of meaning. [1] The demands of probity, therefore, require that the details of this correspondence, which certainly involves systematic exposition of the nature of both levels of representation, be elaborated. Despite this, it does not appear to be an overstatement to report that, through no lack of effort, the attempt to articulate a theory of semantic representation within generative grammar has scarcely gotten off the ground. [2] To be sure, Chomsky -- in the period roughly between 1964 and 1975 (see Chapter 4 §3) -- speaks of the requirement of "a universal language-independent system of semantic representation",[3] while keeping a cautious distance from the various proposals [4] made within generative grammar as to the character of "the level of semantic representation". His endorsement seems to have been limited to expressions of his belief that "sentences have an intrinsic meaning determined by linguistic rule" [5]

[1] E.g., (1972a:116), (1981c:4).

[2] See, e.g., the assessment by a practioner of generative grammar given in Hornstein (1984) "Introduction".

[3] E.g., (1972b:62):"Let us assume given two universal language-independent systems of representation, a phonetic system for the specification of sound and a semantic system for the specification of meaning. As to the former, there are many concrete proposals; for example, the system described in detail in chapter 7 of Chomsky and Halle (1968). In the domain of semantics there are, needless to say, problems of fact and principle that have barely been approached, and there is no reasonably concrete or well-defined "theory of semantic representation" to which one can refer."

[4] By Katz and Fodor, Katz and Postal, Katz, Jackendoff, and others.

[5] (1972a:115).

and of his optimism that such a system might be successfully developed. [1] However (as is further shown in Chapter 4 §3), by 1975 or so, the idea of a semantic representation characterizing "the intrinsic meaning of a sentence" is altogether abandoned, while the most recent work articulates "an approach to U(niversal) (G)rammar" which deals only with levels of representation of "the syntactic component". [2] Methodologically considered, then, an obvious hiatus arises in positing purely formal structures to which "interpretive" semantic structures are supposed to correspond, when no details are provided as to the nature of this interpretive component. Small wonder that critics of the autonomy view have been quick to point out that, given the completely unspecified character of semantic rules, it is difficult to see how the syntactic rules can be considered to be constrained, in the required sense, at all. [3]

A purely philosophical objection may also be lodged against the autonomy thesis. On this view an autonomous syntax is held to be formulable independently of considerations of semantics or of the

[1] (1972b:62-3):"I will, however, assume here that such a system can be developed, and that it makes sense to speak of the ways in which the inherent meaning of a sentence, characterized in some still-to-be discovered system of representation, is related to various aspects of its form."

[2] (1981a:4).

[3] E.g., McCawley (1973b:56):"In view of the fact that...Chomsky's present assumptions leave one with no way of determining in advance what the factual domains of 'syntax' and of 'semantics' are, any restriction on 'syntax' can be met simply by calling rules that violate it 'semantic', if 'semantic' rules are left unconstrained."

use of language or of the functioning of syntactic elements in communication. [1] Nonetheless, from the perspective of what is termed "naturalism" in Chapter 5 §1, such a purely formal system of rules, whether or not it could be shown to be empirically adequate over some domain of linguistic data, must appear completely fortuitous. As Dewey recognized, the existence of a purely formal system, such as mathematics, does not prove the separation of form and matter (or meaning), it merely poses the problem of the relation of form and matter in a fundamental way. [2] That is to say, such a system itself requires explanation. [3] The assumption of a purely

[1] Thus Chomsky's penchant for speaking of the nature of language as "an instrument for the free expression of thought" (1972a: 101) rather than in terms referring to its function in communication; see e.g., the discusssion in (1979b:87-8) where the two views are explicitly contrasted, and the discussion of this issue in (1971), concluding with the remark "Where properties of language can be explained on...'functional' grounds, they provide no revealing insight into the nature of mind (41)."

[2] Dewey (1938:286):"(T)he idea that there is a sharp distinction, if not a separation, between form and matter, rests on a special purely metaphysical tradition. The admittedly formal character of mathematics does not prove the separation of form and matter; it rather poses that problem in a fundamental way."

[3] Here the appeal to a species-specific genetic endowment ("Universal Grammar") which constrains the form of any grammar that a child can 'acquire' raises yet a further mystery: How comes it that evolution has produced such a schema in language users? See further Chapter 4 §3.

formal syntax is tantamount to no less than the assumption of a prior language in which this formalism acquires significance. But for a natural language, there is no prior language; there are only the utterances themselves and the determinable differences of meaning these utterances occasion to members of a given linguistic community. The existence of these determinable differences of meaning is evident in the departure from complete freedom of co-occurrence with one another among the elements of the language. In the absence of a prior language, form or grammatical structure can only be isolated and identified with respect to the linguistic behavior of language users, and in particular, to their recognitions of well-formedness and that some utterances 'say the same' as others. With the empirical control of these aspects of linguistic behavior the grammarian is able to construct equivalence classes of elements, each of whose members is the same with respect to these determinations (see further Chapters 5 and 6).

The only sure way of coming to an informed assessment of the various claims regarding the role of meaning in linguistic analysis is to clarify just what the linguist's 'reliance on meaning' amounts to and whether or not this reliance is susceptible to control by methods and criteria which must be stated in advance. It seems extremely unlikely that the manifold distinctions of meaning apprehended by a linguist in working with a language can ever be adequately reconstructed in the idiom of Quine's favored subset of behavioral criteria. Neither is it apparent why they should be. And neither can these distinctions of meaning ~~they~~ be dismissed as irrelevant unless it is shown, in more than programmatic outline, that grammars whose elements are purely formal are empirically adequate (i.e., do account for the observables of grammatical theory: word cooccurrences and, to be sure, 'linguistic intuitions' regarding these).[1] The use of meaning in linguistics need not, contrary to the allegation in LSLT,[2] indicate that "the bars are down" or that the linguist is trafficking in a shadowy and obscure contraband of ghostly essences. Perceived meaning distinctions may be correlated with differences in distribution. Yet, as Chomsky emphasizes, statements of distributional regularities do not suffice to justify the elements set up in a grammar. These are regularities which must

[1] On the question of 'coverage', see Chapter 4 §2.

[2] (1955a:I-20):"The fact that a certain general criterion of significance (for scientific theories, e.g., reductionism - TR) has been abandoned does not mean that the bars are down, and that 'ideas' and 'meanings' become proper terms for linguistics any more than it means that ghosts are proper concepts for physics."

themselves be accounted for by a grammatical theory sufficiently constrained so as to eliminate ad hoc corrections and adjustments. Observed distributional regularities are data which are to be explained by a grammar in a manner which is uniform as more and more sentences are added to an initial corpus of sentences. It remains to Chapters 5 and 6 to exhibit how meaning, reconstructed as predication-created information, can be the test of adequacy of a theory of language structure and the particular grammars which are constructed in accordance with its constraints. But first, in Chapter 4, ~~it remains to~~ we will examine some of the issues involved in holding that a grammar is a theory of linguistic abilities, and ~~to~~ chart the evolution of generative grammar from its formalist origins in LSLT, to <succinct characterization? Biologism?>.