

ON COMPARING LINGUISTIC THEORIES

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0. Introduction¹

In *Ideology and Linguistic Theory* (Huck and Goldsmith 1995 -- henceforth *ILT*²), we considered the decline of Generative Semantics in the 1970s as an active research program, reaching conclusions about it that challenged some beliefs had been circulating in the generative linguistics community at least since the first publication of Frederick J. Newmeyer's *Linguistic Theory in America* (1980, 1986). Although our view was unconventional, our research underscored an actuality that seems to us also to have been widely acknowledged in that community, which is that no one research group has had a monopoly on or been the exclusive locus of good linguistic work during the past several decades.

One of the popular beliefs that we found no justification for was that the failure of Generative Semantics was a consequence of a measurable weakness in its theories in comparison with those of its contemporary rival, Interpretive Semantics. After examining in some detail the arguments of both sides during the Generative Semantics period and attempting to extract from them the elements that bore most directly upon their empirical or logical validity, we concluded that the demise of Generative Semantics must have followed from factors essentially external to theory. More particularly, we concluded that, although each side in the disputes claimed repeatedly to

¹ We would like to thank Georgia Green, Randy Harris, Frederick Newmeyer, Frits Stuurman, and an anonymous reviewer for helpful comments on an earlier version of this paper.

² See also Goldsmith and Huck 1991, Huck and Goldsmith 1994.

have proved the other's hypotheses either false or of lesser scientific value than their own alternatives, crucial tests that would have distinguished the two programs on their scientific merits were not possible -- in large degree because the theories actually proposed were seriously incomplete and, even where precision was attempted, so plastic and subject to revision as to render the conceptual schema in which they had been generated practically immune to criticism. Since there was no widespread consensus about the status and relevance of data offered as evidence, counterargument could be and regularly was dismissed out of hand. Moreover, virtually all parties on both sides of the dispute can be seen not to have fully appreciated the capacity of the theoretical programs they were attacking to absorb and accommodate their critiques. In any case, as we pointed out, few linguists in the critical years and after were evidently persuaded to switch their allegiances on the basis of the evidence that had been accruing.

A central assumption we brought to this research was that differences between the Generative Semantics program and the Interpretive Semantics program can profitably be viewed against the backdrop of a longstanding tension in modern linguistics between mediational and distributional orientations of grammar. A mediational orientation is one that sees grammar as linking inner thought and outer form and that takes the task of the linguist to be the discovery of the nature of that link. A distributional orientation sees grammar as determining the patterning of linguistic units and takes the task of the linguist to be the discovery of principles governing that patterning, both in individual languages and cross-linguistically. While these orientations are not incompatible, different research programs may rank them differently. Thus, as we argued in *ILT*, Generative Semantics and Interpretive Semantics differed fundamentally in orientational structure: where Generative Semantics was primarily

mediationalist, Interpretive Semantics was distributionalist.³ It was this difference that we saw as behind much of the theoretical dispute between the two programs. But despite this difference, and although the Generative Semantics and Interpretive Semantics programs never produced identical theories with the same sets of core and auxiliary propositions, there ended up being interesting similarities in their respective overall conceptions of grammar.

One of the least surprising of the conclusions we reached in *ILT*, we thought, was that in the actual, workaday world, linguists tend to evaluate each other's hypotheses through a scrim of their own preconceptions, which allow them to derive inferences about those hypotheses that would not otherwise be available. The importance of external factors to theory has been argued extensively in the philosophy of science literature, and we had no substantive philosophical or methodological contributions to add to what has been fruitfully explored elsewhere. Instead, we were interested to point out where linguists were making assumptions during the Generative Semantics period that apparently limited the scope and force of their conclusions. Although we had no reason to think that our own work would be exempt from the processes we examined either in its conception or its reception, we admit to being intrigued by some unexpected interpretations of it that have appeared in recent reviews of *ILT*. Unfortunately, we see no way to remedy such interpretations except by retracing the steps we originally took, an exercise that would no doubt be as tedious for the reader as for us. There are, however, other issues raised by some reviewers of *ILT* that would, we think, benefit from further attempts at elucidation. We will discuss here three of

³ In *ILT*, we identified a third orientation that sees grammar as ultimately a psychological construct and that takes the task of the linguist to be to contribute to the development of psychological theories to account for linguistic behavior. As we noted, Interpretivist theory from its development in the 1960s was heavily psychologicalist in addition to distributionalist. It might have been useful if we had also pointed out that the psychologicalist orientation works on a somewhat different level from the distributionalist and mediationalist orientations. The distributionalist/mediationalist distinction gets at what one wants one's grammatical theories to be about, whereas the psychologicalist orientation is more concerned with how to interpret one's results. In this sense, the psychologicalist orientation is properly opposed to what might be termed a "platonic" orientation (see Katz 1981), which sees grammar, like mathematics, as existing in abstract space.

these issues that seem to us to have interesting implications not only for the history of linguistics but also for contemporary linguistic theory. Finally, we will have some concluding remarks on the term *ideology* as we intended it to be understood in *ILT*.

1 Deep Structure, the Generative Semantics Core, and Constraints on Derivations

The first of the issues we will take up concerns the Generative Semanticist's rejection of a structural level of deep structure mediating between the levels of semantic representation and surface structure. In contrast to the position taken in *ILT*, Newmeyer (1995:136) now argues that the constraints on derivations that the level of deep structure made available in an Interpretivist grammar "are (by definition) unavailable" in a Generative Semantics grammar.

Newmeyer devotes a considerable part of his critique to an attempt to show the summary in *ILT* of the core propositions of Generative Semantics as it established itself in 1967 "cannot possibly be right," because "a few short years after 1967, generative semantics had abandoned each and every one on the basis of empirical evidence" (Newmeyer 1995:133). We precisely agree neither with Newmeyer's understanding of what the Generative Semantics core propositions were nor with his assumptions about what might constitute abandoning them, but we certainly grant that there is room for debate about such questions. However, we have so far been unable to find in Newmeyer's argument anything of particularly general application to the central issue we were addressing in *ILT*, which was whether the adherents of Generative Semantics in significant numbers were rightly persuaded in the early- to mid- 1970s of the correctness of Interpretivist critiques of their program and consequently abandoned it in favor of Interpretive Semantics.

For example, Newmeyer contends that it cannot have been a core proposition of Generative Semantics that "structural differences among the grammatical sentences of a language can be accounted for by a set of phrase structure rules and the transformations that apply to them" (*ILT* p. 20), because by 1969 Generative Semanticists

were advocating global in addition to local derivational constraints. Similarly, he rejects as part of the Generative Semantics core the idea that the distribution of linguistic elements is determined by their structural properties, because Generative Semanticists came to believe that “the formal structure of language is derivative of extralinguistic content” (Newmeyer 1995:133). Ignoring the fact that these moves by Generative Semanticists did not involve accepting Interpretivist ideas that they had previously rejected, it seems to us no more appropriate to say, as Newmeyer does, that Generative Semanticists refuted these propositions than it would be to say that Interpretive Semanticists refuted the proposition that there is a level of deep structure in the *Aspects* sense. In these cases core ideas were developed and ramified via changing networks of auxiliary propositions to fit growing sets of facts while retaining their status as guiding hypotheses. We know of no evidence, for example, that Chicagoans during the Generative Semantics period ever ceased to hold with the idea that Generative Semantics “tries to explain the sense of sentences in grammatical terms,” as Sadock (undated:2) put it in the manuscript of an unpublished textbook on Generative Semantics written in the early 1970s, where the “grammatical terms” referred to were understood to include standard phrase structure trees and derivations. While from the start Generative Semanticists assumed that there were elements of meaning (e.g., those involving factual knowledge, the rules of inference, and discourse phenomena) that could not be handled in this way,⁴ they drew no *a priori* line between what was grammatical and what was not. Here, incidentally, Generative Semanticists apparently did not differ fundamentally from Chomsky, who had already said (1964:51), “it is not entirely obvious whether or where one can draw a natural bound between grammar and ‘logical grammar,’ in the sense of the Oxford philosophers.” In any case, we had understood the core propositions of Generative Semantics circa 1967 given in *ILT* to be

⁴ As against this, Newmeyer (1995:134-5) quotes G. Lakoff, who, in an appendix in *ILT* (p. 112), suggested that he had in 1963 adopted and then in 1968 given up the idea that “*all* aspects of meaning that are relevant to grammar can be represented symbolically in logical form” (emphasis added). But other Generative Semanticists appear not to have experienced such a radical shift in their views. McCawley in his first Generative Semantics paper (1968[1967]:68-9) recognized “features attached to the entire discourse” among other mechanisms that pretty clearly could have no place in sentence grammar, though most of his work then and since has been devoted to grammatical aspects of meaning. See also note 16 below.

consistent without revision with the idea that “the rules needed to determine what a grammatical sentence may mean are also needed to determine what is grammatical at all” (McCawley 1971). Generative Semanticists did come to embrace global and perhaps even transderivational as well as local constraints, but we would not have said that these innovations constituted a revolutionary change to the core ideas of Generative Semantics, or that the distinctions among local, global, and transderivational were core notions in that theory at all.

Other positions on the core differences between the Generative Semantics and Interpretive Semantics programs are of course possible. For example, Robert Freidin (1997:499) while accepting that the “core propositions [of Generative Semantics as given in *ILT*] are not incompatible with ... the core propositions of [Late Interpretive Semantics],” denies that there ever *were* core differences that were the subject of dispute between the two programs. Freidin (1997:496) further endorses Chomsky’s assertion (Barsky 1997:151) never to have taken part in any debates with Generative Semanticists, though he also notes that “Chomsky did not accept the specific analyses of G[enerative] S[emantics] that attempted to spell out these core propositions” (Freidin 1997:499). But whatever one wishes to call it, Chomsky did make his strongly negative assessments of Generative Semantics theories broadly known in three major published papers written between 1967 and 1969, at least two published interviews, numerous class lectures, and dozens of letters both personal and published. In these assessments, Chomsky repeatedly claimed that Generative Semantics either was saying the same thing as Interpretive Semantics or, if it were not, then to that extent it could not be correct. The general (as opposed to specific) character of this critique is in places quite manifest: as we point out in *ILT*, in personal correspondence with McCawley in December of 1967, Chomsky laid out what he characterized as “a near-definitive disproof of any conception that might be worked out within the general range of ideas that you hint at.” This claimed disproof was published in Chomsky 1971:197-8, where he asserted that “an argument of this sort can be advanced against any variety of semantically based grammar (what is sometimes called ‘Generative Semantics’) that has been discussed, or even vaguely alluded to in the linguistic literature.” This argument - however incisive it was - was not decisive (or rather, it would have been decisive only against a particular version of Generative Semantics that McCawley did not hold; see *ILT*, pp. 31-2).

Newmeyer's analysis assumes that constraining theories should not be an incidental interest of the theoretician, and that theories that lack effective constraining mechanisms are *ipso facto* less desirable. This precept has been given prominent play in generative linguistics, though to our mind without adequate justification; we return to this below. We do agree that Generative Semanticists were much less concerned than Interpretive Semanticists with the importance of constraining the power of their theories. Still, it needs to be emphasized that Generative Semanticists did allow for the specification of syntactic constraints on surface structure that would incorporate some of the important constraining functions of deep structure in an Interpretivist theory.

As we had pointed out in *ILT* (pp. 20, 146), Generative Semanticists, beginning with G. Lakoff and Ross 1976 [1967], generally assumed in their writings that the arguments originally offered by Chomsky (1965) in defense of a level of deep structure could be shown to be inconclusive at best in light of what had subsequently been learned about semantics. That they were reasonably justified in this belief is suggested by the fact that they were soon joined by Interpretive Semanticists who embraced Trace Theory in the early 1970s and almost all formal syntacticians since who have not found reason to recreate deep structure in their theories as originally construed. Interpretive Semanticists did, however, continue to assert the existence of a level of deep structure *within syntax* that, in conjunction with the system of transformations and the constraints on them, among other things exhaustively determined permissible surface structures. A question that G. Lakoff and Ross had left unasked in their 1967 prospectus was whether deep structure itself played any significant role in the correct specification of surface structure or whether the mechanisms relating surface and semantic structure would effectively dispose of this. Early on, however, it became apparent that there were at least some surface syntactic patterning conditions that could not be given general statement by means of standard transformations operating on syntactic representations (much less semantic ones). Inspired primarily by the work of Ross (1967) and Perlmutter (1971[1968]), Generative Semanticists and Interpretive Semanticists alike had begun by 1968 to posit, without attempting an explicit and elaborated theory of them, various devices that had the effect of constraining the output of the transformational component. Some of these devices, like Ross's (1972) "Doubl-ing" constraint, functioned

as filters that ruled ungrammatical certain surface structure configurations that would otherwise be produced in derivations.⁵

Other devices, however, enabled Generative Semanticists to constrain the generation of surface structures in their theories in a much more substantial and systematic way. For example, Green (1970), noting that a variety of different semantic structures could be associated with the same surface syntactic construction, argued that there must be a limited set of syntactic “target structures” into which the transformational rules map their representations. The sentences *She shot him dead* and *They buried him alive*, she argued, both share the same superficial syntactic structure, but crucially differ semantically as to whether the adjective indicates a pre-existing state or a result. As she pointed out, “natural language syntax is free to utilize mechanisms by which a large and diverse set of logical and semantic relations are somehow squeezed into a small number of surface structures” (Green 1970:277). In that paper, she referred to such mechanisms as “conspiracies.”

The idea of conspiracies in phonology had been introduced in a number of papers in the late 1960s and early 1970s by Kisseberth (1970a,b), Lovins (1971), and Sommerstein (1974), among others, who argued that the generative notion of derivation was in need of modification and that grammars could be simplified by noting certain formally simple surface constraints. Considering this literature, Kiparsky (1973) sought an account of why “the output of the phonological component of a language often shows clear-cut regularities which do not necessarily have a single source in any particular rule of the system, but are the results of complex interaction among several seemingly distinct processes or limitations on processes.” One of Kiparsky’s proposals involved the development of the notion of rule transparency: a rule of the form $X \rightarrow Y / A _ B$ would be rendered less transparent (equivalently, more opaque) by the appearance of sequences $A X B$ on the surface. If it could be shown

⁵ The “Doubl-ing” constraint was designed to rule out consecutive occurrences of words ending in *-ing*, as in **It is continuing raining* or **He’s going drinking beer*. In Ross’s formulation, this constraint was not simply a surface structure rule but rather one that also incorporated a condition on more remote structures. Subsequent work suggested that the rule operated at surface structure only (see Milsark 1972).

that opacity is quantifiable, and that grammars eschew opacity beyond a certain point, then it might be said that this would “explain” why rules tended to work jointly in such a way as to produce certain surface patterns, even in the absence of any formal statement of this process within the grammar. However, Kiparsky’s program did not quite reach the goal he had set for it. Some aspects of that stage of his work were later integrated into lexical phonology, though none of what had been designed to speak to the issue of apparent surface targets.⁶

Although Green’s analysis of *V NP Adj* structures threw significant new light on the range of meanings they can encode, it was nonetheless generally inexplicit as regards a solution to the theoretical issues that it explored. She provided no rules, nor did she show in any concrete way how the targets and conspiracies she had alluded to might operate in a Generative Semantics grammar. In her thesis (Green 1974[1971]), she went on to provide considerably more evidence concerning the complex relation between semantic representation and surface structure, particularly as regards so-called “dative-movement constructions,” while still finding it inappropriate to specify particular rules or constraints. The central problem addressed in the thesis was how to account for the fact that some predicates freely allow both a prepositional dative (as in *She gave the book to John*) and a

⁶ Phonologists’ concerns in the 1970s about the inadequacies of a relatively simple derivational model, as proposed in Chomsky and Halle 1968, were developed into three distinct themes of phonological theory. Conspiracy effects (most notably of the sort identified by Lovins 1971) were one of the considerations that led to the development of autosegmental theory (Goldsmith 1976); conspiracies of the sort identified by Kisseberth 1970a,b led to the conception of lexical phonology (by Siegal, Strauss, Kiparsky, Mohanan, Pulleyblank, and others; see Goldsmith 1990, chap. 5), with its “structure-preserving” constraint on phonological processes inside the lexicon (intentionally echoing Emonds’s notion); and, most radically, Kisseberth’s and Sommerstein’s (1974) view of the significance of surface-defined constraints led to a series of attempts to develop a view of phonological theory dominated and motivated by violable surface constraints. This scholarly tradition (See Goldsmith 1990:319ff, 1993b passim, or most recently, the contributions in Roca 1997) has had by far the greatest impact through its development in Optimality Theory (Prince and Smolensky, forthcoming), which has had considerable success in showing that effects that had been interpreted as motivation for a derivational account and an underlying representation could be radically reinterpreted as the result of an interaction of violable, but ranked, surface constraints.

nonprepositional dative, (as in *She gave John the book*), whereas other predicates sometimes allow only one or the other (*She donated the book to the museum*/**She donated the museum the book*; *Measles germs give you measles*/**Measles germs give measles to you*). Again, Green appealed to conspiracies and targets as a hypothesis to explain why a broad range of distinct semantic structures is channeled into just two canonical surface structures, *V NP NP* or *V NP PP*. It will become important in what follows to note here that any number of particular instantiations of the concept of conspiracy might have sufficed to accomplish this channeling, but Green apparently did not have in mind arbitrary devices like rule features (Klima 1964, Chomsky 1965, G. Lakoff 1970[1965], Chomsky and Halle 1968), which, she argued “totally fail to characterize the notion ‘exception to a rule’” (Green 1974:18). Although Green spoke routinely of “[semantic] conditions on syntactic rules” (1974:207), her thesis was devoted to eliminating arbitrary descriptive devices in favor of more explanatory mechanisms (1974:194).

Before Green’s work, it was assumed (following G. Lakoff 1970[1965] and R. Lakoff 1968) that exceptions to the set of identified transformational operations were to be accounted for by redundancy rules that assigned rule features requiring or permitting or prohibiting particular transformations from applying in the environment of certain lexical or semantic markers. Accepting the standard Generative Semantics assumption that semantic classes can be structurally defined and that syntactic rules apply to semantic structures, Green proposed that exceptions be analyzed as following from structural limitations in the mapping from semantics to surface structure. In this context, conspiracies and target structures might be most naturally thought of as elements of the mechanism limiting that mapping. What was lacking, however, was any theory of that mechanism.

The inexplicitness of Green’s idea about surface structure constraints implied little about its utility in a Generativist theory, but it did pose problems for an evaluation of her specific contribution. In a review of the published version of Green’s thesis, for example, Richard Oehrle (1977:200) compared her use of the term “conspiracy” with the way it had come to be employed in phonology and found the former “decidedly peculiar.” Referring to Kiparsky 1973, Oehrle noted his understanding that in phonology a conspiracy was usually conceived

of as an output condition implicit in the phonological component as a whole,⁷ although “insofar as it is clear at all” on his understanding of Green’s account, her “conspiracies are not treated as a property of the syntax as a whole, but rather as properties of various subparts of it” (Oehrle 1977:200). Whether or not Oehrle’s speculation about Green’s particular conception of conspiracies was justified,⁸ it is important to note here that he did not demonstrate something about Generative Semantics theories in general -- for example, there was nothing internal to Generative Semantics that would have prevented the statement of perfectly general principles to constrain the association of semantic representations and surface structures in the relevant cases. His broadest conclusion was

⁷ In retrospect, there seems to be no grounds for this observation. As argued in Goldsmith 1993a, for example, what does appear to be critical is that conspiracies – linkages between rule application and target structures – involve targets at specifiable linguistic levels (e.g., the output of the lexical phonology or the surface phonetic form). Any rule which could affect such structures (which is to say, just about any phonological rule) would be grist for a ‘conspiracy’ mill. Nor has history entirely vindicated some of Oehrle’s other judgments about conspiracies. For example, he stated at one point that “the only justification for introducing the notion ‘conspiracy’ is that it allows rules to be simplified by eliminating contextual information and places constraints on the form of new rules added to the grammar.” However, it had become reasonable by the end of the 1980s to view rules in terms of their function in repairing illegitimate structures (see, e.g., Singh 1987 and Goldsmith 1989), and many phonologists would now say that the most important result of the introduction of conspiracies and surface target structures was that they eventually enabled phonologists to see that the whole derivational program was a house of cards best left to collapse.

⁸ As suggested above, however, Green would almost certainly have not endorsed Oehrle’s statement that “conspiracies are not treated [by Green] as a property of the syntax as a whole, but rather as properties of various subparts of it, and ... no claims over and above what is stated in each rule can possibly follow.” Oehrle indicated that he was probably not understanding Green’s proposal in the way that she was when he claimed that, in the case of the dative-movement conspiracy, “the conspiracies alluded to have mutually contradictory targets” (Oehrle 1977:200). That is, he was presumably predisposed to find nothing but ad hoc stipulations on rules, although it is clear from our vantage point that such was not entailed by her account.

that “G[reen]’s ‘conspiracy’ is merely the name of a difficulty” -- viz., the difficulty of explaining why semantic forms are channeled into the particular syntactic forms they are-- though he might as well have added that this was a difficulty at the time equally for all approaches, including the Interpretive Semantics theories he advocated.⁹

At about the time that Green was writing her dissertation, Joseph Emonds (1969, 1970, 1976), investigating within an Interpretivist framework why transformations seemed in general to produce outputs that could have been produced by the rules of the syntactic base, proposed that transformations -- with the exception of a restricted class -- did not change structure. It is perhaps more obvious now than it was then that Emonds’ and Green’s proposals had similar implications for their respective grammars. At any rate, it was apparently not until McCawley 1988:291 that an explicit recognition and substantive exploration of the similarity of such proposals appeared in print. McCawley, in his own analysis of target structures, proposed a selection of twenty “surface gross combinatoric rules” for English that -- ignoring irrelevant details -- could as well have served as the rewriting rules of an Interpretive base yielding standard prelexical deep structures. McCawley (1988:294) further introduced notational conventions into the statement of these rules that served to describe generalizations over them as well as to allow for the incorporation of specific surface restrictions -- both functions standardly accommodated at the deep structure-surface structure interface of an Interpretive grammar.

Considering an observation in *ILT* (p. 146) that the Generative Semanticists’ rejection of a level of deep structure did not rule out syntactic conditions on surface structure in their grammars which might require mechanisms roughly analogous to postulating such a level in Interpretivist grammars, Newmeyer argues in his

⁹ Oehrle’s Interpretivist alternative to Green’s Generative Semantics-inspired proposal was essentially the status quo ante - i.e., the redundancy-rule approach, originally proposed by G. Lakoff (1967[1965]) and R. Lakoff (1968). According to such an approach, rule government conditions specified in the lexicon might be determined by semantic class - e.g., a verb of the *give* class might be marked as optionally permitting dative-movement unless the subject is inanimate, etc. However, this approach had little to say about the way semantic representations were mapped onto syntactic representations: i.e., in this approach “the association of formal operations with semantic classes appears to be arbitrary” (Oehrle 1977:202).

review that “the nonexistence of a syntactic level of deep structure” constituted “a ‘core principle’ of Generative Semantics” (Newmeyer 1995:135).¹⁰ Moreover, he asks, “[s]ince the key defining feature of the level of deep structure was that it served as input to the transformations, how could conditions on surface structure be ‘roughly analogous’?” (Newmeyer 1995:136). An answer to that question had already been suggested in Newmeyer 1980:165, where he had referred to Generative Semantics templates or filters which specified morpheme order as “a sneaking-in of an important aspect of deep structure.” However, as indicated above, Green’s and McCawley’s target structures did not simply specify the linear order of morphemes; they specified category membership and hierarchical relationships as well. Thus, a fully developed target system in their sense presumably would specify the full set of surface structure configurations allowable in the grammar. By comparison, Emonds’ “Structure Preserving Hypothesis” ensured that surface structures in the relevant cases did not deviate from the typology of configurations permitted at deep structure. As McCawley (1988:291) argued, however, the facts cited by Emonds might more successfully be explained by specifying that typology at surface structure.

¹⁰ In *ILT*, we had not included a commitment to the nonexistence of a level of deep structure among the core principles of Generative Semantics. Although “negative” propositions of this sort may accurately characterize certain deeply felt predispositions of program members, they also have characteristics that distinguish them from standard positive core propositions. For one thing, a negative proposition is usually felt to require justification on the basis of some positive core principle. The Generative Semanticists’ resistance to deep structure, for example, was tied to their commitment to the more general principle of parsimony. As Postal (1972:137-8) put it, “any theory which claims the existence of special levels of structure beyond surface structure and semantic representation in grammar” requires “special empirical justification.” In much the same way, Interpretive Semanticists rationalized their opposition to global rules by invoking criteria of restrictiveness. Furthermore, unlike their positive counterparts, negative propositions invite narrow interpretation. As we pointed out in *ILT*, despite their opposition to deep structure, Generative Semanticists were quite ready to admit syntactic constraints on surface structure, just as Interpretive Semanticists resorted to global rules in the interface with semantic representation to account for scopal phenomena. See *ILT*, p. 38.

In rejecting this analogy, Newmeyer refers to Oehrle's critique of Green's treatment of targets, but draws conclusions that do not, so far as we can see, strictly follow from either. Thus Newmeyer states:

As Oehrle (1976, 1977) noted, such target structures could not be properties of the grammar as a whole, but only of the subpart relevant to a particular construction subject to it. Hence, each rule in the grammar would have to be marked with a feature of some sort to indicate whether or not it was subject to a particular conspiracy/target structure. A theory that has a level of deep structure as *input* to the syntactic rules suffers no such problem (Newmeyer 1995:186; see also Newmeyer 1980:166, where a similar claim is made¹¹).

However, as inspection of it will confirm, Oehrle's claim was rather that Green's syntactic conspiracies did not have a property that he associated -- correctly or incorrectly -- with phonological conspiracies. Oehrle did not demonstrate or even assert that target structures in a Generative Semantics grammar could not have that property, nor had he demonstrated that in the mapping from semantic representation to syntactic representation Generative Semanticists would inevitably encounter problems avoidable in an Interpretive Semantics grammar.

¹¹ Except that Newmeyer's (1980:166) claim is stronger. There Newmeyer referred to Green's "14 underlying semantic classes of Dative Movement predicates" and her "14 rules" for generating surface structures and compared them unfavorably with "a model with deep structure [which] can express this surface generalization very simply" via a single phrase structure rule that rewrites a *VP* as *V NP PP* and a single transformation that transforms *V NP PP* to *V NP NP*. However, even before Green did her work it was well known that proposals like Newmeyer's were inadequate without the addition of a rather unattractive stipulative mechanism. The problem was that many of the dative movement predicates in some or all contexts had defective distributions -- sometimes the *V NP PP* alternative was not possible, while in other cases the *V NP NP* alternative was not. Green's semantic analysis attempted to account for those distributions. The conspiracies she postulated were supposed to be responsible for getting a particular semantic representation into a particular surface syntactic configuration, but this did not necessarily require "mark[ing] each rule of the grammar with a feature of some sort to indicate whether or not it was subject to a particular conspiracy/target structure" (Newmeyer 1980:166). In any case, the defective distributions were supposed to fall out from independent principles. While Green did not demonstrate that an adequate grammar could be constructed in this fashion, neither Oehrle nor Newmeyer demonstrated that it could not be.

Because her dissertation was not published until the very close of the Generative Semantics period, the implications of Green's work for Generative Semantics theories went for the most part unexplored. Oehrle's subsequent critique, as we've argued (see *ILT*, pp. 54-8), did not fully come to grips with Green's argument, much less was it prepared to examine its potential within the context that Green had been assuming. Moreover, there was apparently nothing in Green's suggestion that syntactic generalizations may hold at surface structure – that particular surface configurations may serve as targets for traditional transformations – that was at odds with the larger Chomskyan project for generative grammar, where Universal Grammar was conceived of as, *inter alia*, a formal device for grammar compression, for stating grammars at their level of greatest generality.¹² Looked at in this way, Green's conspiracies, like Emonds's Structure Preserving Hypothesis, constituted a potentially valuable technique for grammar simplification, though this is not to say that the two approaches would necessarily have

¹² While it would take us too far afield to discuss this point at any length, it should be noted that the idea as it has been developed in a number of fields is that the formal length of the most highly compressed form of a theoretical description is an important measure of it, regardless of how abstract that description is (including, that is, linguistic grammars). There is a long tradition of work in mathematics and computer science in this area going back to Kolmogorov (who founded a subfield devoted to its study in the 1930s), Solomonoff, and Chaitin, and it is intimately connected with information theory (of the sort pioneered by Shannon and Weaver 1949). A number of popular introductions exist; a standard (but mathematically challenging text is Li and Vitányi 1993, which has a good discussion of the field's historical roots. Recent work by Rissanen, in a Minimal Description Length framework (1989) follows in this tradition; this work has been applied to some problems in linguistics (Rissanen and Ristad 1994, de DeMarcken 1996). Work in other traditions may be consulted as well, including that of Henri Atlan and his associates; an overview can be found in Fogelman-Souli ■ 1994. The reader unfamiliar with this material should perhaps be forewarned that the notion of compression invoked here has nothing to do with so-called “lossy” compression, in which details of the original are lost during compression; the notion of compression that is relevant is “lossless” compression, in which redundancy is extracted from the description and nothing more. Passages such as pp. 394-99 of Chomsky and Halle 1968 must be read in such a light.

produced identical results. McCawley (1998:5), for example, takes issue with *ILT* for claiming that his module of surface combinatorics provided “a Generative Semantic equivalent of Emonds’s theory of structure preservation” (*ILT*, pp. 50-1), since his theory, but not Emonds’s, allows for the statement of restrictions on how constituents can combine at surface structure that have no counterpart in restrictions on deep structure. It remains an empirical question whether, over the full range of data, either surface-structure-based constraints or deep-structure-based constraints can be entirely dispensed with. But our point was that for the cases investigated during the Generative Semantics period, there appeared to be no instances of significant deep structure generalizations that could not be given a surface structure analysis in the Green/McCawley sense.

2. Deep Structure, Surface Structure, and Semantic Representation

Newmeyer 1995:130-2 and Freidin 1997:499 dismiss any similarity between Generative Semanticists’ level of semantic representation and Interpretive Semanticists’ level of LF on the grounds that “LF representations ... are not semantic representations” (Freidin 1997:499) but rather interact “with other cognitive structures” (Chomsky 1975:105) to produce the full semantic reading of a sentence. However, the initial trees that Generative Semanticists worked with were generally not intended to provide full semantic readings either. In Lakoff’s (1971a) formulation, for example, “S[emantic] R[epresentation] = (P₁, PR[esuppositions], T[opic], F[ocus], ...),” where P₁ indicated the initial tree. For the most part, and despite several well-advertised attempts (see, e.g., Lakoff 1971b, Morgan 1973), the formal relation between P₁ and the other elements of meaning (especially those areas covered by the ellipsis) was never satisfactorily worked out by Generative Semanticists,¹³ just as the formal relation between LF and “other cognitive structures” had been ignored in most Interpretivist work (but see Jackendoff

¹³ McCawley (1975:75), commenting on Lakoff’s formulation, observed that “it is an empirical issue whether any of the items following P₁ is actually needed in semantic representation; in the event that they are not, then a semantic representation is a phrase-marker.”

1983 for a notable exception). As Chomsky (in personal correspondence with John Searle, July 19, 1972) pointed out, “There is little if any difference between saying that, e.g., focus (an element of SR) is related to surface structure [SS] by a rule of semantic interpretation such as the ones I suggested, or by a derivational constraint stating that the derivation is acceptable only if this element of SR happens to appear in SS in the manner stated in the corresponding rule of semantic interpretation... [The Extended Standard Theory] and [Generative Semantics] do not differ, except notationally, on this matter.” In McCawley’s work through 1971, one of the most significant differences between the semantic structures he was working with and later Interpretivist treatments of LF was that McCawley did not segregate lexical and morphological processes from syntactic processes. However, this difference does not seem to us to reflect great and ineluctable differences in the core ideas of the two programs. As we pointed out in *ILT*, some Interpretivists have argued for the incorporation of syntactic treatments of such “prelexical” processes in the grammar. And in Chomsky’s more recent work (1995), the form of LF is argued to be specified by “conceptual necessity” -- which we interpret to mean that the particulars of any theory of LF are to be supplied by auxiliary hypotheses.

While it is to be expected that numerous details of application with important empirical consequences would have divided Generative Semantics and Interpretive Semantics theories of semantics, the issue we were interested in was whether the structure of the respective theories encouraged roughly similar approaches to prominent problems. For example, the introduction of LF, as Chomsky observed in Chomsky 1975:116, allowed Interpretivists to make concrete proposals for dealing with “scope of quantifiers and logical particles, anaphora, presupposition, and the like.” Essential to LF as thus conceived were that (i) logical scope is represented structurally, (ii) identity of reference is correlated with coindexing at that level, and (iii) surface structure is related to LF via transformations constrained by principles of the familiar sort. In all these respects, LF directly paralleled the Generative Semanticists’ conception of logical or semantic structure, so that the LF representation circa 1977 of a sentence like *John saw everyone* would in all relevant respects be identical to the corresponding initial tree in a Generative Semantics derivation of that sentence. It should be emphasized that correspondences of this sort were not inevitable. Montague (1973), for example, argued for a direct model-theoretic interpretation of surface

structure, without an intervening level of LF. Although Montague did not demonstrate that the natural language phenomena that linguists of the time had been working with could be handled in this fashion, his hypothesis has in recent years been extended with some success to cover antecedent-contained deletions and other constructions that have traditionally been thought to require LF (see Jacobson 1992; see also Fauconnier 1985). At the very least, this suggests that when Interpretivists adopted LF they were thinking along much the same lines as Generative Semanticists were.

In Newmeyer's comparison of Generative Semantics and Interpretive Semantics, as in many similar comparisons by Interpretivists during the period of the disputes, there are crucial differences in the way certain terms are to be understood with respect to the two programs. In the context of a Generative Semantics theory in Newmeyer's passage cited on p. 000 above, for example, "grammar" evidently denotes the system generating semantic representation and its relation to surface structure. The implicit comparison is with an Interpretive theory where "grammar" would refer to the systems of deep structure and surface structure and the relations between them.

As we pointed out in *ILT*, ambiguities in terminology often constituted a significant source of misunderstanding in the debates between Generative Semanticists and Interpretive Semanticists. Probably no technical term contributed more to such misunderstandings than "deep structure." In the *Aspects* theory, deep structure was defined as, *inter alia*, the input to the syntactic transformations and the level at which lexical insertion was accomplished. A clear distinction was then drawn between the rules relating deep structure and surface structure (syntactic transformations) and those relating deep structure and semantic representation (semantic interpretation rules). The theory of transformations was supposed to take care of distributional issues, while the theory of semantic interpretation rules -- left by Chomsky at that point fairly undeveloped -- was to be responsible for associating deep structures with their appropriate meanings. As early Generative Semanticists quickly discovered, the assumption that paraphrastic constructions are related by transformation led to a deepening of the concept of deep structure to the point that there eventually appeared no reason to maintain the distinction between it and semantic representation at all. After G. Lakoff and Ross pointed out the deficiencies in the *Aspects*

definition of deep structure, Generative Semanticists felt the appropriate theoretical move was to dispense with the level entirely. However, despite this, they did not entirely dispense with the *term* “deep structure,” which they continued to employ on occasion to denote the most abstract level in their grammars related to surface structure via syntactic transformation. Meanwhile, Interpretive Semanticists, having insisted on retaining deep structure as the input to the syntactic transformations, first proposed that semantic interpretation rules apply to surface structure and possibly intermediate levels as well as to deep structure, and then concluded that only surface structure is interpreted. These theoretical moves are illustrated in figure 1.

Insert figure 1 about here

It is important to note that, while in the *Aspects* system syntactic transformations and semantic interpretation rules were formally and functionally distinct and applied at different grammatical levels, in the full development of both Generative Semantics and Interpretive Semantics these two types of rules had coalesced on the semantic axis, where a single type of transformational rule relating semantic representation (logical form) with surface structure now performed both mediational and distributional tasks. This development reflected the achievement of a widespread consensus among linguists at the time that not all apparently distributional facts could be described by a single set of rules that had no access to semantic information. At the same time, as suggested above, recognition by Generative Semanticists of the need for autonomous syntactic surface structure constraints in their grammars reflected similar unanimity that not all distributional facts followed from exclusively semantic conditions.

These particular similarities among theories of the two programs were not, however, much appreciated at the time – and there are fairly transparent reasons why they went for the most part unnoticed. For one thing, adherents of the two programs had significantly different priorities, as we suggested above. For Interpretivists, the important action was at the interface of deep structure and surface structure, the horizontal “B” leg in figure 1. Generative

Semanticists were far more interested in the vertical “A+B” leg where semantic representation interfaced with surface structure. What clouded the distinction was the Generative Semanticists’ tendency to refer to semantic representation in their theories as deep structure, as well as their admitted interest in finding semantic solutions to distributional issues. Since Interpretivists and Generativists were often working on what appeared to be similar problems, the fact that they were doing so from correspondingly different legs of their respective grammars was obscured, and the appearance was given that they had arrived at conceptually incompatible - albeit empirically and theoretically comparable - ways of doing syntax.

What corresponded to the Interpretivists’ “B” leg was not the Generative Semanticists’ “A+B” leg, although virtually all criticisms of Generative Semantics by Interpretive Semanticists that described it as less constrained and less theoretically elegant than Interpretive Semantics hinged upon such a comparison. For example, the fact that Generative Semanticists admitted “arbitrary” (i.e., global) rules in their grammar was generally considered by Interpretivists a decisive factor in favor of an Interpretive theory, which they argued did not. However, Generativists’ global rules were involved in the semantics-syntax interface (their A+B leg), while Interpretivists’ claims about the more limited power of their own rules generally applied only to the deep structure-surface structure interface (the B leg), and the fact that they envisioned the equivalent of global rules on their A + B leg was ignored by them as not relevant to this issue.

Perfect understanding of each others’ models was apparently very difficult to achieve, even where the same leg of the compared models was at issue. In a book expressly about logical form, for example, Robert May (1985:4-6) argued for an approach in the Interpretivist tradition in which the sentence *John saw everyone* would have the Generative Semantics-like LF representation $[[everyone][John\ saw\ [e]]]$, where the quantifier *everyone* was taken to bind and be coindexed with the trace *e*. The LF representation was to be derived from surface structure via the application of a transformational rule -- called “QR” or “Quantifier Raising” -- that moved the quantifier from its surface position embedded in its verb phrase to a LF position adjoined to the sentential node, leaving the trace *e* in its place. The Trace Theory of binding that he advocated would demand that any trace at LF be bound by some binding element, like a quantifier, that commanded it. In a note appended to his discussion of these issues, May

compared his approach with a Generative Semantics approach, in which a semantic representation similar to May's LF representation would be mapped by the transformation of "Quantifier Lowering" onto the surface structure *John saw everyone*. As to this, May says:

Note that assuming trace theory to apply generally to movement operations rules out the possibility of deriving something like S-Structures from something like LF by "lowering" quantified phrases, since such movements would give rise to structures containing unbound traces. Thus the approach here is materially distinct from that of Lakoff (1971[a]), for example. (May 1985:158)

But from the perspective we have taken here, May's approach can be seen *not* to be materially distinct from Lakoff's Generative Semantics approach. Perhaps influenced by the directional terms "raising" and "lowering," May apparently assumed that in the context of Trace Theory a rule of Quantifier Lowering operating on a semantic representation with a quantifier in "raised" position would leave an unbound trace, as if it were a rule transforming either deep structure into surface structure or surface structure into LF in a standard Interpretivist grammar. But the Generative Semanticists' rule of Quantifier Lowering "lowered" a quantifier only relative to a perspective from semantic representation. Viewed from surface structure, Quantifier Lowering "raised" a quantifier, leaving a bound variable equivalent to a trace. That is, on the dimension May was evidently interested in, QR and Quantifier Lowering had virtually the same effect -- they both associated structures containing bound variables at the semantic level with the appropriate trace-less surface structures.

In much the same way as May, Newmeyer in his review of *ILT* "inverts" Generative Semantics theory when he attempts a comparison of QR and Quantifier Lowering. The Interpretivist account, he says, "had no need for a rule of quantifier lowering, a rule typologically unlike the bulk of others that had been proposed...Quantifier raising did not present the typological problems that lowering did" (Newmeyer 1995:131-2). The problem, again, is in viewing Generative Semantics from the perspective of an Interpretivist grammar and imposing on it a directionality that is inappropriate to it. (Indeed, the imposition of any directionality in a derivation would on the standard view be inappropriate, since a derivation is usually understood to be, not a device for representing how one thing is

changed into another; but rather a formal account of the relation between two or more representations.) Newmeyer is correct to suggest that in Interpretivist theories rules that transformed deep structures into surface structures generally did not “lower” elements into subordinate clauses. But as we have seen, Quantifier Lowering was a rule operating at the semantic representation-surface structure interface, not the deep structure-surface structure interface, and it posed there no special typological problem that QR avoided.¹⁴

For all the attention devoted to the hypothesis that transformations operate prelexically, the question of whether lexical insertion on the “B” leg (the Interpretivist option) was independently preferable to insertion on the “A+B” leg (the Generativist option), or vice-versa, was never really addressed. In either case, information in lexical entries had to be carried “round the bend” of surface structure one way or the other, an inefficiency that a compact grammar may well not be able to afford. Similarly, Generative Semanticists treated relation-changing operations on the “A+B” leg, while Interpretive Semanticists tended to collect them on the “B” leg. In her thesis, Green proposed a number of arguments supporting a Generative Semantics strategy, a strategy Oehrle vigorously opposed. But the issue -- which remains unresolved -- can be seen now to have been technically independent of

¹⁴ Newmeyer (1995:132) additionally makes the following claim: “it turned out that the relationship between the moved quantifier and its point of origin was subject to the same kinds of constraints as those between moved elements and their points of origin, i.e., constraints on antecedent-trace configurations. Such constraints were literally inexpressible in generative semantics, which disallowed traces.” We see two serious problems with this argument. First, that quantifier-variable relations are the same as antecedent-trace relations is not a fact but a hypothesis, and the facts in their totality are such that no hypothesis currently accounts for them. No competing theory is of course obliged to take the Interpretivist approach, only to attempt to cover an equivalently wide array of facts. Second, no one today, obviously, is in a position to say what mechanisms Generative Semantics might or might not have developed in its analyses of the facts. It may be true that theories in the Interpretive tradition today account for a wider array of facts in this domain than those of Generative Semantics a quarter of a century ago, but this does not necessarily point to some *theoretical* deficiency in Generative Semantics. To show such a deficiency, one would have to show that Generative Semantics was *incapable* of developing an account over an equivalently wide array.

the dispute between the programs, since any particular sentence alternation might have been accounted for on either leg of a theory in both. A showing that semantic information is irrelevant to a satisfactory description of the great majority of sentence alternations would have called the Generative Semantics approach into question, just as a demonstration that semantic properties for the most part determine syntactic properties would have dealt the Interpretivist approach a serious blow. Although much effort was expended in promoting and defending against those antitheses, no convincing case in our view was ever made for one over the other and the consensus now seems to be rather that surface distributional patterns are determined by a complex interaction of pragmatic, semantic, syntactic, morphological, and phonological features.

3. Learnability issues and non-issues

Freidin (1997:497) in his paper touches on the question of learnability, asserting that “[i]t did not seem that the analyses of G[enerative] S[emantics] and the powerful descriptive devices they employed would lead to the kind of theory that could address the problem of language acquisition.” Here, for example, is how Chomsky put it in 1972 (Chomsky 1972a:125):

...The fundamental problem of linguistic theory, as I see it at least, is to account for the choice of a particular grammar, given the data available to the language-learner... To account for this inductive leap, linguistic theory must try to characterize a fairly narrow class of grammars that are available to the language-learner; it must, in other words, specify the notion "human language" in a narrow and restrictive fashion. A "better theory", then, is one that specifies the class of possible grammars so narrowly that some procedure of choice or evaluation can select a descriptively adequate grammar for each language from this class, within reasonable conditions of time and access to data... For the moment, the problem is to construct a general theory of language that is so richly structured and so restrictive in the conditions it imposes that, while meeting the condition of descriptive adequacy, it can sufficiently narrow the class of possible grammars so that the problem of choice of grammar (and explanation, in some serious sense) can be approached.

The problem that Chomsky was alluding to was that children in a few short years of exposure to their language are able to induce a functional (hence, by hypothesis, correct) grammar, while at the same time it seemed to him that more “powerful” grammatical theories (in the sense that they could describe a wider range of grammars) would be less well equipped to serve eventually as the basis for an account of language acquisition. On Chomsky's view, even if only the formally simplest grammars were admitted that were consistent with the data the child receives, the number of incorrect grammars that would have to be explicitly rejected by the Language Acquisition Device seemed to be far too great. Grammar acquisition, on this view, would be much like looking for a needle in a haystack,¹⁵ but a haystack spread out in a hundred dimensions or more (the number of independent variables that may be specified in a grammar). Would it then not be a step in the right direction to radically decrease the number of grammars the child is presumed to be considering during the acquisition process? Because so little was then known about the process of language acquisition, probably the most judicious answer as of 1970 would have been “Perhaps.” Of relevance here is that Chomsky's answer at the time was “Yes, definitely,” and that that answer formed a major instrument in the offensive against the Generative Semantics program

We know today a great deal more about machine learning (in terms of which the problem of grammar acquisition has generally been conceived), with a range of techniques now having been explored, from genetic algorithms to simulated annealing, as well as parameter-learning in spaces of low dimension.¹⁶ In general, it is not the case that decreasing the size of the search space is the best way to solve a problem; typically, solutions to learning problems involve better means for intelligently moving long distances in the space of grammars quickly, or “hill-climbing” locally, using the relative independence of parameters and a continuous measure of success to find a local maximum. It might have seemed common sense in 1970 that the task of language acquisition would

¹⁵ The classic article assuming that learning involves going sequentially through an enumeration of grammars is Gold 1967.

¹⁶ On the first two techniques, one may see the excellent surveys in Bishop 1995 and Ballard 1997; a recent example of the latter is Niyogi and Berwick 1996.

rendered easier if there are fewer grammars that the human brain can process, but that had by no means been established to be true -- and is now, we believe, unlikely in the extreme.¹⁷

McCawley's position during the Generative Semantics period was that even granting Chomsky's assumptions about the way language is acquired, and despite the efforts that had been made to constrain Interpretive theories of syntax, it had not been shown that Interpretive Semantics grammars overall would require less powerful devices than Generative Semantics theories or that the class of Interpretivist grammars had been restricted in a way that facilitates choice of grammars (see, e.g., McCawley 1975:72-4). But the case against accepting at face value the Interpretivist view of the inferiority of Generative Semantics on learnability grounds can now be seen to have been much stronger.

4. Ideology in Linguistic Theory

In view of its prominence of the term *ideology* in our title, it was probably unfortunate that we did not include in *ILT* an explicit discussion of our understanding of its sense and meaning. This term has had a long and complex history,¹⁸ with much of its present impact derived from the use made of it by Marx and by later Marxists (though they did not use it in the same way). When coined by Destutt de Tracy late in the 18th century, it was intended to

¹⁷ There is some irony here as well, because by 1970 it was already clear to some, including Chomsky, that the critical factors governing acquisition could not be pretheoretically determined, just by keeping one's eyes open, so to speak. Chomsky often remarked that he had no difficulty in theory in viewing language acquisition as an instantaneous process. Generalizations about the dynamics of language acquisition might or might not shed light on the fundamental epistemological relationship between primary data and the final state of the language learner [see Chomsky 1975:15 and McCawley 1976b:240-241 for discussion], and Chomsky's opinion was that such dynamics were of little or no consequence in that task.

¹⁸ An excellent history of the use of the term can be found in Boudon 1986, on which we rely heavily in the discussion that follows.

refer to a theory of ideas, conceived of within a sensorialist, empiricist view of mind. It was Napoleon who shifted the term to the political realm, accusing the ideologues of ignoring political reality for abstract ideas. Marx, most famously in *The German Ideology*, used the term to refer to the false ideas that contradicted the reality found in everyday material life. (The notion of "profit" existing for itself was such a false notion for Marx, who claimed that the reality was rather surplus value created by workers.) Later Marxists, from Lenin to Althusser, have retained the notion that ideology is rooted in society in the core material relations, such as that of master to slave or employer to employee. In this sense ideology inheres in social institutions and the relations (and imbalances) of power that hold among its members – in the academic context, for example, in the professional relations between teachers and students, those hiring and those seeking employment, would-be contributors to journals and the editors receiving their submissions, those submitting abstracts and those on program committees, and so on.

Non-Marxist sociologists, such as Edward Shils, Talcott Parsons, Clifford Geertz, and Raymond Boudon, have debated whether the very definition of ideology presupposes the falsity of any proposition it asserts -- if the proposition were true, then in this sense it would not be ideology. Parsons proposed, for example, that ideology is by definition a deviation from the propositions that one would be led to by principles of scientific reasoning.

In a third and different sense, ideology has been taken not to be propositional at all but rather to consist in the shadowy psychological and sociological forces that operate on thought: it is what may *cause* a deviation from rational thinking. Ideology in this sense should be particularly difficult for anyone held in its clutches to perceive – we simply cannot imagine how a sane, rational person could see things differently. At the same time, we actively resist the notion that we could be influenced by base considerations which others, at a certain remove, appear to have no trouble discerning.

In *ILT* we did not attempt to distinguish between these three associated senses, simply taking ideology to refer to the self-interest that affects both the content of one's beliefs and the tenacity with which one holds to them. Moreover, we did not draw a sharp line between ideology and what might ordinarily be considered rational belief – indeed, it would be difficult to do so. For example, it can be an apparently rational move to espouse a particular

theoretical framework for reasons ranging from a conviction that its premises will ultimately receive convincing empirical support, to the impression that it is the framework in which the most interesting and important work is being done, to the belief that work in that framework will be accessible to a wider audience, to the belief that such work will be cited by a wider range of authors, to the belief that financial support, jobs, and other rewards are more likely to be forthcoming for those associated with that framework. But, as the philosopher of science David Hull has observed, scientists are motivated to undertake the hard work necessary to succeed in science by a variety of more ignoble inducements as well, among them the desire to promote one's own agenda and, when opposed, to "get that son of a bitch" (Hull 1988:160). Such impulses may be practically inseparable from those that are more commonly invoked for participation in science: love of truth, a desire to help humanity, and so on.

Whether or how an objective truth can emerge in such an environment remains the subject of philosophical speculation, but even taking the scope of ideology in science to be wide, positive scientific results that are durable and replicable across theories are routinely achieved – and, in the particular episode in which we were interested, *were* achieved by both Generative Semanticists and Interpretive Semanticists in their empirical work. Linguists on all sides of the Generative Semantics debate made significant contributions in uncovering abstract relationships underlying a broad range of surface phenomena and provided explanations that continue to play an important role in current research. By contrast, what we might call the "antagonistic" contests of theory – those in which linguists attempted to demonstrate the superiority of their own research program relative to another – did not in any of the cases we examined lead to resolution. We did not show that ideology was controlling or even important in these cases, but were suggesting it as a plausible explanatory hypothesis where the broadest claims of the participants did not follow from what had been solidly demonstrated.

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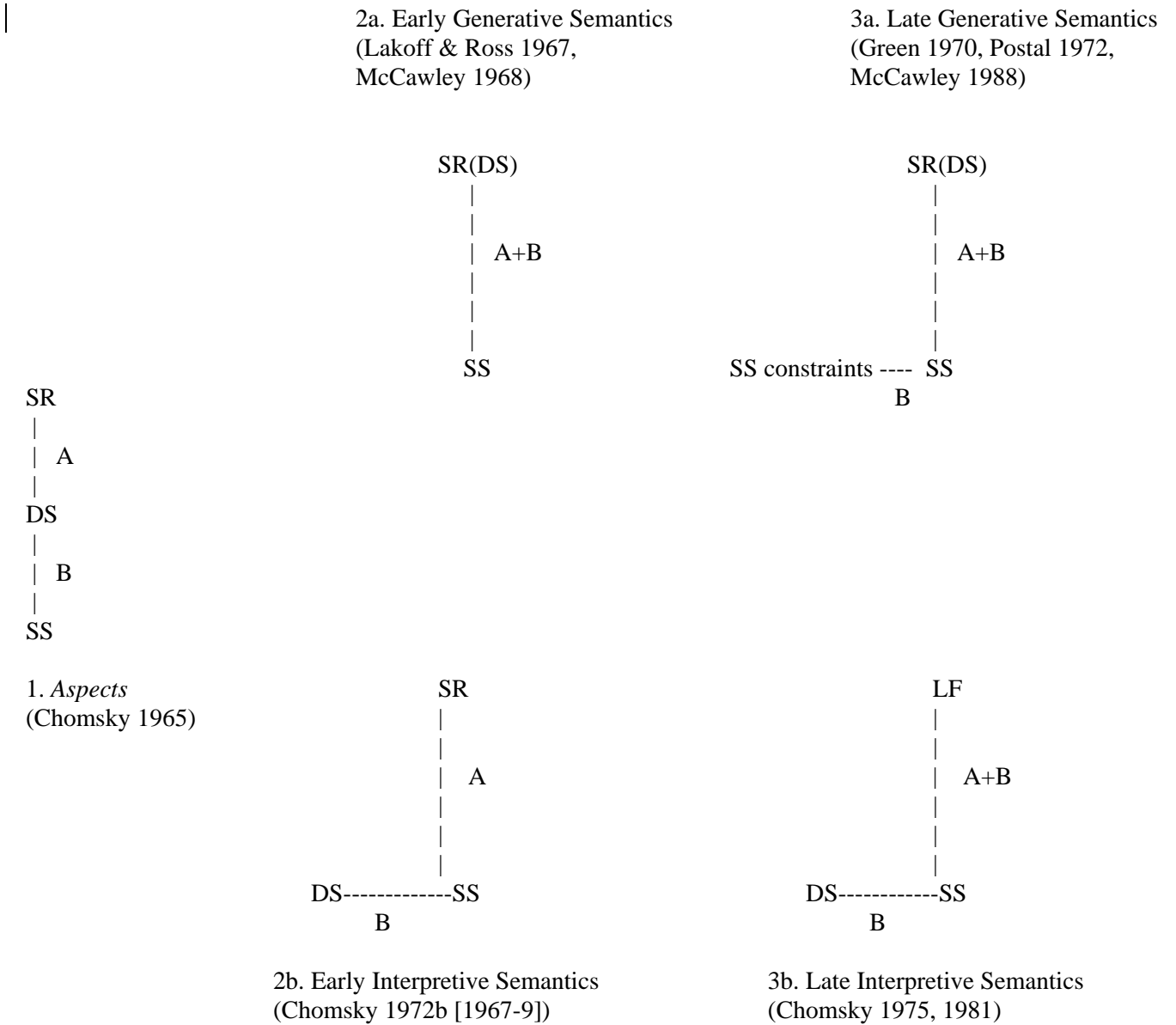
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A: Rules to account for the meaning of linguistic forms
 B: Rules to account for the distribution of linguistic forms
 SR: Semantic Representation
 LF: Logical Form
 DS: Deep Structure
 SS: Surface Structure

Figure 1. The development of Generative Semantics and Interpretive Semantics theories of grammar

We examine several issues raised in critiques of *Ideology and Linguistic Theory* and find no reason either to reject or to revise our conclusion that Interpretivist arguments purporting to establish the superiority of Interpretive Semantics over Generative Semantics were weak and were justifiably dismissed by proponents of the latter. We also briefly discuss our use of the term 'ideology' and its application in an analysis of linguistic thought.

Nous considérons certaines questions qui ont été posées dans les compte rendus de notre livre, *Ideology and Linguistic Theory*. Nous ne trouvons aucune raison ni de rejeter ni de réviser notre conclusion centrale, à savoir que les arguments interprétivistes qui visait à établir la supériorité de la Sémantique Interprétive par rapport à la Sémantique Générative étaient faibles, et que le rejet de ces arguments par les partisans de la Sémantique Générative était justifié. Nous discutons brièvement notre usage du mot "idéologie" et son rôle dans une analyse de la théorie linguistique.

In diesem Aufsatz untersuchen wir verschiedene Kritikpunkte, die in Besprechungen unseres Buches *Ideology and Linguistic Theory* erhoben wurden. Wir sehen keinen Grund, unsere ursprüngliche Schlußfolgerung aufzugeben oder zu revidieren, welche besagt, daß interpretivistische Argumente, die die Überlegenheit interpretativer Semantik gegenüber generativer Semantik zeigen sollten, schwach waren und zu Recht von Anhängern der letzteren zurückgewiesen wurden. Wir diskutieren außerdem kurz unseren Gebrauch des Begriffes "Ideologie" und seine Anwendung in einer Analyse linguistischen Denkens.