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DISCUSSING LANGUAGE

DIALOGUES WITH
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MOUTON

GEORGE LAKOFF

George Lakoff, what is the way to do linguistics today? What is language as the object of linguistics?
– I take linguistics to be the study of natural language in *all* of its manifestations. This is a broad conception of the field, and I think it is an appropriately broad one. It includes not just syntax-semantics, phonetics-phonology, historical linguistics, anthropological linguistics, etc., which form the core of most academic programs in this country, but also the role of language in social interaction, in literature, in ritual, and in propaganda, and as well the study of the relationship between language and thought, speech production and perception, linguistic disorders, etc.

One might object, of course, and propose that linguistics be considered a narrower discipline, on the grounds that one can only arrive at a coherent formal theory by limiting one's sights. I think it is true that many of the advances made in the period of transformational grammar, for example, could not have been made had Chomsky not limited the object of his theory to the study of something that does not exist in the real world, namely, the abstract set of rules of grammar (excluding meaning and use, and production and perception mechanisms) internalized in the mind of an ideal speaker-hearer in a homogeneous speech community. Perhaps the most interesting result to come out of transformational grammar is that such a limitation of the discipline is impossible – no coherent linguistic theory results from narrowing one's sights in this way. This is an interesting result, because it is not *a priori* true. In chemistry, for instance, there is a useful theory of ideal cases. It was thought that one might have been able to come up with a coherent theory of such a very limited domain in linguistics – 'ideal grammar'. The fact that the study of grammar ultimately had to take into account the study of meaning and use makes me wary of any artificial limitations of the domain of linguistics.

For this reason, I would not presume to tell anyone how to do linguistics; I don't think there is any one way. It is important to distinguish between what one considers a profitable *research strategy* at a given point in history and what is the *domain* of the field. I think that from the late 1950s to the mid 1960s transformational grammar provided a very profitable research strategy for certain people, though not others. An enormous amount of knowledge was gained: but by the late 1960s transformational grammar had pretty much outlived its usefulness as a research strategy. By 1967

it was no longer possible for those working seriously in transformational grammar to maintain that there was a syntactic deep structure that was distinct from logical structure, that fully determined meaning, contained all lexical items, and was the input to all transformational rules.

Which new research strategies were proposed at that time?
– Chomsky, in his extended standard theory, created a new and very different notion of deep structure, giving up any significant claims to its semantic relevance, but keeping it as a level prior to the application of any transformations and containing all lexical items. Such a deep structure, if it could be shown to exist, would be very close to surface structure. Generative semantics, on the other hand, gave up the idea of deep structure altogether, letting *logical structure* play the role of underlying syntactic structure. While Chomsky tried to maintain the idea that syntax was independent of meaning and use, generative semanticists suggested the opposite. Of course, the two uses of the term 'syntax' here are not at all comparable, since they are meant to cover entirely different ranges of phenomena. In generative semantics, 'syntax' is taken to be the study of what strings of words can express what meanings in what context. In this respect, generative semanticists are not doing generative grammar. Generative grammar assumes that strings of words can be determined to be syntactically well-formed or ill-formed in isolation, and sees as its goal providing a set of rules that can generate the well-formed strings. We reject the assumption that syntactic well-formedness in isolation is a viable concept. Generative semantics sees rules of syntax not as generating strings of words, but rather as generating relations between strings of words and what they mean relative to given contexts.

It should be borne in mind that in adopting generative semantics as a research strategy, we are not attempting to provide a *complete* theory of language. For example, we are not attempting to account for the facts of speech production or speech perception or the use of language in ritual or in literature. Instead we are trying to provide a theory of a subpart of linguistics, the relationship between sentences and what they mean in limited sorts of contexts. We are operating under the gratuitous assumption that such a theory can be constructed without taking into account the actual processes of speech production and perception, among other things. I have the creepy feeling that such an assumption will turn out to be wrong, just as the generative grammarians were wrong in assuming that a coherent theory of syntax was possible without taking meaning and use into account. But for the present I think it is wise, at least for me, to stick to generative semantics, since it has turned out over the past six years to be very fruitful as a mode of inquiry and has not yet outlived its usefulness. At the same time I am glad that competent investigators are studying various other aspects of language that generative semanticists are not looking at, since such studies will bring us closer to the day when an integrated theory of language in *all* of its manifestations will be possible.

I should also say that I do not think that *theory construction and verification* is the

only or even the most important mode of doing linguistics. Theorizing is more glamorous these days than doing careful descriptive work. I think that is unfortunate. Linguistic description is still an art, and is not likely to become a science for a long time to come. Unfortunately it is an art that has begun to die just at the time when it should be flourishing most. The reason is that it is still widely believed that linguistic descriptions of little-known languages should be formal and should follow some particular theory. But it has become clear in the past decade that no linguistic theory is anywhere near adequate to deal with most facts. What is wrong with formal descriptions is that they only allow for those facts that happen to be able to be dealt with by the given formalism. At this time in history, any description of a language that adheres strictly to some formal theory will not describe most of what is in the language. Moreover, as formal theories become outmoded, as is happening at an ever-increasing rate, descriptions of exotic languages made on the basis of those theories become increasingly less useful. I think the time has come for a return to the tradition of *informal* descriptions of exotic languages, written whenever possible in clear prose rather than in formal rules, so that such descriptions will still be useful and informative when present theories are long forgotten. In the past ten years the domain of known linguistic facts has spread far beyond the reaches of any foreseeable formal theories. In order to cope with these facts, we need all the help we can get. For this reason, it is important to recognize that there is no one particular 'way' to do linguistics. What there are are various research strategies, some more productive at present than others.

You have to redefine the concept of linguistic competence *and the opposition of competence and performance used by Chomsky, but also by structural linguists...*
– Chomsky has used the terms 'competence' and 'performance' in different ways at different times. The only consistent way in which I can understand his use of the term 'performance' is that he takes it to be a wastebasket for all the phenomena that cannot be accomodated by whatever theory he happens to be maintaining at a given time. Let me give some examples of the disparate ways in which he has used the term. Sometimes[1] he uses the term 'performance' to means what a person actually does, with 'competence' being whatever mental abilities enable him to do what he does. Call these concepts 'performance-1' and 'competence-1'.

At other times,[2] Chomsky uses 'performance' to include perceptual strategies and psychological processing abilities. Since a set of perceptual strategies is not part of what one does, but is rather part of one's mental abilities, this notion, call it 'performance-2', is part of competence-1. In performance-2, processing abilities and perceptual strategies are taken to be part of a human being's general mental abilities, rather than being part of some particular natural language, like Mohawk or Djirbal or Vietnamese. The abilities required to speak a particular natural language would come under the rubric of competence-2.

[1]   N. Chomsky, *Aspects of the Theory of Syntax* (Cambridge, Mass., 1965), p. 4.
[2]   Ibid., pp. 10-13.

However, Chomsky also speaks of particular languages as having "performance rules", for example,[3] he speaks of free word order as being determined by "rules of performance". This then is performance-3, in which certain not-clearly-specified language-particular rules – part of the grammar of some language like Latin or Navaho – are considered as part of 'performance' rather than 'competence'. Competence-3 would then cover certain language-particular rules, but not others. Where can one reasonably draw the line? I find the performance-3/competence-3 distinction particularly hard to comprehend. If Chomsky takes linguistics to be the study of linguistic competence, then taking competence to be competence-3, the study of word free order (which is never completely free, but has constraints varying from language to language) would *not* be part of the study of linguistics. Very strange indeed.

What is stranger is that Chomsky would, I believe, classify many of the most interesting phenomena currently being studied by linguists as performance. We can make a brief list: Ross' work on fuzzy grammar;[4] the work of Labov and others on variable rules; Robin Lakoff's work on the relation between grammar and the use of language in social and cultural contexts;[5] Fillmore's work on deixis;[6] Morgan's study of sentence fragments;[7] the studies of literal versus indirect meaning done by Gordon, myself, Sadock, Heringer, Green and Cole,[8] which is based on the study of speech acts by Searle and implicatures by Grice;[9] work on natural logic being done by myself,

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[3] Ibid., p. 127.
[4] J. Ross, "The Category Squish: Endstation Hauptwort", *Papers from the Eighth Regional Meeting Chicago Linguistic Society* 8 (further as *CLS*) (1972), 316-328; J. Ross, "A Fake NP Squish", in C. J. N. Bailey and R. Shuy (eds.), *New Ways of Analyzing Variation in English* (Georgetown, 1973); J. Ross, "Nouniness", in preparation; G. Lakoff, "Fuzzy Grammar and the Competence/Performance Terminology Game", *CLS* 9 (1973).
[5] R. Lakoff, "Some Reasons Why There Can't Be Any Some-any Rule", *Language* 45 (1969), 608-615; R. Lakoff, "Tense and its Relation to Participants", *Language* 46 (1970), 838-844; R. Lakoff, "If's, And's, and But's about Conjunction", in Ch. Fillmore and D. T. Langendoen (eds.), *Studies in Linguistic Semantics* (New York, 1971), 115-150; R. Lakoff, "Passive Resistance", *CLS* 7 (1971), 149-162; R. Lakoff, "The Pragmatics of Modality", *CLS* 8 (1972), 229-246; R. Lakoff, "Language in Context", *Language* 48 (1972), 907-927; R. Lakoff, "Language and Woman's Place", *Language and Society* (1973); R. Lakoff, "The Logic of Politeness: or, Minding your P's and Q's", *CLS* 9 (1973); R. Lakoff, "Questionable answers and answerable questions", in B. Kachru et al. (eds.), *Papers in Linguistics in Honor of Henry and Renée Kahane* (University of Illinois Press, 1973).
[6] Ch. Fillmore, "Santa Cruz Lectures on Deixis" (1971, unpublished).
[7] J. Morgan, "Sentence Fragments", in B. Kachru et al. (eds.), *Papers in Linguistics in Honor of Henry and Renée Kahane* (University of Illinois Press, 1973).
[8] D. Gordon and G. Lakoff, "Conversational Postulates", *CLS* 7 (1971), 63-84; J. Sadock, "Queclaratives", *CLS*, 7 (1971), 223-231; J. Sadock, "Speech Acts Idioms", *CLS* 8 (1972), 229-239; J. Heringer, "Some Grammatical Correlates of Felicity Conditions and Presuppositions", *Working Papers in Linguistics* 11 (The Ohio State University Department of Linguistics, 1972); G. Green, "How to Get People to Do Things with Words", *Georgetown Roundtable* (Georgetown, 1972); G. Green, *Semantics and Syntactic Regularity* (Cambridge, 1973); P. Cole, "Conversational Implicature and Syntactic Rules", in C. Bailey and R. Shuy (eds.), *New Ways of Analyzing Variation in English* (Georgetown, 1973).
[9] J. Searle, *Speech Acts* (Cambridge, 1969); H. P. Grice, "Logic and Conversation" (1967, unpublished).

Karttunen, McCawley, Horn, Dahl, and Keenan, including the study of hedges and fuzzy concepts being done by Zadeh and myself.[10]

It is often thought that the principal difference between Chomsky and the generative semanticists resides in their differing conceptions of the relation between syntax and semantics. There are certainly great differences there, but the biggest difference has to do with the question of what is the *scope* of linguistics. We consider the work mentioned above as central to the study of linguistics, that is, as investigating important phenomena that any linguistic theory with claims to any adequacy at all must deal with. So far as I can tell, Chomsky seems to believe that these are all outside the scope of linguistic theory.

In short, generative semantics is trying to come to grips with a much wider domain of facts than either Chomsky's standard or extended standard theories are set up to deal with. It is partly for this reason that Chomsky's claim that the two theories are notational variants is utterly crazy. How can two theories be notational variants if they are about two very *different domains of facts*? In one sense the question of what one calls 'competence' and what one calls 'performance' is a piddling issue of *terminology* – the facts are the same, call them what you will. In another sense, though, it is a matter of critical importance, if, like Chomsky, one uses the terms 'performance' and 'competence' to characterize what kinds of facts you feel a linguistic theory should be responsible for. In the latter case, such a decision can have an effect on whether a given fact is a crucial counterexample, or 'merely a matter of performance' which can be brushed under the rug. Chomsky's shifting definitions of performance provide him with a rug big enough to cover the Himalayas.

How to define then the relation between acceptability *and* grammaticality *?*
– I don't think that such a distinction makes sense. A number of concepts that were basic to transformational grammar, such as grammaticality, were more artifacts – not real natural language concepts, but artificial concepts that Chomsky needed to make it look like that theory had a chance of working. As I have suggested elsewhere,[11] I don't think that one can in general say that sentences in isolation are grammatical or not. Instead one has to ask whether a given *sentence* can be paired with a given *logical structure* in a given *context*. If one views grammars as generating relations

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[10] G. Lakoff, "Linguistics and Natural Logic", in D. Davidson and G. Harman (eds.), *Semantics of Natural Language* (Dordrecht, 1972), 545-665; L. Karttunen, "Some Observations on Factivity", *Papers in Linguistics* 4 (1971), 1; L. Karttunen, "Presuppositions of Complex Sentences", *Linguistic Inquiry* 4 (1973); L. Karttunen, "Remarks on Presuppositions", in A. Rogers et al. (eds.), *Performatives, Presuppositions and Implicatures* (Center for Applied Linguistics, Washington D.C., to appear); J. McCawley, *Selected Papers on Grammar and Meaning* (Seminar Press, 1973); L. Horn, "On the Semantic Properties of Logical Operators in English" (UCLA Diss., 1972); E. Keenan, "Two Kinds of Presupposition", in Ch. Fillmore and D. T. Langendoen (eds.), *Studies in Linguistic Semantics* (New York, 1971), 45-54; G. Lakoff, "Hedges", *CLS* 8 (1972), 183-228.
[11] G. Lakoff, "Presuppositions and Relative Grammaticality", in D. D. Steinberg and L. A. Jakobovits (eds.), *Semantics. An Interdisciplinary Reader in Philosophy, Linguistics and Psychology* (Cambridge, 1971), 329-340.

between sentences, contexts and logical structures, the notion 'grammaticality' has no meaning. In its place there is the concept of the degree of well-formedness of triples of the form [sentence, logical structure, context].

Don't you need for doing linguistics some sharp methodological *notions and oppositions?*
– I take it you are referring to the question of what constitutes empirical evidence for a given analysis, that is, what kind of data we deal with. Back in the days of transformational grammar, linguistic data was taken to consist of intuitive judgements of grammaticality. Everybody has given up on that idea, including Chomsky and his students, who, like those of us in generative semantics, found no way of distinguishing 'ungrammatical' from 'semantically anomalous' sentences. They maintained the distinction, though it seems to have lost any empirical status. Chomsky's dictum is: let theoretical considerations decide what is ungrammatical and what is semantically anomalous. Within the extended standard theory there is no way of deciding on empirical grounds whether a given phenomenon is to be handled by syntactic rules or interpretive semantic rules. Within generative semantics, we take as our primitive data intuitive judgements as to the degree to which a given *sentence* can have a given *meaning* in a given *context*.

Actually, that's too brief to be entirely clear. Coded into the expression "have a given meaning in a given context" are such matters as: (1) When you use a given sentence to mean a given thing, are you being sincere or not, polite or not, formal or not, joking or not, etc.? (2) Given a sentence and a fixed context what can the sentence mean in the context? If it can have more than one meaning, is one 'stronger' than another? Or more likely? Or more 'normal'? (3) Are certain sentences with certain readings limited to given types of discourses, e.g., answers to questions, astounded responses, stories, meek inquiries, etc.? (4) What assumptions is a speaker making when he uses a given sentence to convey a given meaning in a situation? (5) What is the literal meaning of the sentence in a given situation, and what is 'conversationally implied' by the sentence? This will give you some idea of the kind of data we are after.

What do you mean by context: the behavioral, psychological, situational, or verbal context?
– Any aspect of context that interacts with rules of grammar. *Previous discourse* is obviously important. Answers to questions, for instance, require a knowledge not merely of the meaning of the question asked, but also various aspects of superficial form. Take an example:

(1) Did you give a present to someone?
    a. Yes, Zelda.
    b. Yes, to Zelda.
(2) Did you give someone a present?
    a. Yes, Zelda.
    b. *Yes, to Zelda.

The questions in (1) and (2) have the same meaning, but permit different answers because of differences in their superficial forms.

Conceptual contexts, that is, the assumptions made by speaker and hearer, also interact with rules of grammar. For example, as Robin Lakoff shows,[12] sentences with past and future tenses differ from corresponding sentences with the periphrastic equivalents of past and future. For example, in certain deictic constructions, past and future tenses but not their periphrastic equivalent, indicate present time but carry an assumption on the part of the speaker that the object in question was or will be within the perceptual field of the participants in the conversation. Consider the following examples:

(3) That *was* a chipmunk; see, there he is again climbing that tree.
(4) That'll *be* a chipmunk; wait till it comes out of the bushes.

In both cases, the speaker is committed to the *present* truth of proposition, namely, that the object referred to *is* a chipmunk, and the tenses used reflect past or expected future appearance in the perceptual field of the participants. The so-called periphrastic tenses, *used to* and *be going to*, do not work this way:

(3') That *used to be* a chipmunk; see, there it is again climbing that tree.
(4') That is *going to be* a chipmunk; wait till it comes out of the bushes.

(3') and (4') are not paraphrases of (3) and (4), if they can be used appropriately at all. She further showed that sequence-of-tenses rules are subject to the same pragmatic constraints.

In another article of R. Lakoff[13] it was demonstrated that indications of the speaker's attitude toward a state or event were tied to the occurrence of *get*-passives. For instance, a newspaper trying to maintain a stance of objectivity would use (5a) not (5b) in a newspaper story:

(5a) Fred Snurdley was arrested yesterday on a marijuana charge.
(5b) Fred Snurdley got arrested yesterday on a marijuana charge.

(5b) would indicate that it was a bad thing to happen and hence express sympathy for Snurdley. For the same reason, taped TV programs use (6a), not (6b):

(6a) This program has been pre-recorded.
(6b) This program has gotten pre-recorded.

(6b) would suggest that it was a bad thing for the program to have been taped.
Bolinger[14] had noted that assumptions made by the speaker some times motivated the choice between *some* and *any*. Klima[15] had assumed that the distribution of *some*

[12] R. Lakoff, "Tense and its Relation to Participants".
[13] R. Lakoff, "Passive Resistance".
[14] D. Bolinger, "Linguistic Science and Linguistic Engineering", *Word* 16 (1960), 374-391.
[15] E. Klima, "Negation in English", in J. A. Fodor and J. J. Katz (eds.), *The Structure of Language* (Englewood Cliffs, 1964), 246-323.

and *any* was a syntactic phenomenon. He attempted to account for the phenomenon within syntax by postulating a syntactic feature [+ affective] to account for the distribution of *some* and *any*. Such examples were ignored by transformational grammarians until Robin Lakoff, in accord with her general program, suggested that the Bolinger-Klima approaches to the question be unified. She found further evidence that extra-linguistic assumptions affected the distribution of *some* and *any*, and proposed that such assumptions needed to be represented somehow in the statement of syntactic rules. Consider, for example, how sentences (7) and (8) below differ from (9a) and (9b):

(7) If you eat any of the candy, I'll smack you.
(8) If you eat some of the spinach, I'll give you a dollar.
(9a) If you eat some of the candy, I'll smack you.
(9b) If you eat any of the spinach, I'll give you a dollar.

[*any* here is unstressed.] Note first of all, a subtle difference in meaning, or intention, in the first pair. Although they are syntactically parallel, (7) functions as a threat, (8) as a promise. That is, in (7) the speaker is warning the addressee that, if he does not comply with instructions, something will happen to him that he won't like; in (8), on the other hand, the speaker is suggesting that, if the addressee complies with instructions, something will happen to him that he will like. The first is negative in tone, the second positive. But there is nothing overtly present in either of the sentences that distinguishes them in this way, merely the implicit assumptions and their consequences:

(a) Getting a smack is not good. Hence, it *is not* good to eat *any* of the candy.
(b) Getting a dollar is good. Hence, it *is* good to eat *some* of the spinach.

In Klima's examples, the clearer cases involved sentences where superficial negative (or non-positive) environments (e.g. negatives and questions) conditioned the occurrence of *any*. In such cases, it was possible to assign *some* and *any* to sentences by purely syntactic rules. But (3) – (6) show that, if we are to state a general principle to account for all uses of *some* and *any*, it must look beyond superficially present syntactic phenomena: we must admit to the discussion the implicit assumptions of the participants and the consequences of these assumptions. If the consequence is positive, we will find *some*; if negative, *any*.

The conditions under which (9a) and (9b) can be appropriately used follow this principle. For instance, (9a) and (9b) are perfectly appropriate in contexts where (9a) constitutes a promise and (9b) constitutes a threat. Such an interpretation requires assumptions that are somewhat odd given the world as we know it, namely, that the addressee enjoys being smacked and hates monetary rewards. But given such assumptions, (9a) and (9b) would be appropriate. (9b) could also be a promise rather than a threat in case the speaker had negative expectations of the conditions being fulfilled. In

such a case a negative assumption could trigger the occurrence of *any*.[16] Further studies following the program initiated by Bolinger and Robin Lakoff which have provided further analyses of *pragmatic* factors tied to grammatical processes were published in the meantime.[17] The importance of this work is that it shows that linguistic rules cannot simply be taken as having the function of distinguishing grammatical from ungrammatical sentences; grammar must also specify the *conditions* under which sentences can be *appropriately* used.

A great many types of participants' assumptions in a conversation interact with rules of grammar. Some of the more startling ones are things that used to be considered purely part of speaking behavior, the placing of interjections, like *uh*, *oh*, *ah*, etc., in a sentence. James,[18] following up on work by Robin Lakoff,[19] has shown that even interjections such as these cannot be excluded from the study of grammar. He shows that they cannot just be randomly inserted into sentences. Rather their use is rule-governed and *specified* by syntactic, semantic, and pragmatic conditions. She has shown that in order to determine (a) whether a given 'extralinguistic' particle is usable in a particular environment and (b) how it affects the meaning of the sentence, one must take into account such syntactic phenomena as Ross' constraints on movement transformations. She also shows that such particles interact differently with different types of idioms and negative polarity items, and that the nature of the interaction varies with the particle. The following are but a handful of James' examples:

(10) John threw the ball, oh, up.
(11) *John threw his dinner, oh, up.
(12) I saw, uh, 12 people at the party.
(13) I saw, oh, 12 people at the party.
(14) *For some stupid reason, ah! not many people came to the party.
(15) I believe that Bill ate, oh, five cookies.
(16) *I believe the claim that Bill ate, oh, five cookies.
(17) *I regret that Bill ate, oh, five cookies.

The difference between (10) and (11) is due to the fact that *throw up* is in the first case composed of two semantically distinct units, a verb + directional particle, while in the second case it constitutes an indivisible idiom (it is equivalent to *vomit*). In (10), one might have several choices of direction in which to throw the ball, before settling on 'up': the use of *oh* indicates this casting about among possibilities. But once one

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16 Further examples along this line are given by A. Borkin, "Polarity Items in Questions", *CLS* 7 (1971), 53-62, and by L. Horn, "On the Semantic Properties of Logical Operators in English" (UCLA Diss., 1972).
17 Cf. the publications of R. Lakoff, mentioned in note 5; further G. Lakoff, "Presuppositions and Relative Grammaticality", A. Rogers, "Three Kinds of Physical Perception Verbs", *CLS* 7 (1971), 206-222; G. Green, cf. note 8; J. Lawler, "Generic to a Fault", *CLS* 8 (1972), 247-258; P. Postal, *On Raising* (Cambridge, Mass., forthcoming).
18 D. James, "Some Aspects of the Syntax and Semantics of Interjections", *CLS* 8 (1972), 162-172; D. James, "The Syntax and Semantics of Interjections" (U. of Michigan Diss., forthcoming).
19 R. Lakoff, "The Logic of Politeness: or, Minding your P's and Q's".

was aware of the general type of action described in (11), there is only one way to end the sentence: there are no other choices within the idiomatic framework. So *oh* is not possible here. The next pair illustrates a semantic difference between *oh* and *uh*: they are not mere meaningless particles, as many grammarians have stated, used merely to purchase time in speaking. (12) would be used if the speaker wanted to state an exact number (and knew this number), but it had slipped his mind for an instant. *Oh* (in (13)) would be used to mean, 'approximately', if the speaker were not certain of the exact number, or didn't care enough to give it. Thus, if I enter the room where the party is and find fourteen people present, I can accuse the speaker of (12) of misleading me, or being inaccurate at least, but not the speaker of (13). So these sentences are semantically distinct. The next sentence is of interest in that it shows that the occurrence of these particles is subject to syntactic or perhaps semantic constraints: they are not interjected randomly into sentences at will. When an adverbial phrase presupposing the truth of the main clause of a sentence is preposed, it cannot be followed by a particle, then the main clause. Finally the last three examples illustrate the interaction of strictly syntactic movement constraints and the presence of interjections. Interjections may occur inside a sentence embedded after *believe*, as in (15), but not in environments apparently similar, like (16) and (17). The reason, according to James, is that the embedded sentences in (16) and (17) form islands, in Ross' sense, but this is not the case in (15). Interjections reflecting the speaker's feelings obey typical island constraints. This shows that these interjections are governed by the same sorts of strict syntactic constraints that control movement rules, sequence-of-tenses changes, and other unquestionable syntactic rules. This rules out any possible claim to the effect that these parts of speech are in any sense 'performance phenomena', if by this we mean they are either (i) random, meaningless, and non-rule-governed, or (ii) part of general cognitive processing mechanisms and therefore outside the domain of linguistic rules, both language-particular and universal. Of course, one could always try to redefine performance to keep interjections and hesitation phenomena within its domain. What James' work shows is that any attempt to do so will make many principles of grammar, both universal and language-particular, part of such a redefined 'performance', which would be a considerable change in the original sense of the term.

Social context also interacts with grammar. For example, there are certain constructions in English which express polite requests and which express rude requests. Compare (18) and (19):

(18) Can you take out the garbage? (polite)
(19) You can take out the garbage. (rude)

There are also idiomatic expressions that are rude and that yield grammatically ill-formed sentences when put in polite constructions:

(20) You can take your methodology and shove it.
(21) *Can you take your methodology and shove it?

You wrote that linguistic study is rooted in the study of human thought and culture. The domain of linguistics, so defined, is very large....
– That's right. I don't think one can describe, much less explain, linguistic rules unless one studies the *uses* to which language can be put. Transformational grammar tried and failed. One thing that one might ask is whether there is anything that does *not* enter into rules of grammar. For example, there are certain concepts from the study of social interaction that are part of grammar, e.g., relative social status, politeness, formality, etc. Even such an abstract notion as *free goods* enters into rules of grammar. Free goods are things (including information) that everyone in a group has a right to. What counts as free goods will, of course, vary from subculture to subculture. For example, in the counterculture in Berkeley, food is free goods; at a counterculture restaurant someone at the next table may ask you for a bite of your sandwich and refusal identifies you as being either out of the subculture, selfish or impolite. In other American subcultures the prices of household articles are free goods to friends; in others they are not. Someone from a subculture of the former sort might, upon walking into your house for the first time, say "Hey, that's a nice rug. What did it cost?" In that subculture, such a question would count as a compliment. In subcultures where prices are not free goods, such a question would be completely out of place. If someone wanted to know what your rug cost (say, because he wanted to buy one himself), he would have to say something like "May I ask you what that rug cost?" The form "May I ask you...?" is used when the item asked for is not free goods, but when the speaker has no reason to believe that he is not welcome to it anyway. Note, incidentally, that this is not simply a matter of asking permission – one may not substitute "be allowed" for "may" in such questions. "Am I allowed to ask you what that rug cost?" or "Please give me permission to ask you what that rug costs?" would be totally inappropriate. Any adequate grammar of English would have to point out that *may*-questions of the above sort involve the notion of *free goods*.

Now let's get back to the question of whether there are any concepts that do *not* enter into rules of grammar. The notion of *political equality* as opposed to social equality seems not to play a role. There seems to be no rule of grammar that I have heard of in any language that applies just, say, when both (or neither) of the speakers have the right to vote in national elections, or when one does and the other doesn't (unless, of course, there is a corresponding *social* equality or inequality). The sort of concepts discussed in political science as opposed to sociology seem not to play a role in rules of grammar.

Has linguistics then to become sociolinguistics?
– It has been for a long time, except perhaps for the decade from 1957 to 1967, during which transformational grammar was dominant. Traditional grammars have always paid a great deal of attention to things like politeness, formality, status, etc., especially in languages like French and Russian, where the use of second-person

pronouns requires a fair amount of knowledge about social relations, and in languages like Japanese, where there are honorific particles. Anthropological linguists within this century have been especially careful to take note of such things. The idea may seem new to my generation, but only because we were brought up on transformational grammar.

Is there no boundary between semantics and pragmatics, in your opinion?
– I think Richard Montague was on the right track when he suggested that the apparatus of model-theoretical semantics could be adapted to handle various so-called pragmatic phenomena. Given the fundamental notion of model-theoretical semantics, namely, satisfaction in a model, and the derived notion of logical consequence, one can deal with a great many phenomena that had previously been called 'pragmatic'. In my paper "Pragmatics in Natural Logic",[20] I suggest a way in which indexicals, speech acts, and conversational implicatures can be handled using just model-theoretical semantics and transderivational syntax.

What do you mean to be the scope of what you called 'natural logic'?
– Natural logic is the study of *reasoning* in natural language. As such it differs from classical logic in many respects. First, its scope is much broader. Classical logic concerned itself with concepts like *and, or, if-then, not, every,* and *some.* More recently, logicians have attempted to deal with a handful of other concepts, such as *logical necessity, obligation, belief, knowledge, tenses,* certain *adverbs, many, few,* etc. – mostly in isolation as minor extensions of classical logic. A complete natural logic would have to deal with all of these concepts together, plus hundreds and perhaps thousands more – depending on how many primitive concepts natural language can be reduced to. In short, it is the full study of the conceptual resources of natural language.

What is the importance of ordinary language philosophy in this respect?
– Even a cursory scanning of the linguistic literature of the past three or four years will show that ordinary language philosophy has had an enormous influence on linguistics. Strawson's pioneering work on presuppositions[21] has influenced a great many people and led to a considerable number of important studies. The work on speech acts by Austin and Searle and Grice's theory of conversational implicature[22] have also greatly expanded the range of linguistic studies. Incidentally, just about none of the linguistic studies that have come out of ordinary language philosophy have accepted the premises or claims of the ordinary language philosophers. What have

[20] G. Lakoff, "Pragmatics in Natural Logic", in A. Rogers et al. (eds.), *Performatives, Presupposi tions, and Implicatures* (Center for Applied Linguistics, Washington D.C., to appear).
[21] P. Strawson, *Introduction to Logical Theory* (London, 1951).
[22] Cf. note 9.

been taken over are the empirical observations, for example, Austin's observations about performatives.

Model-theoretical semantics in the tradition of Tarski, Carnap, Kripke, and Montague[23] has also had a profound effect on linguistics in recent years. Chomsky and Katz, having been brought up more in a proof-theoretical than model-theoretical tradition, attempted to characterize meaning in purely *combinatorial* terms. That is, they assumed that whatever there was to be said about meaning could be said in terms of combinations of a finite stock of elements. This made it impossible for them even to begin to come to grips with problems of reference and coreference. Kripke's possible world semantics enabled generative semantics to begin dealing effectively with these problems. Moreover, purely combinatorial semantics cannot deal with fuzzy concepts at all. There is no way for combinatorial semantics to provide meanings for hedges like *sort of, rather, pretty,* etc., which map fuzzy concepts into new fuzzy concepts. Model-theoretical semantics, on the other hand, provides a natural way for dealing with both reference and fuzziness.

Is the sentence, *in generative semantics, the primitive unit of language, as in transformational grammar?*
– No. In the theory of generative semantics as I have formalized it so far, the abstract objects generated are not sentences but quadruples of the form (S, LS, C, CM) where S is a sentence, LS is a logical structure associated with S by a derivation, C is a finite set of logical structures (characterizing the conceptual context of the utterance), and CM is a sequence of logical structures, representing the conveyed meanings of the sentence in the infinite class of possible situations in which the logical structures of C are true.

But even this is inadequate. One must take into account much more than conceptual contexts (that is, assumptions of speaker and hearer). Rules of grammar also require that one take into account the *stylistic* type of *discourse* one is in. For example, there is a grammatical construction in English which can only be used in stories and not in conversation; and within a story, it can only be used to describe a setting. Consider sentence like:

(22) Noon found Harry standing in front of the Blue Parrot Saloon.

Compare (22) with:

(23) At noon, Harry was standing in front of the Blue Parrot Saloon.

They both have the same cognitive content, but (22) can only be used in a story while describing a setting. It is completely inappropriate in anything like an ordinary conversation. Somehow this fact must be represented in a grammar of English,

[23] Cf. among others, S. Kripke, "Semantic Considerations on Modal Logic", *Acta Philosophica Fennica* 16 (1963), 67-96; R. Montague, "Pragmatics and Intensional Logic", in D. Davidson and G. Harman (eds.), *Semantics of Natural Language* (Dordrecht, 1971), 142-168.

which means that one cannot view a grammar as merely generating sentences without any indication of the types of stylized discourses which they are restricted to.

Is the logical structure of a sentence the same as its underlying grammatical structure?
— That is the basic assumption behind generative semantics. Of course, it requires more elaboration, since a sentence may have one logical structure in one context and another in another context. That is why we speak of generating quadruples of the form (S, LS, C, CM). What we are claiming is that sentences are not just paired with logical structures in isolation; what logical structures they are paired with, and the *degree* to which they are paired with them, depends upon *context* and upon constraints on possible conveyed meanings.

Logical structures ought, incidentally, to be distinguished from semantic representations of the Katzian variety. One thing that I think my work on hedges and on conversational postulates shows is that there does not exist any such thing as a semantic representation, that is, a single combinatorial structure representing all elements of the meaning of the sentence. *Literal* meaning must be distinguished from *conveyed* meaning. And certain model-theoretical aspects of meaning cannot be represented combinatorily.

What do you mean by the 'presupposition' of a sentence?
— The term 'presupposition' has been used (confusingly) to cover two very different concepts. A *logical* presupposition is a relation holding between two logical structures. We can further define an extended sense of logical presupposition as a relation holding between two surface sentences just in case the logical structure of one logically presupposes the logical structure of the other. A *pragmatic* presupposition is a relation holding between an individual and a proposition. This is the sort of presupposition that linguists usually talk about; perhaps there would be less confusion if we used the term 'assumption' or 'presumption' instead. The two notions are, of course, related, since speakers usually presume the truth of the logical presuppositions of sentences when they utter them sincerely.[24]

I assume that the study of logical presuppositions is part of the study of natural logic. I take pragmatic presuppositions as being handled by transderivational syntactic rules, which will take into account logical, syntactic, lexical, and phonological facts of the language in question. It should be noted that it is not at all obvious how, if at all, one is to draw the line between logical and pragmatic presuppositions. The problem of trying to sort out one kind from another is a little like the problem of telling when one should deal with lexical meaning by meaning postulates or by lexical decomposition. There are some clear cases, but mostly there is no evidence one way

[24] For a discussion, see L. Karttunen, "Some Observations on Factivity", "Presuppositions of Complex Sentences"; "Remarks on Presuppositions". Further see R. Stalnaker, "Pragmatics", in D. Davidson and G. Harman (eds.), *Semantics of Natural Language* (Dordrecht, 1971), 380-397.

or the other. This situation may suggest that we are looking at things wrong. If generative semantics has an Achilles heel, it is here.

How are the notions of reference *and* coreferentiality *to be formalized in linguistics?*
— The same way they are formalized in formal semantics, namely, using the notion of a *denotation* at a point of reference. Recall that logical structures are taken to be model-theoretically interpreted. That means that there will be a denotation function assigning to each variable in logical structure at each point of reference a member of a universe of discourse. Two instances of the same variable will, of course, always be assigned the same referent at a given point of reference. Hence, two instances of the same variable will be coreferential. *Reference* and *coreference* for nominals in surface and intermediate structures will be defined using the notion of 'corresponding node' as defined in global grammar. To find out what a surface nominal refers to at a given point of reference, look at its corresponding node in logical structure and see what it refers to at that point of reference.

Why was the introduction of transderivational rules *in your theory necessary for grasping the contextual meaning?*
— Before I answer your question directly, let me point out a couple of things. First, transderivational rules are not only necessary for dealing with contextual constraints in grammar; they are needed on totally different grounds, for example, cases where a derivation may be blocked because it could lead to *ambiguity*.[25] Note that ambiguity is not a property of a single derivation; we get ambiguity when there is more than one derivation for a given sentence. To block one derivation because of potential ambiguity we must know what the other derivation with the same surface string is. Rules that take into account the presence or absence of a *morphological contrast* are also transderivational in nature, since morphological contrasts do not usually occur in a single derivation. Moreover, rules of analogy are also fundamentally transderivational rules that have nothing whatever to do with context; they are needed on independent grounds.

Next, it is impossible to build into a single derivation a specification of all of the contexts in which a sentence can be appropriately used. The reason is that there can be an *infinite* number of such contexts, and although they cannot be listed in a single logical structure (or deep structure or semantic representation or whatever), they can be finitely characterized by transderivational rules. For example, suppose we have a sentence, S, with logical structure P. Given a derivation relating S and P, we can ask with respect to what contexts it is well-formed. Suppose that it is well-formed with respect to every context X such that $X \cup \{P\} \Vdash Q$, for some Q. That is, the set of logical structures X taken together with P entails Q. To state the general principle in finite terms we need only be able to specify finitely the form of Q, make reference

[25] G. Lakoff, "Some Thoughts on Transderivational Rules", in B. Kachru et al. (eds.), *Papers in Linguistics in Honor of Henry and Renée Kahane*.

to the entailment relation, and use a quantifier over contexts X. Thus we can characterize by finite means an infinite class of contexts with respect to which a derivation linking P and S is well-formed.[26]

It should be clear that the rules that are needed to handle such cases must be *transderivational*. Assuming that S can have a literal reading (that is, CM = P), the grammar will generate an infinite class of quadruples of the form (S, P, X, P), where $X \cup \{P\} \Vdash Q$. A transderivational rule can generate an infinite class of quadruples of this form. But there is no way of listing in a single finite logical structure or semantic representation the infinite class of contexts that X can vary over.

Is one of the purposes of generative semantics to discover meaning universals?
– The term 'meaning universals' has many senses, but the answer is yes in all cases. First, we assume that natural logic is universal, that is, it is intended to characterize the meanings of all the primitive concepts that occur in human language. In short, it is intended to characterize rational thought itself. Second, since we take rules of grammar as associating surface sentences with their logical structures, contexts for appropriate use, and conveyed meanings, universals of grammar are for us largely universals concerning the rules by which sentences are associated with their meanings.

As should be clear, the terms 'universal of grammar' and 'meaning universal' encompass much more in generative semantics than they do in transformational grammar, since the subject matter of generative semantics is so much larger. Moreover, we assume that there is a relatively small and limited stock of rules of grammar that are possible in natural languages; that is, we assume that there are a great many *substantive* universals, rather than just universals of form, as Chomsky assumes for transformational grammar. In general, I find transformational grammarians relatively conservative on the issue of universals.

Do you consider generative semantics only a new step in the development of transformational grammar, or is generative semantics a new theory dependent on a great number of inspiring theories?
– I guess that if you considered generative grammar a new theory as opposed to just an extension of Harris' transformational grammar, then you would have to consider generative semantics a new theory. The subject matter is much broader than the subject matter of generative grammar, the questions it seeks to answer are very different, the types of mechanisms it uses are different – so it would seem to be a new theory. Of course, historically it started out as an extension of generative grammar. This is clear in the first paper I wrote on the subject.[27] But it has developed quite a bit since then. Just as Harris looked upon Chomsky as simply extending his theory (which is what

[26] Examples of cases of this sort are given in G. Lakoff, "The Role of Deduction in Grammar", in Ch. Fillmore and T. Langendoen (eds.), *Studies in Linguistic Semantics*.
[27] G. Lakoff, "Towards Generative Semantics" (M.I.T. Mechanical Translation Group, 1963, unpublished).

Chomsky started out doing), so Chomsky probably looks at us in the same light. But in both cases the differences are substantial. I think that what carried generative semantics furthest away from generative grammar was the work of Robin Lakoff and Charles Fillmore on the role of context in grammar. I would say that their contributions in this direction, more than any others, split off generative semantics as a separate field. Since then, Ross' contributions to the study of fuzzy grammar and my work on fuzzy logic have made the division all that much wider. Our views of the role of *context* in grammar and the role of *fuzziness* in grammar and meaning distinguish us very sharply from generative grammarians.

Can you say that the most important difference between Chomsky's theory of grammar and yours concerns the relation between syntax *and* semantics?
– No. That is one important difference, and historically it was at one time, around 1968, the most important difference, but now there are a number of differences of equal importance. First, the role of *context*, especially social context, implicatures, etc., in grammar is at least as important as the role of literal meaning. Second, the role of *model theory* has become very important. Thirdly, *fuzziness*, both in grammar and logic, is a matter of prime importance for distinguishing the theories. What attracted me initially to transformational grammar was that it seemed to give some insight into meaning. What is attracting our current students to generative semantics is not only that but that it seems to give one some insight into social interaction and the *use* of language.

Do you agree with the terms 'autonomous syntax' versus 'semantic syntax' for characterizing Chomsky's theory as opposed to your theory?
– I think it would be appropriate to call Chomsky's theory of syntax 'autonomous'. But I don't think that 'semantic syntax' quite gets at the heart of what we're doing – nor does the term 'generative semantics'; but we started using it in 1963 and now we're stuck with it. Anyway, we are not just involved in the study of grammar and meaning, but in the study of the much broader relationship between language, thought, and culture.

What's the kind of empirical *evidence for making a choice between the two alternative theories: transformational grammar and generative semantics?*
– That's a difficult question. In order to compare two theories at all on empirical grounds, one has to find enough common assumptions to make a comparison plausible. This is particularly difficult since the two theories make such different assumptions and cover such different subject matter. Basically, there are two approaches you can take, and both are exemplified in the literature of the field in the past five years. The first approach uses the fact that the subject matters of the two fields are very different, though they overlap in certain areas of classical syntax and the study of certain aspects of literal meaning. The idea is to show that you can state

some general linguistic principle if you take the wider subject matter into account, but that you cannot state any such general principle if you fail to. It is like arguing against someone who says that the subject matter of linguistics is the study of the first halves of sentences. You argue against him (if you really feel you have to) by showing that general principles emerge by studying whole sentences but not by limiting oneself to halves of sentences. I used this type of argumentation with respect to contrastive stress, relative pronouns, and anaphoric expressions[28] and others, with respect to polarity items.[29] Robin Lakoff's "Language and Woman's Place" ends with a particularly effective argument of this sort.[30]

The second approach is to try to find some area in which the two theories cover the same subject matter and make the same assumptions – and what is most important of all, where all parties agree on how to interpret the crucial facts. This is particularly difficult, since the assumptions made by theories are so very different and since adherents of the two theories can very often choose to interpret the facts differently. If you take this approach you can never find any empirical evidence that will absolutely once and for all decide between the two theories to everybody's satisfaction. All you can do is provide evidence relative *to certain assumptions and to a certain interpretation of the data.* Then you can convince people who agree with your assumptions and interpret the data the same way you do. Many of the disputes between generative grammarians and generative semanticists are of this sort. Neither ever convinces the other, but people in the audience who are willing to accept one or the other's assumptions and interpretation of given data will be convinced.

Most of the arguments about *lexical decomposition* fall into this second range. From 1963 to 1969, when it seemed there was a pretty much agreed upon set of assumptions shared by everyone in generative linguistics, it seemed that the lexicon was the place where we could most likely show that transformational grammar was wrong. If lexical items had to be syntactically decomposed into semantic elements, then there could be no independent syntax in the old sense. That is why so much of the argumentation centered on lexical decomposition. It was one area where there seemed at that time to be enough shared assumptions so that one could find empirical evidence against transformational grammar. As it turned out, all that one could find was evidence relative to certain assumptions. For example, I and many others had always assumed that if anything was part of syntax, as opposed to semantics if they could be separated, then *number agreement* was. In other words, I assume that a violation of number agreement would lead one to a syntactically ill-formed sentence. For example:

(24) I are tall.

I had taken it for granted that if anything was ill-formed syntactically, sentences like

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[28] G. Lakoff, "Presuppositions and Relative Grammaticality"; "Linguistics and Natural Logic".
[29] R. Lakoff, "Some Reasons Why There Can't Be Any Some-any Rule"; A. Borkin, "Polarity Items in Questions"; L. Horn, "On the Semantic Properties of Logical Operators in English".
[30] R. Lakoff, "Language and Woman's Place".

(24) were. One could imagine why Perlmutter's observation that number agreement in Spanish had to take semantics into account was of interest to me. It become the basis of an argument to the effect that one had to decompose a lexical item like *parents* into its component parts in order to account for number agreement by a single general rule.[31] If one accepted number agreement as a rule of syntax, that is, if one took (24) to be syntactically ill-formed, then Perlmutter's example showed that generative semantics was right and transformational grammar was wrong. However, at a conference at the University of Texas in 1969, Chomsky was asked about this case, and he replied that he saw no reason to think that (24) was syntactically ill-formed as opposed to being semantically anomalous. He simply chose to interpret the data differently. What this comes down to is that if you think that number agreement is a rule of syntax and that (24) is syntactically ill-formed, then rationally you should believe in generative semantics. If you don't assume that number agreement is a syntactic phenomenon, then Perlmutter's facts prove nothing. Since Chomsky can interpret the data to suit his theory, no absolute arguments along these lines are possible. However, it is still worthwhile to look for relative arguments, since they do force generative grammarians to ever crazier positions.

Is the existence of grammatically ill-formed sentences an argument against your viewpoint?
– No. There are various types of *shallow* and *surface* constraints that do not in any direct way involve semantics. The claim that syntax is not autonomous is not the claim that there are no autonomous rules of grammar. Rather it is the claim that not all rules of grammar are autonomous. As it happens, most of them aren't. This point is often confused. Autonomous syntax claims that *all* rules of syntax are independent of meaning, context, and use. To refute the claim, you only have to find one that isn't.

One needs in linguistics a logic where truth can be a matter of degree...
– Yes. Natural language concepts are fuzzy; therefore natural logic must be a fuzzy logic – that is, a many-valued logic, perhaps even a continuous-valued logic. What I have called 'hedges' show this very clear. Expressions like *sort of, pretty much, rather, strictly speaking, loosely speaking, technically, regular, par excellence*, etc., affect truth values in a way that can only be described adequately if one assumes a fuzzy logic. *Sort of*, for example, raises intermediate values and lowers extreme values. There is even a hedge that requires one to have fuzzy presuppositional logic. The hedge 'to the extent that it makes sense to say that...' raises intermediate nonsense values and lowers extreme nonsense values.[32]

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[31] For the argument, see G. Lakoff, "On Generative Semantics" (1969), distributed by Indiana Linguistics Club, Boomington, Ind.
[32] For details, see G. Lakoff, "Fuzzy Grammar and the Competence/Performance Terminology Game".

Language is not only rooted in thought but also in culture. *Does linguistics have to have a specific sociological theory?*
– Certain theories of social organization may be consistent with the linguistic data and others inconsistent. Linguistic data may very well, at some time in the future, enable one to decide among alternative theories of social interaction – at least for those phenomena that are reflected in linguistic structure.

A part of the culture from which language arises is the ideological *environment. Do you need in linguistics also a theory of the current ideologies? Can one say that there is a link between politics and linguistics?*
– Not on the theoretical level, so far as I have been able to tell. There is as yet no evidence that political concepts are reflected in linguistic structure. *Teaching* linguistics these days is not without some indirect – very indirect – political consequences. In linguistics, as in politics, much of the relevant data to support or refute many claims are available to the average person. In linguistics, it is in your mind and all you have to do is train yourself to recognize it. In politics, it is all around you, in the newspapers and on TV. Again you just have to be trained to recognize it. Just about any beginning linguistics student, with some careful thought, can in an afternoon think up enough crucial examples to show the inadequacy of our most sophisticated current theories. Similarly any citizen of average intelligence can pick out many of the lies that his government tells him. The thought processes are not all that different, though the subject matter is. Any beginning linguistics student will discover with a little thought that men of great stature in the academic establishment, even very bright ones like Chomsky, can be wrong on just about every issue. It makes one wonder about the 'experts' who are running our governments.

If what you are getting at is the question of whether there is any link between Chomsky's politics and his linguistics, I would have to say no. Someone with William Buckley's political views could hold Chomsky's linguistic views without inconsistency. Nor is there anything politically revolutionary about the content of transformational grammar – if anything, it is reactionary today. I have seen some pretty stupid things written about the relationship between linguistics and politics. For example, I read in one report of the French student uprising in 1968 that linguistics became an issue: structuralism was identified with institutional rigidity and transformationalism with change. No one who knows anything about the actual content of structuralist and transformationalist theories could believe any such thing.

Transformationalism as a rationalism is usually opposed to structuralism as an empiricism or a behaviorism...
– That is the typical line you get from reading most books on transformational grammar. I think it's false. Chomsky's theory about the organization of language, that is, that there are deep structures, transformations, etc., is consistent with strict

empiricism; while structuralist linguistic theories about the organization of language (i.e., that there are phonemes, etc.) are consistent with *rationalism*.

Chomsky's argument for rationalism is based on the existence of *complex linguistic universals*. Chomsky always cites examples of putative universals from transformational grammar, but the fact is that just about every other theory of grammar that has ever been seriously proposed has, either implicitly or explicitly, incorporated claims for extremely complex and sophisticated linguistic universals. This is true of structural linguistics, stratificational grammar, tagmemics, Montague grammar, generative semantics, etc. In fact, contrary to what Chomsky suggests, the most extensive studies of complex linguistic universals have been carried out within the framework of structural linguistics. The classic works of the European structuralists Trubetzkoy and Jakobson in phonology and of Joseph Greenberg in American structuralist syntax have been the foundation for all of the more recent (and less extensive) studies of universals done in the tradition of transformational grammar. Chomsky's claims in favor of rationalism over behaviorism do not rest upon his theory of transformational grammar being right and structuralism being wrong. One can make exactly the same argument using the structuralist universals instead, since the universals discovered in structural linguistics are more than complex enough for the purpose of the argument.

One should also be aware of the limitations of Chomsky's arguments for *innateness*. Chomsky has claimed that people possess innately not merely general learning mechanisms, but a specifically *linguistic* innate faculty. His argument is of this form: there are complex linguistic universals that everyone learns uniformly. There are at present no general learning theories that can account for this. It is hard to imagine what any such theories could be like. Therefore, it is plausible to assume that there can be no such theories. The argument is fallacious. Nothing follows from a lack of imagination. What Chomsky has shown is that *either* there is a specifically linguistic innate faculty *or* there is a general learning theory (not yet formulated) from which the acquisition of linguistic universals follows. The former may well turn out to be true, but in my opinion the latter would be a much more interesting conclusion. If I were a psychologist, I would be much more interested in seeing if there were connections between linguistic mechanisms and other cognitive mechanisms than in simply making the assumption with the least possible interest, namely, that there are none.

Chomsky has characterized structural linguistics as being fundamentally behavioristic and concerned solely with taxonomy. This is a misleading view of a broad, diverse, and interesting field, which happened not to be very good at dealing with syntax, and which showed little if any interest in formalized theories. Chomsky's teacher, Zellig Harris, did happen to be an extreme case of a behavioristically-oriented taxonomist. Bloomfield and Hockett, in their theorizing moods, also fit the mold, though one can argue that they did not always adhere to their theories in their linguistic analyses. Though these were prominent structural linguists, they were by no means typical of the wide range of European and American structuralists, either in their interests or in their commitment to behaviorism. Distinguished structuralists like Boas,

Sapir, Jakobson, Pike, Weinreich, Bolinger and Greenberg never had much, if any, commitment to behaviorism. Their interests and their linguistic theories ranged far beyond mere taxonomy to such areas as linguistic universals, the relation between language and culture, dialectal variation, cross-linguistic interference, ritual language, poetics, and much much more. When transformational grammar eclipsed structural linguistics, it also eclipsed many of these concerns, much to the detriment of the field.

Chomsky seems to have shared Harris' commitment to behaviorism to some extent even after developing his own theory of transformational grammar. In Chomsky's earliest book, "The Logical Structure of Linguistic Theory" (which was distributed in mimeoed form but was never published), Chomsky spends a good deal of time discussing the possible development of "operational procedures" for the validation of grammars. He cites the "pair test" favorably as "a thoroughly nonsemantic operational device" and ends Chapter I by suggesting a program of "formulating behavioral criteria to replace intuitive judgments".[33] Though Chomsky rejected Harris' ideal of using behavioral criteria for *discovering grammars*, he, at that point in his career, did not reject the idea of using behavioral criteria for *validating* grammars.

How to define Chomsky's dependence upon his predecessors? Is it possible that he is more empiricist than he would like to be?
– I would say that, of contemporary linguists, Chomsky is among the more empiricist linguists. He is very much wedded to that period of linguistics represented by Harris and Bloomfield, in the sense that he is still principally interested in accounting for distributions of formatives in surface structures without regard to meaning. This was one of the principal characteristics of empiricist linguistics. He has given up working with corpuses, but from our perspective his goals are not all that different from Harris' goals.

It is ironic that his own work has served to move people away from his views of what linguistics is all about. People got interested in transformational grammar not because it gave them a better way of accounting for the distribution of elements in surface structures, but because it gave them a way to approach the study of *meaning*. Transformational analysis seemed to be a way to get closer to logical structure – that was what was attractive about it for most people. Had early transformational analyses not been semantically revealing, no one would have cared about transformational grammar at all. Nowadays students are interested in generative semantics because it is a way for them to investigate the nature of human thought and social interaction. Somehow, the study of the distribution of surface elements as an end in itself couldn't be more boring to most people, including me.

Chomsky was extraordinarily dependent on his teachers for his intellectual development. Most of his early linguistic analyses are taken directly from Harris, as is the idea

[33] N. Chomsky, "The Logical Structure of Linguistic Theory" (Cambridge, Mass., 1955), I-36e and I-59.

of transformations. The idea of evaluation metrics was taken over directly from Nelson Goodman. As much as Chomsky has rebelled against the empiricist views of Harris and Goodman, he has, I believe, still essentially retained their views of the relationship between meaning and the study of natural language.

Is transformational grammar essentially a 'Cartesian linguistics'?
There seems not to have been any such thing as 'Cartesian linguistics'. Chomsky claims in *Cartesian Linguistics*[34] that Cartesian rationalism gave birth to a linguistic theory like transformational grammar in its essential respects. He bases his claims on the *Grammaire Générale et Raisonnée* by Antoine Arnauld, a disciple of Descartes', and Claude Lancelot, a language teacher, published in 1660. The *Grammaire Générale* followed a series of other grammars by Lancelot, the most extensive being his Latin grammar. Chomsky never checked out his Latin grammar (an English translation of which was in Widener Library) but Robin Lakoff did, and published her findings in *Language*.[35] She discovered that in the introduction Lancelot credited all of his interesting findings to Sanctius, a Spanish grammarian of the previous century whose work antedated Descartes by half a century. Checking into Sanctius, she found that Lancelot was not being modest. He had indeed taken all of his interesting ideas from Sanctius. In short, what Chomsky called 'Cartesian linguistics' had nothing whatever to do with Descartes, but came directly from an earlier Spanish tradition. Equally embarrassing for Chomsky's claims is the fact that the theories of Sanctius and the Port Royal grammarians differ from the theory of transformational grammar in a crucial way. They do not acknowledge the existence of a syntactic deep structure in Chomsky's sense, but assume throughout that syntax is based on meaning and thought. Chomsky has steadfastly opposed this position from his earliest works straight through to his most recent writings.

The publication of Syntactic Structures *in 1957 had been called a revolution in linguistics because grammar became then* generative *and transformational. What does it mean for you that grammar has to be* generative*?*
– Transformational grammar was started by Harris, not Chomsky, and most of the actual linguistic analyses that you find in *Syntactic Structures* were present in Harris' work; the linguistic content is not that much different from what you find in Harris.

So far as *generative* grammar is concerned, 'generative' simply means 'complete and precise'. The idea is that a grammarian formulates formal rules that characterize just what is and what is not in the language. The reason that such rules are formulated as *operations* rather than as static statements is that Chomsky got the idea from recursive function theory. I have called grammars of this sort *production grammars*, taking the term from Post productions, which are abstract directional operations on strings.

[34] N. Chomsky, *Cartesian Linguistics* (New York, 1966).
[35] R. Lakoff, "Review of *Grammaire Générale et Raisonnée*", *Language* 45 (1969), 343-364. Also, H. Aarsleff, "The History of Linguistics and Professor Chomsky", *Language* 46 (1970), 570-585.

Following ideas of McCawley, I have suggested replacing production grammars with what I have called *well-formedness grammars*. Well-formedness principles are static in nature, rather then being directional operations. Well-formedness grammars can do everything transformational grammars can do and more, since they also provide a natural way for formalizing global, transderivational, fuzzy grammars. In generative grammars it was assumed that a grammar generated *sentences*. In generative semantics it is assumed that a grammar generates *quadruples* of the form (S, LS, C, CM), where S and LS are linked by a *derivation*.[36]

What is the right sense of 'generative' in generative semantics *?*
– 'Complete and precise'. As I mentioned, 'generative semantics' is not a very accurate descriptive term for what we are doing. When I first used the term back in 1963, it was an amalgam of 'generative grammar' and 'interpretive semantics', since I was claiming then that there should be no line drawn between syntax and linguistic semantics. In those days, I even assumed that transformational grammars were basically correct. I didn't give up entirely on transformational rules until 1969.

Is Chomsky's conception of generativity *in language not linked with his more general and sometimes ideological idea of* creativity *?*
– Chomsky has spoken of 'the creative aspect of language *use*', by which he means that people can make up new sentences in new situations. This is a completely new and very strange use of the word 'creative', since it has nothing whatever to do with creativity in the ordinary sense of the word. There is nothing in transformational grammar that accounts for human creativity or that even pretends to do so. All that transformational grammar does is provide a recursive mechanism for generating sentences. There is nothing 'creative' about this. It is like constructing a computer program to do arithmetic. The program could perform an infinity of arithmetical operations, but no one would say that it accounted for mathematical creativity.

Before concluding, some few more technical questions. Chomsky argues against generative semantics that it is only a notational variant *of his extended standard theory…*
– That is a very strange thing for him to say, since he also thinks generative semantics is wrong. If generative semantics were only a notational variant of the extended standard theory, then by claiming that generative semantics is wrong, Chomsky would be claiming that his own theory was also wrong. There is an air of contradiction here, to say the least.

Generative semantics is very very far from being a notational variant of the extended standard theory – and I think it takes a considerable amount of *chutzpah* for Chomsky to even suggest that it is. If you ask whether two theories are notational variants, the very first question you ask is whether they cover the same subject matter. As

[33]   Cf. p. 163 of this book.

was pointed out above, they don't. Just consider the principal areas of reasearch in generative semantics these days: *Natural logic* (that is, the study of human reasoning) including logic of fuzzy concepts, and the model-theoretical interpretation of logical structures; *Pragmatics*, including the appropriate use of language in context, especially social context, performatives, speech acts, implicatures, conveyed meaning in general, deixis, discourse types and styles of speech; *Fuzzy grammar*, including degrees of category membership and degrees of rule application. To my knowledge none of these is even part of the subject matter of the extended standard theory. So far as I have been able to tell, Chomsky and other lexicalists don't even consider these things to be part of the study of the structure of language; they seem to have arbitrarily defined them as being outside linguistics, they seem to have swept them under the rug of 'performance'. Since 1969, these have been the principal areas of study in generative semantics. They are not part of the subject matter of the extended standard theory. Since generative semantics and the extended standard theory don't even come close to having the same subject matter, they cannot conceivably be notational variants. In fact, it is hard to imagine how anyone who has kept up with developments in generative semantics since 1969 could even make such a claim.

It has been suggested to me (by Arlene Berman, personal communication) that there might be one way of interpreting such a claim. Suppose we limit ourselves to just that subject matter where generative semantics and the extended standard theory overlap. Could it not then be the case that, *only with respect to that subject matter*, generative semantics and the extended standard theory are notational variants? This would be like suggesting that transformational grammar and phrase structure grammar are notational variants, if you just ignore all transformational phenomena. The question misses the point of bothering to do generative semantics, namely to account for that subject matter. But in addition, in this case, I don't even think the question makes any sense. The subject matter of the extended standard theory is the distribution of morphemes and the relation between surface sentences and literal meanings (without any model-theoretical interpretation); its assumption is that this is a coherent subject matter. But one of the principal claims of generative semantics is that this is not a coherent subject matter; instead, the distribution of morphemes depends upon various aspects of context, conveyed meaning, and the model-theoretical interpretation of logical structures. If one cuts out context, conveyed meaning, and model-theoretical interpretation from generative semantics, one does not get a coherent subtheory.

These arguments concern the non-similarity of the scope *of the two alternatives. But even the conceptual apparatus of the two theories is very different, at least apparently…*
– The theoretical mechanisms are very different, indeed. The extended standard theory has phrase structure rules, transformations, and surface (and deep) interpretive rules – which are not yet specified as to what their outputs are or what their form is. Generative semantics has correspondence rules, global rules, transderivational

rules, and principles of model-theoretical interpretation for logical structures (which are taken to be universally given). There is nothing like a one-to-one correspondence between the mechanisms of the extended standard theory and generative semantics. Moreover the grammatical categories are very different and cannot be matched up in anything like a one-to-one fashion. The extended standard theory has primitive categories like N, A, V, N̄, Ā, V̄, N̄̄, Ā̄, V̄̄, AUX, M(odal), DET(erminer), Sentence adverb, COMP, etc. It also uses syntactic features; in fact, it requires an infinite number in order to avoid having to use global rules.[37] For us, the primitive grammatical categories are the categories of logical structures: S, PRED(icate), ARG(ument). There are secondary (nonprimitive) categories which are defined in terms of primitive categories and global properties of derivations; membership in them is taken to be a matter of degree.[38] Because secondary categories are fuzzy, there is no possible one-to-one mapping between either the primitive or secondary categories of generative semantics and the categories of the extended standard theory. If the formal mechanisms are different and the categories are different and incapable of being set in a one-to-one correspondence, then how could the theories possibly be notational variants?

For two linguistic theories to be *notational variants*, at least the following would have to be the case: (a) they would have to have the same subject matter; (b) their formal mechanism would have be able to be set in a one-to-one correspondence; (c) their grammatical elements would have to be set in a one-to-one correspondence; (d) there would have to be a one-to-one correspondence between their linguistic analysis of particular sentences, for all natural languages – in other words, they would have to make the same claims about every sentence of every natural language.

We have seen that (a), (b), and (c) do not hold. It should be obvious that (d) also does not hold. In fact, the theories are so very different that I cannot even imagine one sentence of any language for which the theories provide the same analyses, or for which they even come close. Certainly the burden of proof is on someone who wants to claim that the theories are notational variants. He would have to show, at the very least, that (a)-(d) hold. No one who has made such a claim has tried to undertake such a demonstration. All that those who make such claims have done is to draw little diagrams with boxes and arrows to represent generative semantics and the extended standard theory, and to show that one can relabel the boxes to suit one's fancy. At this level of vagueness any theory can be made to have the same little-box-structure as any other theory. But until you look at the details of the proposals and at the very least satisfy (a)-(d) above, drawing all the little boxes in the world won't prove a thing.

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[37] See G. Lakoff, "The Arbitrary Nature of Transformational Grammar", *Language* 48 (1972), 76-87.
[38] See G. Lakoff, "Fuzzy Grammar and the Competence/Performance Terminology Game".

Another general remark made by Chomsky about generative semantics is that this theory is too powerful...
– First, if you put this claim of Chomsky's together with his notational variant claim, he turns out to be claiming that his own theory is too powerful, since it would be in one-to-one correspondence with a theory that was too powerful. Luckily for Chomsky, the theories are not notational variants. Secondly, with respect to weak generative capacity (which is about as uninteresting a subject as I could possibly imagine for a linguist) Peters and Ritchie have shown that transformational grammars can be made to mimic any Turing machines; in other words, it is impossible for there to be a theory which is more powerful than transformational grammar so far as weak generative capacity is concerned.

The 'too-powerful' issue is usually directed at *global rules*. The only proposal to handle the same phenomena with nonglobal rules has been the Baker-Brame proposal, which would permit grammars to have an infinite number of grammatical categories.[39] That is hardly a proposal that reduces descriptive power. Moreover, the same people who claim that global rules are "too powerful" are more than willing to admit surface interpretation rules with no constraints whatsoever on them. Moreover, it usually forgotten that only global rules of a very restricted sort have been proposed. Just as transformational grammar admitted not every conceivable type of mapping from trees into trees but rather only a limited number of types of operations, so only a small number of types of global rules have been proposed. The real problem with global rules is not that they are too powerful, but that they are too *weak*. They appropriately handle a certain range of phenomena, but they are entirely inadequate to deal with most natural language phenomena, just as all other types of rules proposed so far have been. Anyone who believes that "you can do anything with global rules" ought to sit down and try sometime. The problem with all current theories is that they are just too weak to deal with most linguistic phenomena. Anyone who wants to convince himself of this should read the collected works of Robin Lakoff, Charles Fillmore and Dwight Bolinger.

Can I ask you the explanation of one of your statements that I consider as central for your conception of language, namely, that "the form of language cannot be studied independently of its function"...
– As I mentioned above, recent results indicate that the syntactic form of sentences is not independent of the *meanings* that they convey in *context*. In trying to account even for the distribution of morphemes, one must take into account not only the literal meaning of the sentence but also what you are communicating indirectly and how you are doing it; the function of the utterance in terms of communicative interaction cannot be ignored. One must consider both the expressive and communicative functions of language at the same time.

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[33] C. Baker and M. Brame, "Global Rules: A Rejoinder", *Language* 48 (1972), 51-75.

Modern linguistics was always operating with strong dichotomies. Is it your explicit purpose to transcend all these boundaries and distinctions?

— Yes. In fact, that is one of the most interesting things coming out of generative semantics. We have found that one cannot just set up artificial boundaries and rule out of the study of language such things as human reasoning, context, social interaction, deixis, fuzziness, sarcasm, discourse types, fragments, variation among speakers, etc. Each time we have set up an artificial boundary, we have found some phenomenon that shows that it has to be removed. That is not to say that there are no bounds on the study of linguistics. I only suggest that at this point in history the boundaries are disappearing daily, and one should not be too surprised if the domain of the field continues to expand.

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