

Distributionalist and Mediationalist Themes in the Development
of Linguistic Theory

Geoffrey J. Huck and John A. Goldsmith

Our aim in this paper is to explore real and apparent differences between some prominent research programs in linguistics during the past sixty years by focusing upon their research objectives or goals.* A program's objectives, unlike the hypotheses it generates, are not subject to empirical confirmation or falsification. Nevertheless, these objectives affect the results the program achieves, since they both determine which questions are worth investigating and exclude as potential answers to those questions hypotheses that are felt to be in conflict with them.

We will be concerned here with two very general objectives for grammatical research programs, both of which have long histories. The first has assumed that linguistic analysis should be undertaken in an attempt to discover and explain the relationship between sound and meaning, between outer form and inner form. We will call this objective the mediational objective to underscore the idea that language in this view is something that mediates between outer form and meaning.

The second objective takes it that linguists' central task is to explain the patterning and distribution of the formal elements of language. A principal assumption here is that there IS a formal structure to language and that it can be described and explained on its own terms. We will call this the

distributional objective.

The two objectives can happily coexist; indeed, most linguists in this century have at one time or another adopted both. For example, the mediational relation between sound and meaning is crucial in the work of Leonard Bloomfield, although some have erroneously interpreted the antimentalism that Bloomfield espoused to indicate that he had no place for semantics in his conception of grammar. The importance of the sound-meaning connection for Bloomfield is perhaps nowhere clearer than in his definition in his book Language (1933:160) of a constituent as "the common part of any (two or more) complex forms" which bear "partial phonetic-semantic resemblance[s]" to each other. Although Bloomfield sometimes spoke of "forms" (= "phonetic forms") as stretches of sound, he intended the technical term "complex form", on which his definition of constituency depends, to denote a sound-meaning combination. That is, a linguistic form for Bloomfield is "[a] phonetic form with its meaning" (1933:166). Thus, John is a constituent of the complex forms John ran and John fell, since these two sentences contain stretches which are not only phonetically identical, but which also carry identical meanings (1933:159). Indeed, Bloomfield (1943:272) explicitly denied that a consideration of meaning could be excluded from linguistic analysis:

"In language, forms cannot be separated from their meanings. It would be uninteresting and perhaps not very profitable to study the mere sound of a language without any consideration

of meaning."

At the same time, Bloomfield was wary of the indeterminacy of semantic concepts as compared with the better understood distributional properties of sounds. In Language (1933:140), he calls the statement of meanings "the weak point in language study" and suggests it will remain so "until human knowledge advances very far beyond its present state." He later contends that the difficulty in analyzing meanings is a consequence of the fact that "the things and happenings in the world are many and varied and also because different languages classify them in different ways." (1943:274) Thus,

"It is easy to describe, classify, and arrange the forms of a language, but even if we commanded the entire range of present-day knowledge, we should still be unable to describe, classify, or arrange the meanings which are expressed by these forms." (1943:273)

All of this suggests to Bloomfield that language analysis must start with forms and not with meanings (1943:273). "Meanings," he states firmly at one point, "cannot be defined in terms of our science and cannot enter into our definitions." (1931:156)

It should be understood that this last statement does not directly contradict Bloomfield's earlier assertions about the importance of meaning in linguistic analysis. From a mediational perspective, it is clear that he had no doubts about the place of meaning in the sound-meaning dyad: "If we had an accurate

knowledge of every speaker's situation and of every hearer's response" - in short, if we had a scientific theory of meaning - then

"Linguistics, on this ideal plane, would consist of two main investigations: phonetics, in which we studied the speech-event without reference to meaning, investigating only the sound-producing movements of the speaker, the sound waves, and the action of the hearer's ear-drum, and semantics in which we studied the relation of these features to the features of meaning, showing that a certain type of speech-sound was uttered in certain types of situations and led the hearer to perform certain types of response." (1933:74)

That is, in this idealization everything that we today consider to be part of grammar would be included for Bloomfield in semantics. However, since "our knowledge of the world in which we live is so imperfect", that research program must be postponed. In the meantime, we must "act as though science had progressed far enough to identify all the situations and responses that make up the meaning of speech forms" and (at least in the case of our own language) "trust to our everyday knowledge to tell us whether speech-forms are the same or different" (1933:77). Hence, we are to rely on "the fundamental assumption of linguistics": we must assume that "in every speech-community some utterances are alike as to form and meaning" (1933:78).

In distributional analysis, the linguist uses the fundamental assumption to identify the linguistic forms of a

language:

"Only by finding out which utterances are alike in meaning, and which ones are different, can the observer learn to recognize the phonemic distinctions" (1933:93)

"In studying a language, we can single out the relevant features of sound only if we know something about the meaning.... An observer who first hears the Chippewa of Wisconsin or Michigan will note down such forms as [gi:zik, gi:sik, ki:zik, ki:sik], and he will not know whether he has recorded one, two, three, or four different words. Only when he learns that all four indifferently mean 'sky' and when he finds similar variations for other unit meanings, will he realize that these variations are not significant. (1943:272)

That is, there are two ways in which meaning might enter into an analysis. First, an analyst might assume with Bloomfield that linguistic forms are sound-meaning bundles and attempt to identify them by noting stretches of sound which carry identical meanings. This can of course be accomplished without a theory of the structure of meaning beyond that which is necessary for a determination of synonymy (although "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" (1933:145)). Second, an analyst might employ broader hypotheses about the structure of

meaning in a particular analysis (such as, for example, that "nouns denote 'things'"). It was this latter that Bloomfield wished to rule out.

An undisciplined resort to meaning was easier to abjure in theory, however, than in practice. Among linguists in the 1940s and 1950s (as today) it was common "to use the meanings of utterance fractions as a general guide and short-cut to the identification of morphemes" (Trager & Smith (1951:54)). For example, given the Huichol forms k ye 'tree', k yezi 'trees', p k ye 'It is a tree', and p k yezi 'They are trees', the linguist would be expected to isolate the morpheme -zi by noting the meaning difference between k ye and k yezi. Any analysis which omitted a translation of the Huichol words and postulated morphemes, relying only on the native speaker's judgment that the words all have different meanings, would seem barren and unconvincing. Indeed, it is not just the fact that the four words have different meanings which permits the standard analysis, but rather the fact that their meanings are related in a certain way. ²

On the other hand, certain Bloomfieldians, most notably Zellig Harris, Bernard Bloch, and George Trager, sought to purge the intrusion of meaning as fully as possible from the theoretical apparatus. Since Bloomfield himself had not entirely delimited the distributional role he envisioned for meaning, these Bloomfieldians took its elimination in that sphere as a worthwhile goal and at the same time assumed that, in doing so,

they were parting company with their teacher (see, e.g., Bloch's statement (1948:6) "our approach differs in some respects from Bloomfield's - chiefly in that Bloomfield invokes meaning as a fundamental criterion...").

Trager, who campaigned vigorously for the exclusion of meaning from distributional analysis, made explicit what was implicit in Bloomfield's approach: "microlinguistic analysis [i.e., phonology, morphology, and syntax] works with difference of meaning only: it asks, 'Are these two items the same or different?'" (1953) (cited in Hymes & Fought (1979:140)) Thus, "the syntax of a language like English can be constructed objectively, without the intervention of translation meaning or any sort of metalinguistic phenomena" (Trager & Smith 1951:68).

But if Trager did not depart significantly from Bloomfield with respect to the place of meaning in grammar, Harris, who was Chomsky's teacher at Pennsylvania, did. Harris's pronouncement in 1940 that "the structures of language can be described only in terms of the formal, not the semantic, differences of its units and their relations" (1940:701) presaged the development of a distributional approach in which even difference of meaning has no place. In his Methods in Structural Linguistics (1951), Harris proposed a rigorous procedure for determining whether two stretches of sound in a corpus are descriptively equivalent

"we ask two informants to say these [stretches] to each other several times, telling one informant which to say (identifying it by some translation or otherwise) and seeing

if the other can guess which he said. If the hearer guesses right about fifty percent of the time then there is no regular descriptive difference between the utterances; if he guesses right near one hundred percent, there is." (1951:32)

This procedure - which came to be known as the pair test - clearly does not depend on "difference of meaning", in Trager's sense; in fact it "precludes our asking the informant if two morphemes are 'the same'. (1951:38) Harris underscores this distinction near the end of the book:

"In determining the morphemes of a particular language, linguists use, in addition to distributional criteria, also (in varying degrees) criteria of meaning difference. In exact descriptive linguistic work, however, such considerations of meaning can only be used heuristically, as a source of hints, and the determining criteria will always have to be stated in distributional terms.... The methods presented in the preceding chapters offer distributional investigations as alternatives to meaning considerations." (1951: 365)

It should be emphasized that the issue of the place of meaning in grammar was for Harris by no means simply a methodological one, as some (e.g., Newmeyer 1980:31) have supposed. Harris assumes that the structure of meaning differs from the structure of form:

"It is true that language has a special relation to meaning, both in the sense of the classification of aspects of

experience, and in the sense of communication. But the relation is not simple. For example, we can compare the structures of languages with the structure of the physical world ..., or with what we know about the structure of human response (e.g., association, transference). In either case, it would be clear that the structure of one language or another does not conform in many respects to the structure of physical nature or human response - i.e., to the structure of objective experience from which we presumably draw our meanings.... If one wishes to speak of language as existing in some sense on two planes - of form and of meaning - we can at least say that the structures of the two are not identical, though they will be found similar in various respects." (1954:780-81)

This perception, like Bloomfield's dictum that "Meanings ... cannot enter into our definitions", obviously proceeds from an assumption about the nature of the object examined, rather than about the examining process itself. The crucial conclusion here, however, is that on the "plane of form," language has a discoverable structure which does not simply follow from its function or meaning -- that is, that the systems of phonology, morphology, and syntax are autonomous and independent of the system of meaning. Joos (1950:353) provides a simple simile:

"Linguists .. have elevated their descriptive technique to the rank of a theory about the nature of language. They say, in effect, that the design of any language is

essentially telegraphic - that the language has the structure of a telegraph code, using molecular signals made up of invariant atoms, and differing e.g. from the Morse code principally in two ways: the codes called 'languages' have numerous layers of complexity instead of only two, and in each layer there are severe limitations upon the combinations permitted." [emphasis supplied]

The assumption that the forms of language are structured independently from the meanings of language (and are subject to "severe limitations" with respect to their combinability at each formal level) appeared to fit well with Bloomfield's desire to postpone the investigation of meaning "until human knowledge advances very far beyond its present state". That is, descriptive linguistics could (and should) be limited to questions of distribution -- of "the freedom of occurrence of portions of an utterance relatively to each other" (Harris (1951:5)).

It was, however, Harris's student, Noam Chomsky, who became the most celebrated advocate of the distributionalist approach. In a paper delivered at a Georgetown University conference in 1955, Chomsky claimed that "semantic notions are really quite irrelevant to the problem of describing formal structure, and that their irrelevance is disguised only by their unclarity and by the failure to formulate the purported dependence of linguistic analysis on meaning with sufficient care and precision" (Chomsky 1955). The hypothesis that syntactic

analysis could be done without recourse to semantic terms - which later became known as the Autonomy Hypothesis - has played a central role in all of Chomsky's work since his undergraduate days.

In a manuscript completed in 1955-6, Chomsky (1975) attempted to defend the claim that "a linguistic theory constructed on a 'distributional' basis does delimit an interesting and significant area of linguistic behavior"; by 1957, with the publication of Syntactic Structures, he had made his commitment to the priority of the distributionalist program explicit: "the fundamental aim in the linguistic analysis of a language L is to separate the grammatical sequences which are the sentences of L from the ungrammatical sequences which are not sentences of L and to study the structure of the grammatical sequences." The theoretical device which both effects this separation and determines the structure of the grammatical sequences was of course to be the grammar.

In the distributionalist vision of Syntactic Structures, the grammar is organized by means of a set of levels, as in Harris's work as well. However, in Chomsky's model, representations on the different levels were related by a set of rules that applied sequentially, in such a fashion that the representations on one level could be viewed as the input to such a set of rules, while the output of those rules could be viewed as the representation on the "next" level, in most cases (all cases, roughly, except the "transformational" level). The "top"

level of the grammar is the phrase structure level, which contains rules from which the phrase structure of a sequence can be reconstructed. To sequences derived at this level, transformational rules apply to carry sequences with phrase structure into new sequences that serve as input to the morphophonemic rules, and so on. Chomsky proposed two distinct types of transformational rules: first, there were singular transformations, which applied to simple sentences produced by the phrase structure rules; second, generalized transformations embedded one sentence into another. Singular transformations were further assumed to be marked as either optional or obligatory, and a sequence to which only the obligatory transformations had applied was called a kernal..

Chomsky did not offer an explicit theory concerning the relationship between the representation of a sentence at the various grammatical levels and its meaning, although he was prepared to acknowledge that one must exist: "Having determined the syntactic structure of the language, we can study the way in which this syntactic structure is put to use in the actual functioning of language. An investigation of the semantic function of level structure ... might be a reasonable step towards a theory of the interconnections between syntax and semantics." (1957:102) Indeed, one of the important conclusions that Chomsky had reached in the mid-fifties was that the meaning of a sequence depended to a significant degree on its grammatical analysis (1957:92). He had identified numerous cases of

constructional homonymity, for example, where he claimed that an adequate syntax would be required to assign a particular string alternative structural analyses corresponding to its alternative readings. However, no systematic investigation of the theory of semantics was attempted at this point.

Echoing Bloomfield, Chomsky noted in Syntactic Structures that "[p]art of the difficulty with the theory of meaning is that 'meaning' tends to be used as a catch-all term to include every aspect of language that we know very little about." (1957:103) Some significant progress in overcoming that difficulty was felt to have been made in the work of Jerrold Katz and Jerry Fodor (1963), which attempted to place an "upper bound" on any semantic theory which was to be part of a linguistic description of a language. In Katz and Fodor's view, semantic rules - called projection rules - would apply to each distinct grammatical structure of a sentence to produce a semantic interpretation for the sentence. They set three criteria by which a theory of semantic interpretation can be evaluated: the interpretations "must mark each semantic ambiguity a speaker can detect; they must explain the source of the speaker's intuitions of anomaly when a sentence evokes them; they must suitably relate sentences speakers know to be paraphrases of each other." (1963:493)

By incorporating a level of semantic interpretation in the grammar which was determined by rule from syntactic structure, Katz and Fodor had demonstrated that Chomsky's transformational theory could be legitimately considered a mediational one. The

principal problem with the Katz-Fodor view from the mediational perspective was the extreme indirection of the relationship between surface form and semantic interpretation on that view. That is, a semantic interpretation was to be arrived at only by applying projection rules to each derivation of each of the kernels of the sentence as well as to the output of any generalized transformations or of any singular transformations that changed meaning. The semantic interpretation for a sentence was then a union of sets of readings for each of the constituents of the sentence.

Now, mere complexity in the relation between levels in a theory is not in itself a mark against it, as long as the theory as a whole is demonstrably simpler than its competitors. But the more complexity there is, the more difficult it becomes to state conditions on the relation that capture broad and significant generalizations. Nevertheless, the chief complaint lodged against the Katz-Fodor theory was that their postulated readings did not contain sufficient structural information to distinguish nonsynonymous strings made up from the same lexical items (Weinreich 1966, McCawley 1968a).

Two developments during 1963-4 significantly reduced the complexity of the proposed relation between the syntax and semantic interpretation in the Syntactic Structures theory. The first proceeded from Katz and Paul Postal's (1964) hypothesis that no singular transformations change meaning: if sustained, the hypothesis meant that the rules of semantic interpretation

could be constrained to apply just to the inputs to singulary transformations, and not to their outputs. The second proceeded from Fillmore's (1963) proposal to the effect that all singulary transformations applied to a sentence before it could be embedded into another sentence by any generalized transformation, and that singulary transformations only applied to a sentence after all generalized transformations had applied to it. If the position in a matrix sentence into which another sentence is to be embedded by a generalized transformation is always marked by a special symbol, as Katz and Postal (1964) had argued, then the interaction of the singulary and generalized transformation will be entirely predictable. Following this, in his 1965 book Aspects of the Theory of Syntax, Chomsky proposed to eliminate generalized transformations altogether and to permit the phrase structure rules to introduce recursion.

From a mediational perspective, these moves were of considerable importance, since they entailed that there was a single level - deep structure, as Chomsky had called it - which was the output of the phrase structure rules and to which the rules of semantic interpretation applied. As has been often observed, these moves also paved the way for a deepening of the concept of deep structure until no difference between that level and the level of semantic interpretation remained.

One question which the semantic investigations of Katz, Fodor, and Postal during this period did not satisfactorily resolve is precisely what kind of thing a semantic interpretation

for a given sentence was to be. Katz and Postal (1964:26) had accepted Katz and Fodor's (1963) three criteria for the evaluation of a semantic level, and added the requirement that in a satisfactory theory a semantic interpretation for a sentence must also indicate "such properties as whether or not the sentence is analytic, synthetic, or contradictory". But the latitude permitted by these criteria in the formation of a semantic theory was considerable. Furthermore, as Janet Fodor (1977:68) has pointed out, although Katz and Postal apparently intended the output of their projection rules to be readings which would attach to the nodes of a deep structure, they never explicitly specified any conditions on the form that such a semantic interpretation would take.

In work published in 1968, James McCawley argued that semantic interpretations were in fact labelled trees which resembled in certain respects constructions of standard logics, in that they took the predicate-argument organization of the clause to be basic and expressed quantificational scope and anaphor-antecedent relations directly (McCawley 1968b, 1968c). This proposal had a stimulating effect on those who were attempting to sort out the syntax-semantics interface: the level of semantic interpretation was at once made concrete and homogenous, and since standard systems of symbolic logic had been developed in an attempt to deal with ambiguity, anomaly, synonymy, and so on, it was not hard to see how a level constructed from such logical materials might be highly rated

according to the Katz-Fodor-Postal criteria.

At the same time, numerous arguments were being offered that deep structure in the Aspects sense was not simply the input to the rules which determined the logical forms of the sentences of a language, but could be identified with those forms themselves. Thus, transformations, which had originally been posited to serve the distributionalist purpose of explaining patterns observed among surface structures, were now to be understood as the principal mediationalist device in relating form to meaning - or as George Lakoff put it, "the rules determining which sentences are grammatical and which ungrammatical are not distinct from the rules relating logical forms and surface forms" (Lakoff 1972:551). This hypothesis - which we'll henceforth refer to as Lakoff's Conjecture - admits of two interpretations. On the strong interpretation, what is hypothesized is that a rule serves one purpose if and only if it serves the other; but if so, then the distributionalist and mediationalist research programs should not merely turn out to be compatible, they should ultimately produce identical results. On the other hand, on the weak interpretation, which Lakoff himself appeared to endorse, there need only be some overlap, and hence an autonomous syntactic component might still be retained (albeit one which did not account for all of the distributional facts of a language).

As is well known, Chomsky - in a series of papers written in the late sixties (Chomsky 1972) - aggressively resisted the identification of deep structure with logical form. It is

commonly assumed that Chomsky's was a purely distributionalist counterattack, since he placed the Bloomfieldian methodological precondition on linguistic analysis that it be based upon non-semantic considerations. Nevertheless, the version of grammar that he developed in the ensuing years was consistent with Lakoff's Conjecture on its weak - and possibly even its strong - interpretation. For example, the theory offered in Chomsky 1977, 1981, as in Lakoff 1971 and McCawley 1980, 1988, posits both a set of autonomous syntactic rules whose output consists of representations at the level of surface structure and a set of transformational rules of semantic interpretation which relate the levels of surface structure and logical form. Chomsky's level of logical form is not to be exactly identified with McCawley's or Lakoff's (Chomsky has not accepted the latter's arguments for a radical reduction in the number of grammatical categories at that level), although all assume that quantifier phrases are sisters of the propositions which are taken to be within their scopes and that pronominal elements are bound variables (see Bach 1977). There appears to have been some confusion as to whether Chomsky's logical form is a level of semantic representation, or whether it is just another syntactic level; as Chomsky has suggested on various occasions, the issue is merely a terminological one. The rules relating surface structure to logical form in all of these works - in Chomsky's as well as in McCawley's and Lakoff's - are not only designed to play a role in the determination of the grammaticality of the

sentences of a language, but are also crucial in providing an account of the full range of sentence ambiguity, synonymy, and anomaly in the language.

Even if, as Chomsky maintains, it is a matter of post hoc discovery that a grammar set up on distributionalist grounds is also to be preferred on mediationalist grounds, it has not seemed possible in the wake of Lakoff's Conjecture to present a legitimate distributionalist theory that is not at the same time a mediationalist one, and vice-versa. Hence few analogues of the semantically innocent distributionalist approach to grammar of Chomsky 1957 have survived,³ and even those varieties of Chomskyan grammar that attempt to do away with the level of logical form (see, e.g., Williams 1986) still provide a way to encode coreference and quantificational domains in the remaining structural representations. On the other side of the coin, those mediationist theories which have no place for an autonomous syntactic component of any sort and which have consequently been hard-pressed to account for the central distributional facts turned up by syntacticians over the years have not had great success in attracting adherents.

We take it, then, as a natural development that the position which appears to be gaining ascendancy in the field is that syntactic structures are conventional, i.e., that they are for certain purposes validly considered to be arbitrarily given, and that these structures are put to use by speakers of the language primarily in service of communication. This is, of course, just

the traditional, Saussurian view of language, and differs from it chiefly in the complexity of the relation it sees between form and meaning. This view is also consistent with Lakoff's Conjecture on its strong interpretation to the extent that it makes little sense to investigate the properties of the structural component whose output is surface forms without also at the same time investigating the properties of the system that interprets those forms: the systems, although independent to a degree, are fundamentally interrelated.

The important issue for the proponent of any variety of this unified distributionalist-mediationalist view is how much power one system has relative to the other. As Morgan (1973) pointed out, the solution to this problem depends greatly on exactly what semantic properties one believes should be represented at the semantic level, and what form such representations should take. We see several considerations that bear on the answers to these questions:

1. The rules of semantic interpretation need not produce homogenous representations at a single semantic level for the semantic theory of which they are a part to be a satisfactory mediationalist theory. Jackendoff's (1972) scheme, which produced semantic representations at four different semantic levels, would have been no more mediationalist if it had posited a single semantic level. Similarly, McCawley (1975) denied that at the level of logical form all synonymous sentences have identical representations; his claim is rather that they only

have identical entailments; furthermore, McCawley also posited identical logical forms for nonsynonymous strings, relying on rules of Gricean implicature to provide the differences (McCawley 1978, 1981).

2. Thus, it may be that certain of the semantic properties of sentences reside not in their semantic representations but in properties of the system in which they are interpreted or processed. The most radical proposal along these lines is that there are no integral representations at all, but rather vast bundles of components of them distributed throughout the system (see, e.g., Hinton, McClelland, and Rumelhart 1986). In such approaches the line between system and representation is very much obscured, and it's not clear in precisely what sense the approaches themselves can be called mediationalist (see also Jackendoff 1983 and Lakoff 1987).

3. A perhaps not-so-obvious corollary of the foregoing is that notational aspects of a semantic level should not be mistaken for what they denote. As Montague made clear (1970), in his system the level he called Intensional Logic was dispensable: it was simply that postulating that level provided a convenient way for him to talk about the model-theoretic interpretations of surface sentences that he was ultimately interested in. Similarly, decisions to represent coreference via indices or to represent quantificational scope via either linear position in a string or hierarchical position in a tree are notational decisions, since the indices or the linear or hierarchical

positions must in general be further interpreted. This is not to say that the level of logical form is dispensable in all theories; e.g., as we've seen, Logical Form in Chomsky 1981 plays a role in separating the grammatical strings of a language from the ungrammatical ones.

In order to understand the significance of the work produced by any research group, it is still necessary, we believe, to take into consideration its principal program objective. Of one thing we are convinced: the descriptions that were offered at the time of what separated the Chomskyans from the Bloomfieldians in the 1950s or the generative semanticists from the interpretive semanticists in the 1960s and 1970s were overly simple and too partisan. We hope to have made it clear that the objectives of the research programs of these pairs of antagonists, and a fortiori the results they achieved, were more compatible than is generally thought. More importantly, the fundamental issues which shaped the direction of their research remain with us. This is a case where an understanding of the present depends to a significant degree on an understanding of the past.

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Linguistic Inquiry 17.265-299.

Notes

* Support for the research reported in this paper was provided in part by a grant from the American Council of Learned Societies.

1. Or students: these data constitute the first problem in Merrifield et al.'s (1971) popular text.

2. As we note, this practice raises questions today just as it did in the past, questions which remain ultimately unresolved. Since the appearance of the earliest works in generative phonology, skeptics and critics have raised questions as to what the basis is for deciding that there is anything to be explained regarding the difference in pronunciation found in, say, the English words omen and ominous, or the first syllable of hypocrisy and hypothesis (both examples from Kiparsky 1982). Something having to do with meaning is involved, of course, since no one would take seriously the proposal that wit is derived from it (and win from in) by w-insertion, or that a rule of k-insertion derives quit from wit, or quick from wick, or daiquiri from dairy. McCawley 1986 addresses this thorny question, which has received precious little consideration in the past twenty-five years relative to its importance.

3. Relational Grammar being a possible exception - although Relational Grammarians do not generally claim to a present complete theory of language. As has been noted, there is a tension in the Relational Grammar literature with regard to whether "initial stratum" relations should be taken to be a rough analogue of semantic representations (see Rosen 1984 and Delancey 1985).