DOCUMENT RESUME

ED 084 907 FL 004 507

AUTHOR	Wilks, Yorick
TITLE	Lakoff on Linguistics and Natural Logic.
INSTITUTION	Stanford Univ., Calif. Dept. of Computer Science.
SPONS AGENCY	Advanced Research Projects Agency, Washington, D. C.
REPORT NO	STAN-CS-72-289
PUB DATE	Jun 72
NOTE	21p.
EDRS PRICE	MF-$0.65 HC-$3.29
DESCRIPTORS	Communication (Thought Transfer); Concept Formation; Deep Structure; *Linguistic Theory; *Logic; *Morphology (Languages); Phrase Structure; Psycholinguistics; *Semantics; Surface Structure; Syntax; Transformation Generative Grammar; Transformations (Language); Translation
IDENTIFIERS	*Lakoff (G)

ABSTRACT

This paper examines and criticizes Lakoff's notions of a natural logic and of a generative semantics described in terms of logic. The author argues that the relationship of these notions to logic as normally understood is unclear but suggests a number of possible interpretations of the thesis of generative semantics. Further, on these interpretations, the thesis of generative semantics is considered false, unless it be taken as a mere notational variant of Chomskyan theory. In addition, he asserts that Lakoff's work may provide a service in that it constitutes a reductio ad absurdum of the derivational paradigm of modern linguistics and shows that only a system with the ability to reconsider its own interferences can do the job that Lakoff sets up for linguistic enquiry. (Author/DD)

FILMED FROM BEST AVAILABLE COPY

ED 084907

STANFORD ARTIFICIAL INTELLIGENCE PROJECT
MEMO AIM-170

STAN-CS-72-289

LAKOFF ON LINGUISTICS AND NATURAL LOGIC

BY

YORICK WILKS

U S DEPARTMENT OF HEALTH, EDUCATION & WELFARE
NATIONAL INSTITUTE OF EDUCATION
THIS DOCUMENT HAS BEEN REPRODUCED EXACTLY AS RECEIVED FROM THE PERSON OR ORGANIZATION ORIGINATING IT POINTS OF VIEW OR OPINIONS STATED DO NOT NECESSARILY REPRESENT OFFICIAL NATIONAL INSTITUTE OF EDUCATION POSITION OR POLICY

SUPPORTED BY
ADVANCED RESEARCH PROJECTS AGENCY
ARPA ORDER NO. 457
JUNE 1972

COMPUTER SCIENCE DEPARTMENT
School of Humanities and Sciences
STANFORD UNIVERSITY

LL004 507

STANFORD ARTIFICIAL INTELLIGENCE PROJECT
MEMO NO. AIM-170

COMPUTER SCIENCE DEPARTMENT
REPORT NO. 289

LAKOFF ON LINGUISTICS AND NATURAL LOGIC

by

Yorick Wilks

ABSTRACT: The paper examines and criticises Lakoff's notions of a natural logic and of a generative semantics described in terms of logic, I argue that the relationship of these notions to logic as normally understood is unclear, but I suggest, in the course of the paper, a number of possible interpretations of his thesis of generative semantics. I argue further that on these interpretations the thesis (of Generative Semantics) is false, unless it be taken as a mere notational variant of Chomskyan theory. I argue, too, that Lakoff's work may provide a service in that it constitutes a reductio ad absurdum of the derivational paradigm of modern linguistics; and shows, inadvertently, that only a system with the ability to reconsider its own inferences can do the job that Lakoff sets up for linguistic enquiry----that is to say, only an "artificial intelligence" system.

The views and conclusions contained in this document are those of the author and should not be interpreted as representing necessarily the official policies, either expressed or implied, of the Advanced Research Projects Agency or the U.S. Government.

This research was supported by the Advanced Research Projects Agency, Department of Defense (SD 183), U.S.A.

Reproduced in the U.S.A. Available from the National Technical Information Service, Springfield, Virginia, 22151.

I)INTRODUCTION

In this paper I want to examine George Lakoff's discussion of natural logic in some detail, and then to examine his, closely connected, thesis of Generative Semantics [GS]. He writes:

"Generative semantics claims that the underlying grammatical structure of a sentence is the logical form of that sentence, and consequently that the rules relating logical form to surface form are exactly the rules of grammar."[16, p. 18]

It will be one of the principal tasks of this paper to argue that this claim is false , at least if its words have anything like their normal meanings.

Two immediate comments are appropriate about this general thesis: (a) about its consequences; and, (b) about its originality. The consequences of the thesis, if it is true, seem to me not to have been sufficiently appreciated. For this thesis of Lakoff's is in a sense a reductio ad absurdum of modern linguistics, in that it would be reduced to a mere handmaiden of logic or worse, whose only residual role would be to provide the details of the translation of sentences into logical form, which would then be the real, or primary, structure of language. If Lakoff is right in this matter then Chomsky's whole enterprise of the last 15 years, to construct a formal linguistics independent of logic, has been a radically misguided one from the start. Some of us could contemplate that possibility perhaps, but not the other consequence of GS that there cannot be any other, non-Chomskyan, linguistics independent of logic either.

As to the originality of Lakoff's thesis, it seems to me interesting to point out that it is not as novel or striking as is sometimes assumed: it consists in taking seriously an idea floated by BarHillel in 1954[4], and to which Chomsky replied in one of his earliest, least known, and best papers[7].$$

In order to make this point let me sketch briefly what seems to me the flow of energies from logic to linguistics in this century. That there are differences between the analyses that grammarians and logicians provide of the same sentences has been remarked on for millenia. And much of what, in recent years, has been called the difference between deep and surface structures is no more than a relabelling of that difference. The distinction took a sharper form when Russell and Whitehead provided a notation in which to make points such as that "John loves" and "John exists" have the same grammatical forms, in some sense of those words, but different logical forms: in that "loves" could be represented by some predicate in the predicate calculus, while "exists" could not.

$$ I am indebted to Prof. Julius Moravcsik for bringing this paper to my notice.

It is not necessary to agree with that particular point of Russell and Whitehead's to accept that the high point of that whole way of thinking------that logic was "deeper" than grammar---- was Carnap's Logical Syntax of Language, where he wrote [6]:

"By the logical syntax of a language we mean the formal theory of the linguistic forms of that language -----the systematic statement of the formal rules which govern it, together with the development of the consequences which follow from these rules. . . . The difference between syntactical rules in the narrower sense and the logical rules of deduction is only the difference between FORMATION RULES and TRANSFORMATION RULES, both of which are completely formulable in syntactical terms. Thus we are justified in designating as "logical syntax" the system which comprises the rules of formation and transformation".

For Carnap the formation rules of the logical syntax of a LOGICAL language were to be the rules that produced all and only the well-formed formulas of the system. The best contemporary example was the provision by Jaskowski [14]) of a set of phrase structure rules producing the formulas of the propositional calculus. In the case of a NATURAL language Carnap thought of the formation rules as an extension of linguistic syntax (as THAT was then thought to be): in Carnap's view linguistic syntax would prohibit the string "Caesar is and" whereas LOGICAL syntax would prohibit "Caesar is triangular", while producing "Caesar is brave".

It was Bar-Hillel who, in the paper I mentioned, reminded linguists of this largely forgotten work of Carnap. Bar Hillel argued that someone, a linguist presumably, should extend Carnap's work in detail, and moreover that he should go further and use the Carnaplan notion of transformation to bring much of conventional logic within linguistics. Bar-Hillel wrote: "There exists a concept of syntax, due to Carnap, that is purely formal(structural)and adequate in a sense that the concept prevalent among American structural linguists is not. This conception entails a certain fusion between grammar and logic , with grammar treating approximately the formational part of syntax and logic its transformational part. The relation of COMMUTABILITY may be a sufficient basis for formational analysis, but other relations, such as that of formal CONSEQUENCE , must be added for transformational analysis". [4]

The main point Bar Hillel was making that concerns us here is his claim that the notion of logical consequence has a proper place within linguistics. It was Chomsky who replied to this paper of BarHillel's, pointing out that the notions of inference and consequence have nothing at all to do with that of syntax or well-formedness, except in Carnap's own rather trivial sense of syntax as covering any formal operations whatever. Several important and closely related historical points should be noted here:

1) When he replied to Bar-Hillel Chomsky had not produced his now well known theory of grammar. However, when he did so, making use of two well worn philosophical metaphors (Carnap's of formation vs. transformation; and Wittgenstein's of surface vs. deep grammar) Bar Hillel took Chomsky to have been following up his own suggestion. For indeed Chomsky had made use of the WORD "transformation" just as Bar-Hillel wanted. What Chomsky had NOT done was to make use of the word's content, for Chomsky's transformations(just like those of Harris before him) had only the form of Carnapian ones:that is to say the passage from one string to another, the passage was not to be interpreted as one from truth to truth for the reasons Chomsky set out in his original reply to Bar-Hillel.

2)This is not inconsistent with the fact that certain vestiges of Carnapian transformational CONTENT did remain in Chomsky's transformations---the relation between Chomsky's paradigm cases "John loves Mary" and "Mary is loved by John" WAS that of valid inference (Carnapian transformation). Chomsky also hankered after the establishment of some relation between his kernels and logical forms [9, p. 162]. However, these as I said were vestiges, and need not have been there. (Though it must be admitted that at various subsequent times , Chomsky has claimed that transformations ARE meaning-preserving, and thus are rules of valid inference.) With the construction of his own system, Chomsky was genuinely extending the work of Carnap, though not in the way BarHillel called for. As will be seen from Carnap's example that I quoted, the rejection as improper of "Caesar is triangular", this was just the sort of task that Chomsky's transformations were to carry out

3)So, I shall argue that Lakoff's GS is, in a sense, a return to Bar Hillel's 1954 suggestion, rejected at the time by Chomsky, and in no way to be found in Carnap himself. For Carnap, generational differences in the formation rules of a logical syntax were categorial (that is to say, phrase structure)matters , and had no obvious relationship to questions of logical form as Lakoff thinks they do. For example, in the triangular/brave case , the difference would be expressed by Carnap with the aid of categories----for both these sentences would be related to a logical form P(c), or $\exists x$, (x=c. P(c)). The difference between them would lie simply in the fact that if c was Caesar then P could be Bravery but not Triangularity.

My points so far have not been intended to defend Chomsky in any particular way, but only to point out that ,with his thesis of GS,Lakoff is claiming what Carnap never considered, what Bar Hillel adumbrated and Chomsky rejected, nearly twenty years ago. Moreover, that controversy of 1954 in no way said the last word on the difficult questions involved, particularly because the starting point of BarHillel's argument was the now largely irrelevant question of distributional analysis. In addition, it will take some argument on my part in the body of this paper to establish my assumption that Lakoff's GS not only considers logical forms to be linguistically basic, but also considers the derivations from them that establish

surface structures in a natural language to have inferential content. And so that when Lakoff wrote of grammatical rules as "relating logical form to surface form" he did not mean simply rules of translation. I shall have no difficulty in establishing this from the examples Lakoff uses, hence GS is a thesis that does bring the notion of consequence into linguistics, and in an even stronger form than Bar Hillel looked for. For, in the paper I referred to, Bar Hillel thought that consequence would probably not be necessary for what he called "formational analysis".

I shall argue, then, that the notions of inference with which Lakoff is concerned are of no particular linguistic interest. Moreover, in that many of the inferences with which he concerns himself are what would normally be called inductive inferences, they are of no particular logical interest either.

By that I intend to refer to the whole area of inferences that humans make on the basis of what they see, hear, know, and remember, but which are not VALID inferences, in that they may well turn out to be wrong. For example, if we hear someone say "Please sit down", we may infer, as a matter of social habit, such things as that there is a chair in the presence of the speaker;that whatever is spoken to is human; that in obeying the request, if he does so, the hearer will move downwards (though he may already be lying down). Any or all of these inferences may be true, and may moreover be usually true, but may also be false on any particular occasion . These inferences are all inductive, habitual, empirical, but have no interesting logical content, however, because they are not valid inferences.

If, and I shall show this below, the inferences Lakoff deals in are of this sort, then his system cannot be any sort of logic, other than a probabilistic logic, which is not in question here. However, it seems to me that in bringing linguistic attention back to the subject of general inference(which I take to include inductive inference)Lakoff may be doing a service. For the enterprise that IS concerned with such inferences is neither logic, nor linguistics as traditionally understood, but artificial intelligence[AI]. AI is much concerned with the construction of a human-like reasoning and understanding system, and that is no small or unworthy task.

My view is that such inductive rules can only be a useful part of a mechanism which is able to FOLLOW UP these, possibly mistaken, inferences to see whether or not they are justified by the information reaching the system later, and hence is able to abandon erroneous inference where possible. No such procedure is possible within the conventional paradigms of logic or linguistics; certainly not within inductive logic in the probabilistic form in which those words are normally taken. Only within some such context as artificial intelligence, then, does it make much sense to discuss the sort of dubious inference I am referring to, such as whether or not a chair was present in the "Please sit down" example I gave earlier.

Now, there should be nothing astonishing in claiming that, if Lakoff's work has a proper place it is within AI, for most modern linguistics has been fundamentally concerned with the construction of a possible mechanism, even if in a less clear way than Lakoff's work is, and even though the linguists in question might utterly reject my description of what they have been up to all this time.

Chomsky's self-imposed task, it will be remembered, was the description of a possible mechanism that would generate all and only the language strings satisfying some criterion of correctness. That remains the fundamental description of what Chomsky was aiming at, even though it is now called "weak generative capacity", and the criterion of correctness itself has wobbled a bit over the years, and even though Chomsky has given other desiderata that the mechanism also had to satisfy; such as being a scientific description of data, reducing them to order; describing a possible mechanism by which humans IN FACT produce language; formalising the structure that humans THINK their language has, and so on.

There seems to be a continuing confusion in current linguistics on this point, in that, in their eagernesss to disclaim any intention to model the mind or brain or other processes of an actual speaker, some linguists have gone too far and disavowed the original gospel of sentence production as well. The task of the linguist is then thought to be to assign descriptions to individual sentences, but by methods which must remain wholly mysterious if he has already rejected all specific analytic or productive [1 9] algorithms. As I shall show, Lakoff himself is in danger of falling into this particular procedural limbo. Chomsky's original description [8] of his own enterprise was undoubtedly productive, and even when he came later to clarify the notion of "generation" he continued to draw the analogy with Post derivations in logic [9, p. 9] which are paradigms of directed mechanical sentence production.

Perhaps I have set rather a wide and extensive scene for the detailed discussion that follows, but then Lakoff himself does consider his own contributions to be fundamental, and not merely peripheral, to linguistics. I must now establish three points by detailed reference to Lakoff's text, in order to justify the rather large general claims of this introduction. They are (1) that the thesis of GS uses consequence to establish linguistic well formedness; (2) The notion of consequence used is frequently inductive consequence; (3) That the thesis of GS, unless it is merely a notational variant of existing linguistic theory, is false whether or not it rests on a notion of consequence.

In the two sections that follow I first examine the notion of a natural logic and then proceed to the central thesis of GS.

II) THE NOTION OF A NATURAL LOGIC

It is proper to ask first what Lakoff means by a natural logic. He writes (16, p. 54):

"(iv) We want a logic in which all the concepts expressible in natural language can be expressed unambiguously, that is, in which all non-synonymous sentences ------ have different logical forms.

(v) We want a logic which is capable of accounting for all correct inferences made in natural language and which rules out incorrect ones. We will call any logic meeting the goals (above) a "natural logic"".

Again (ibid, , p. 58):

"In natural logic . . . . logical equivalences could not just be arbitrarily set down; rather they would be just those necessary to characterize the notion "valid inference" for natural language arguments".

And again (ibid, , p. 126):

"Natural logic, taken together with linguistics, is the empirical study of the nature of human language and human reasoning".

This all sounds a very nice idea, and generally a good thing, but what does it really come to? These quotations, for example, taken together, express a curious ambivalence towards formal logic that runs right through Lakoff's paper. Lakoff writes of a natural logic in terms of the general study of human reasoning, but the fact is that most real human reasoning is of a sort that is of interest to no one but psychologists, and sometimes psychiatrists. Real people argue much of the time along the lines of "That man has a squint, therefore he probably wants to mug me". And, of course, sometimes they are right in such inferences. The notion of inference, as such, has no real logical content: inferences are just the inferences that people actually make. Philosophers from Moore [20] to Lakoff [ibid, , p. 9] have criticised the basic connective "⊃", of material implication, in the Propositional Calculus, on the grounds that it in no way expressed the natural usage of "if. . . . then" in ordinary language, because it allows any statement to imply any other, as long as the first is not true while the second is false. In the Propositional Calculus one could properly say that "The Apollo space craft is nearing the moon" implies "I have a head-ache coming on" if indeed I do, and that has always seemed to me quite a fair picture of how people actually reason in real life. But more seriously, Lakoff also refers, in the passages I quoted, to "valid" and "correct" inference when setting out what a natural logic is to be. "Valid" is a reasonably well-understood term and covers such inferences as "all f's are g and all g's are l, therefore all f's are l", as well as those like "John is a younger son, therefore John

has a brother".

We can easily construct a sense of "correct" inference, too, different from that of "valid inference" but still of interest to logic. For example, and to use an old logical favorite, we can infer from "This is a creature with a heart" that "This is a creature with a liver". We can do this because the missing premise is universally true, since all creatures with hearts do as a matter of fact have livers, though it does not depend on the meanings of words as does the "younger son" case. But such inferences will be correct in some clear sense in that they will (while the world stays roughly the same as now) always lead from true premises to true conclusions, and so a "natural logic" should probably be concerned with them. But, and this is my point, what does Lakoff think logicians, traditional and modern, have been up to, if not the discussion and investigation of such valid, and sometimes, correct inferences?

To be precise, does Lakoff present any valid or correct inferences in his paper, as part of a proposed natural logic, that have not been extensively discussed by logicians in the normal course of their job? I would think not, and this leaves me puzzled as to what Lakoff intends the distinctive contribution of his natural logic to be.

There are, in his paper, a considerable number of relationships established of the sort that have constituted one of Lakoff's contributions to linguistics; such as that we can go from "Last night Sam smoked pot" [ibid., p. 4] to "Sam smoked pot last night", but not necessarily from "Last night I realized Sam smoked pot" to "I realized Sam smoked pot last night". All of which is perfectly true, but the first example is not, I should have thought, what a believer in a natural logic would want to call a valid inference, in that inferring a trivial synonym from another is the sort of thing that logicians do, and Lakoff complains of, rather than a real life natural inference. For who would actually say "Sam smoked pot last night, therefore last night Sam smoked pot"?

Now there are indeed inferences to be found in Lakoff's paper, that are real world inferences, but would not be found in a logic book. However, they also have the drawback mentioned earlier, that they are not valid, or even correct, in the sense defined above.

Lakoff writes [ibid., p. 42]:

"(34)a. Nixon refused to try to shut Agnew up. . . . . (34a) entails (35a). . . . . (35)a. Nixon didn't try to shut Agnew up".

If Lakoff is using "entail" in its normal sense to cover valid inferences, those where the consequent must be true if the antecedent is, then what he claims is just not so. To refuse to do something is to decline, to perform a verbal act, and is so described in both American and British dictionaries. It is perfectly possible to refuse to do x and then to do it, even though as a matter of fact it

may be usual not to do x once you've refused to.

Again [ibid. , pp. 8-10] Lakoff argues at length that the sentence "One more beer, and I'll leave" is derived from a sentence containing "if" such as "If I drink one more beer then I'll leave", and the force of the example is that there is a relation of consequence between the two sentences in the derivation (of one from the other), in which case he is saying that "If I have one more beer then I'll leave" entails "One more beer and I'll leave". But that again is not so, for I might neither have another beer nor leave, in which case "If I have another beer then I'll leave" is still true, but "I'll have one more beer and I'll leave" (a natural meaning of the consequent) is false, and so there can be no entailment, since the antecedent with "if" is true, and the consequent is false.

Now, I may have interpreted the whole notion of GS wrongly (see below part III) in that the derivation relation here is not intended to be consequential. But IF IT IS then here again is a very shaky form of inference at the heart of the system, and one which, as I argued in the first section, will just not fit into the standard logical or linguistic derivational paradigms, but only into one that has the capacity to find out that it has inferred wrongly and to try again.

While pointing out that modern logic is still concerned with valid inferences, it must also be admitted that much of Lakoff's criticism of its preoccupations is true. His demonstrations of the ways in which logical calculi fail to capture the awkward proliferations of language are familiar to readers of Moore, Wittgenstein and Austin, but nevertheless valuable as reminders, in that the arrogance of logicians about language blossoms again in every generation as if it had never been trimmed. Again, much of the preoccupation of logicians with the axiomatisation of logic is hard to understand for those concerned with the problems of language, and indeed Kneale [15] has pointed out that there is something rather odd about wanting to axiomatise logic itself (which is where much logical energy has gone in this last fifty years): axiomatisations always used to be of some area of subject matter, such as geometry, using the techniques of logic.

But here again, things are not as bad as they might seem, and even the most foundational logicians are aware that their formal systems must respect the valid inferences of some area of discourse. The trouble is, from the point of view of those interested in language, that the area of discourse that many or all foundational logicians are interested in is mathematics, not natural language.

But some of these foundational concerns should be of ultimate concern to Lakoff in the construction of a natural logic. Supposing he were able to do what appears to be his aim; to put together an enormous number of postulates or rules of inference for natural language argument. It would surely be important to know if they were consistent: for the fact that speakers felt sure about each of them

individually would not guarantee that consistency at all. At one point, though, [ibid. , p. 94] Lakoff does refer in passing to one of his postulates as a theorem, and theorems are derived from axioms, so perhaps he does have in mind some ultimate axiomatisation and test of consistency. However, there are other more immediate barriers in the way of such an assemblage of postulates. When I wrote above of Lakoff's ambivalence towards logic, I had in mind his switch from, often justified, criticisms of formal logic to an extraordinary degree of acceptance. One form of it in this paper is an assumption that one can assemble an aggregate of postulates by picking and choosing from different areas of logic.

The overall format of Lakoff's paper is in fact a gentle meander through different areas of logic: quantifiers, meaning postulates, modal logic, model theory. But there is no system suggested at any point, only an aspiration and a new notation with every subsection. It is not at all self-evident that all these notions can be usefully combined in one system. Meaning postulates and model theory, for example, represent very different ways of going about doing logic.

Another form of Lakoff's over-confidence in logic is his apparent assumption that a number of logical concepts offer firm tools for the job he has in mind. One such is entailment, which Lakoff somewhat misunderstands as we saw. The basic notion of entailment is fairly clear, but it cannot be pressed too far or it slips through the fingers. Some philosophers would certainly argue that the so-called "paradoxes of entailment" have this effect, and, from another point of view, Quine [23] has given much time to arguing that the notion be dropped, though if one did so it is not easy to see how we would continue to describe many of what seem to be perfectly clear valid inferences in natural language. Another case is that of presupposition, which is an extremely difficult notion, partly because the usual definition of it, such as Strawson's, is in terms of entailment: [27] "S presupposes S' if and only if "S is truth valued" entails S' This is a very difficult notion to apply to real language examples, and most of the logicians who have made use of a formal notion of presupposition have kept it safely within calculi. But Lakoff uses it blithely where entailment would seem more appropriate. He tells us [ibid. , p. 51] that "Sam realizes that Harry is a fink" presupposes that "Harry is a fink", which sounds alright in an everyday sense of presuppose but if Harry is not a fink do we really want to say that "Sam realizes etc. " has no truth value? It seems to me much more straightforward to say that it is NOT TRUE in that case that Sam realizes etc. , because he only thinks he realizes etc. And if it is not true then we do not have a presupposition, but something more like an entailment.

A footnote [ibid., p. 133] suggests that Lakoff is aware of definitions of "presupposition" like the one of Strawson's quoted above, and that there is some need to beware of confusing the two. Moreover, he explores the notion of the transitivity of presupposition with some care, and contrasts its failure in certain

cases with the transitivity of entailment(though that is disputed too, of course, but let us ignore that). But then come such revealing footnotes as [17, p.235n.] where he says that some philosophers might prefer to use "pragmatic implication" for what he calls presupposition. I think perhaps Lakoff is right, and that indeed is very much what I wrote earlier when I accused him of confusing entailment with mere inductive inference. And, as I argued there, that is a very damaging criticism of Lakoff's whole case about logic---even if it comes as an admission from his own hand. For neither linguistics nor logic proper can really handle an inductive logic that may infer wrongly at any point.And,even if there could be an inductive logic adequate to such a task,it would be an odd claim that mastery of English also required mastery of that logic.

So then, I do not see why Lakoff clings so strongly to the notion of presupposition in his linguistic description and to claims such as [ibid, p. 52] "An account of the logical form of a sentence must include an account of the presuppositions of that sentence";and [ibid., p. 51] "Then we will say that the surface form S1 can be related to the logical form L1 ONLY IF (my capitals) the relation, ... holds between L1 and L2. . . . . ". For these claims just cannot be justified in terms of the notion of presupposition as normally understood;even though in certain cases, such as Russell's Theory of Definite Descriptions, there are well known arguments for incorporating certain existence assumptions into a logical form,yet those cannot be called presuppositions in Strawson's or Lakoff's senses of course, because for Russell, if there is no King of France,then the statement that the present King of France is bald is FALSE,not just without truth value.And,as is well known of course, Strawson's notion of presupposition was specifically directed against that doctrine of Russell's. But if Strawson has some other clear,non-Russellian,notation for incorporating presuppositions into logical forms he does not really set it out.What he gives us is a notation for presuppositions due to Horn[see 25] which claims to be a formalization of a notion of Austin's,and is in any case different from the Strawsonian def tion that Lakoff sometimes seems to embrace [i.e.,ibid.,p.131].

On Horn's view presupposition and entailment are to be distinguished as follows:

If $(S \rightarrow S')$ and $(\neg S \rightarrow S')$ then S presupposes S',

If $(S \rightarrow S')$ and $(\neg S' \rightarrow \neg S)$ then S entails S',

This is different from Strawson's definition ----in that it is not a definition of presupposition in terms of entailment,and it does not make use of the notion of being truth valued-----but like it ,it is inconsistent with the Theory of Descriptions[see 25]. However,its weak point is the unexplained "$\rightarrow$" ,for what is this to be?It cannot be even as strong as material implication,for if S' is false we cannot infer $\neg S$ (as we can with $S \supset S'$) if we are to preserve a notion

of presupposition different from that of entailment on the basis of those two definitions. For if ¬S' allows us to infer ¬S then the two notions, by the definition of entailment given above, cannot be distinct. I would then argue that, if weaker than material implication, Lakoff's and Horn's "→" can only be our old inductive inference again, with all the tro ɔs for a derivational linguistics that I have mentioned already.

As a general thesis about the translation of sentences into logical form Lakoff's claim about presupposition, quoted above, is surely unacceptable. For example, there is the problem of the recession of presuppositions in quite straightforward sentences: should every sentence about a physical object, such as "The boy threw the stone" have a presupposition "Something exists" embedded in its logical form? (And the prefix ∃x does not quite do that). I do not see how Lakoff can avoid doing this without resort to an arbitrary cut-off of presuppositional level.

But of course there is no need for any such nonsense, for all that Lakoff describes as presuppositions can be handled perfectly well by inductive inferences without any embeddings in LF's, and he admits as much in the footnote I quoted. The only trouble from his point of view is that the handling must be as part of an artificial intelligence system.

Note that I am not saying for a moment that I am shedding any light on these difficult notions, such as presupposition and entailment, but only pointing out that they are difficult and unclear, have vexed logicians and philosophers, and are not nice clean tools that Lakoff, or any other linguist, can just pick up and get to work with. They need a lot of conceptual cleaning up themselves, and Lakoff shows no sign of being prepared to do that. Another term in this category is the central one of logical form. Lakoff uses the term freely all the way up to [ibid., p. 53] before he admits that "it makes sense to speak of logical forms of sentences only with respect to some system of logic".

The logical form (LF) of a sentence is the form it requires to take part in deductive relations. Some logicians would also hold that the LF is in addition the real meaning, or structure, of a sentence. This one could call the "backbone" view of LF. Lakoff is tempted by both these points of view and, since he is a linguist not a logician, this leaves an important ambiguity in what GS means (See Section III below).

The very first example in Lakoff's long paper fails to notice the fundamental relation of LF to deduction. He writes [ibid., p. 1]:

"(1) The members of the royal family are visiting dignitaries.

(2) Visiting dignitaries can be boring.

. . . . corresponding to each of these grammatical analyses (of 1) we find a pattern of deduction".

But that is just not so. We cannot deduce two conclusions from the above sentences, depending on the preferred grammatical analysis of (1), because we cannot deduce anything until the sentences are in some recognizable LF. And that LF does not have to be symbolic; indeed, one could say that the heart of Aristotelean logic consists in trying to squeeze sentences into Aristotle's rebarbative predicate form, all of which was wholly expressed in the natural language of "terms". Only when something equivalent to that has been done can we begin to talk of deduction. But, of course, in doing it the grammatical ambiguity Lakoff's example requires will have disappeared.

Lakoff warns us that LF only makes sense with respect to a particular logic, but then, without warning, settles for a modified Predicate Calculus for expressing sentence structures : one with predicates and arguments that can themselves be predicates. He then uses this format, when it is appropriate to the system he is discussing, but expressed in tree form. There is no reason why a linguist should not express predicate formulas by trees rather than strings if he wishes: all that makes this odd is that Lakoff also makes use at other points [ibid. , pp. 14, 15 for example] of standard phrase structure trees, and writes of them as LF's, which leaves one in considerable doubt as to what Lakoff thinks a logical form is. He has not pinned down the Predicate Calculus format he seems to have adopted closely enough for one to know whether or not it is capable of expressing the linguistic variety that he, especially, would want to get into it.

In the matter of quantifiers, too, one's faith in the commonsensicality of Lakoff's natural logic is not increased by his initial battery of examples which starts with:[ibid. , p. 12]

"(1) The archaeologist discovered nine tablets".

This, Lakoff claims, is ambiguous because " It can mean either that the archaeologist discovered a group of nine tablets or that the number of tablets he discovered altogether totalled nine, though they may not have been in a group".

But, one is tempted to reply, it might just as usefully be argued that the sentence is ambiguous depending on whether or not the archaeologist is an officially certificated one!What Lakoff has done here is to take a distinction fundamental in mathematics and logic, that between a set and its members, and to claim that it has empirical significance in a natural language. But that is an extraordinary procedure , and doubly so for an advocate of a NATURAL logic, one free from the preoccupations of mathematically oriented logicians----for what normal speaker could seriously consider the quoted sentence ambiguous?

It is important to be clear here that Lakoff's point is quite different from a recurring one of Quine's ;that we understand certain grammatical structures and distinctions better by seeing them illuminated by logical description [see for example 23, p. 44] . Quine assumes, in order to make this point, pre-existing grammatical distinctions to be illuminated, and does not envisage a SEARCH for mathematical distinctions in the operation of natural language.

But some advances are present in Lakoff's treatment of quantifiers: it was becoming well known that standard transformational theory could not handle the notions of variable and quantifier, in particular, so as to give two readings to such old logical chestnuts as "Everybody loves someone". Most people can be got to see that this sentence can be interpreted to mean two quite different things, that could be associated with the Predicate Calculus formulas $(\exists x)(y)(Lyx)$ and $(x)(\exists y)(Lxy)$ respectively, even though they would not normally express the two messages involved by means of that one sentence if they wanted to be understood. Now Lakoff discusses this particular example, and displays two (conventional phrase-structure) trees for the sentence, but (and this seems to me the vital point) he gives no bottom-level rules that show how one could take the sentence and derive two readings for it. That is the serious test in this case, and the trick we all want to see done. While Lakoff is discovering logic Moravcsik and Gabbay have provided a strong set-theoretic logic with grammar rules[12&21]that does do just that. Sandewall [24] and Simmons[26] have also provided modified Predicate Calculus notations that deal with such examples in a procedurally determinate manner.

Lakoff's failure to provide any sort of system of rules, however miniaturised in scope, is an important one, as I argued earlier. For it leaves an important doubt as to just what a natural logic, or indeed a generative semantics, is intended to accomplish with regard to some body of sentences in a natural language. And, it is not possible for Lakoff to take refuge here in some competence-performance distinction and to say that of course he is not attempting to model a speaker's performance etc. etc. , precisely because that is not what he is being accused of. The request for determinateness and precision is in no way to be confused with a demand for psychological imitation.

It is perfectly true, of course, that logicians import structures into their work and inform their readers that those structures represent certain natural language sentences, without ever giving a hint of a determinate translation procedure that would take us from the sentences to the structures. But I do not think that Lakoff could take shelter with the logicians here, for there is an important difference between the logicians' enterprise and his own. The logician is concerned above all with the formal relations between the structures he derives ;the exact relation between the structures and the natural language they "hook onto" is secondary even though vitally important. But Lakoff, on the other hand, describes his task

in terms of the production or generation of sentences along with their structures. So, for him, the missing determinateness is, and must be, central.

I may well not have done justice to the wealth of Lakoff's examples in this paper. But it should be said that there are certain quite gratuitous difficulties in the way of doing so. In particular Lakoff's curious treatment of the status of linguistic examples. It has been remarked in detail elsewhere by Lindsay[38] how bizarre the "* notation" is when used to mark sentences considered linguistically illegitimate. Let me add two glosses to those criticisms to illustrate the additional difficulties present in the work of Lakoff's under discussion.

In the present paper Lakoff also uses the "*" to mark LOGICAL items that he considers false, or logically false. For example, the asterisk is attached to $PE_R MI_T(X, Y, S1) \supset {}_R EQUI_R E(X, Y, S1)$ [ibid., p. 75] to indicate an inference that does not in general lead to true conclusions. But the statement can hardly be called ungrammatical in any sense, unless that hardworked word is to carry an even heavier load!

Lakoff also displays an opposite technique in this paper: opposite that is to the arbitrary exclusion of examples. For it amounts to the arbitrary acceptance of examples. Two quotations from footnotes should give the flavor of the method:

"The assignment of asterisks in the following examples corresponds to the author's speech. Readers whose idiolects disagree with these examples can easily construct similar examples in their own speech."

"Sentences like (1) are not normal in standard English, and are restricted to certain dialects. These are most common in urban centers in which there are, or were, a large number of Yiddish speakers. Again, the facts given here are from the author's native dialect and the argument is based on the existence of a dialect in which such facts hold" [ibid., pp. 130-131]

It's hard to know what to say to this, except that he must surely see that if his examples depend on the particular dialect and cannot be reproduced in standard English, then it throws considerable doubt on what he is arguing for. I myself cannot "easily" reproduce his examples in my dialect and, moreover, see no reason why I should, since he is writing the paper and it is his job to convince me. On the other hand, if the examples can be reproduced easily in standard English, then it is sheer perversity, in a paper apparently IN standard English, for the author not to do so. I referred to his method as the arbitrary acceptance of examples (analogous to the arbitrary exclusion with "*") because, if one does not understand the author's dialect, one feels that there are no holds barred and that an author could make any arbitrary point about English in this way.

In the end I feel a sense of disappointment about Lakoff's discussion of natural logic, partly because I do not get the feeling that behind the ragbag of bits and pieces there is a real hard system of analysis coming into being, and partly because there is so much of real (non-formal-logic) argumentation in natural language that Lakoff does not even consider. He considers only the structure of simple sentences and simple inferences, whereas the real structure of informal argument seems to me to take over a much larger scale. And here I am thinking of the considerable work of Austin [2] and Bambrough [3] to pin down the notion of informal argument; Hesse's [13] efforts to give a formal definition of analogical argument;Passmore's [22] analyses of basic forms of argument in philosophy and ordinary discourse that seem to elude conventional formal description----such as the Reductio ad Absurdum;Colby's [11] computer-construction of discourses and inferences appropriate to certain forms of mental disorder;Anderson and Belnap's[1] efforts to formalize enthymatic, or incomplete, arguments;as well as my own efforts [30] to trace ,formally,the sense shifts in argument, based on some important ideas of Bosanquet [5]. All these seem to me to deserve some consideration in the context of a real natural logic, to supplement merely wandering through what the conventional formal logicians have to offer as Lakoff has done.

III GENERATIVE SEMANTICS

Lakoff's thesis of GS can be discussed separately from natural logic because natural logic is clearly about the explicit inferences people make, for better or worse, when they reason. GS, on the other hand, is about the more standard linguistic task of pinning down the production of well formed sentences , or, if one prefers to speak in a psychological mode, about implicit inferences made in the generation process for sentences.

GS can be discussed briefly here because heavyweight analysis would be out of place until Lakoff says more clearly what he means by it. As I quoted earlier, he writes that "the rules relating the logical form to the surface form are exactly the rules of grammar". I think we can take the "exactly" as having only rhetorical force here, since any such perfect coincidence would almost certainly have been noticed before the year of our Lord 1970.

It may well be the case that certain of the rules to which Lakoff has drawn attention in this paper do have a part to play in any general language-to-logic translation AND in any reasonably general grammar, of whatever sort. But that is a far cry, of course, from the burden of proof required by the "exactly" in the last quotation. If it is replied that the quotation expresses only a conjecture,then it seems clearly a false one, since it is not hard to find ----for two such prima facie different tasks as grammatical production and translation to logic----- examples of rules that will certainly function in one enterprise and equally certainly not in the other. I do not believe, for example, that the grammaticality, in any sense of that word, of

sentences containing "possibly" can require a rule relating that word to some primitive symbol expressing the concept of certainty. Yet translation of such sentences into modal logic will require some such rule ----or the complement of it, where "certain" replaces "possible" mutatis mutandis. Surely Lakoff's conjecture-assertion about rule identity excludes this possibility?

Again, Lakoff's defense of GS at this point involves some very odd forms of argument indeed. The following seems to be essential to his justification;[ibid. , p. 11]

"It should be noted that the above conclusions [that is, GS] depend upon a form of argumentation upon which just about all of the linguistics of the past decade and a half depends, namely, that if a given theory necessarily requires that the same rule be stated twice then that theory is wrong. Not just inelegant, but empirically incorrect. "

Well, if that is true, then perhaps so much the worse for recent linguistics. For that form of argument, if correct, would put linguistics in a unique theoretical position among the sciences and humane disciplines. There are very complex discussions in the contemporary philosophy of science about what exactly it means to say that one theory is more economical than another, in terms of excluding more alternative possibilities and so being more testable in some defined sense. Paradigms of such argument concern, for example, whether the hypothesis that the planets have circular orbits is more or less economical than the alternative in terms of elliptical orbits. But no one, to my knowledge, has suggested the employment of the principle referred to by Lakoff : that a less economic theory, in any sense, is not just less economic (with respect to the same data) but is ergo EMPIRICALLY WRONG!

With GS, as with all such theses, there ae two ways of looking at them: one is to take the words as meaning what they appear to mean; the other is to assume that they mean something quite different. The first approach gives us what I shall call the TRANSLATION view or the CONSEQUENCE view depending on how we take the word "relate" in that last quotation. The second approach would give what I could call the RENAMING view. By that I mean that when Lakoff speaks of logical form he doesn't mean that in any standard sense, but as some linguistic structure, either familiar or of his own devising. In either case, on the renaming view, GS would not really be ABOUT logic at all, and disputes about it would be wholly an internal matter for linguistics. When Chomsky [10] writes of GS as "notational variant" of his own work he is taking the renaming view.

The consequence view is the most obvious possibility, namely that the "relates" is by inference, valid or otherwise, and that the well formedness of sentences is settled by whether or not they can be inferred from logical forms. My points in the introduction about Barhillel assumed that this was Lakoff's view. Much of the evidence

for this assumption is circumstantial because Lakoff rarely actually discusses GS in general terms. But it is reinforced by his introduction of rules of inference with "It is clear that there is more to representing meanings than simply providing logical forms of sentences" [ibid, , p. 75]. That quotation seems to me to rule out the translation view: that logical forms are the meaning , or "backbone", of sentences and can be related to them by mere rules of translation. The translation view also becomes less plausible when one remembers how much of the paper is about inference : if GS were really about translation into logical form then inference would have no place at all in a discussion of natural logic. So then, the consequence view must be Lakoff's view if he has a firm view. Two clear and simple considerations tell against it:

(1) There is just no clear notion available of inference going from logical forms to sentences. Rules that cross the logical form-sentence boundary are rules of translation.

(2) There is the problem of "reverse direction": how could we analyse sentences with reverse inference rules to produce logical forms? Reversing inference rules is to produce false,hood, as in "if this is not colored then it is not red." What possible interpretation could we attach to such a procedure in the context of GS?

In addition there is the general implausibility of believing that the form or meaning of what we say is determined in any way by operations involving the notion of truth. This is a separate and detailed philosophical matter, of course, one inappropriate for discussion here, but which should,I believe,by now be considered settled in favor of the common sense position.The questions involved have been much discussed,but Strawson's[28]is an excellent recent restatement of that position.

The possible analyses of GS I have offered, and the knockdown arguments I have produced against it when so interpreted, may be criticised as cavalier and inadequate. That is true, I am sure, but I do not see how justice can be done until Lakoff produces considerable clarification of GS, at the top level, if I may use that phrase. It should also be added, in fairness, that I have not mentioned the many fundamental points, such as the primacy of semantics and the importance of what is now called "lexical decomposition", on which I, like many unreconstructed pre-Chomskyans, warmly applaud Lakoff's recent positions.

REFERENCES

1)Anderson ,A.R. & Belnap,N.D.,
Enthymemes,Journal of Philosophy,1961.
2)Austin,J.L., How to do things with words.
Oxford,1962.
3)Bambrough,J.R., Principia Metaphysica,Philosophy,1964.
4)Bar-Hillel,Y., Logical Syntax and Semantics,Language,1954.
5)Bosanquet,R., Remarks on Spinoza's Ethics,Mind,1945.
6)Carnap,R., The Logical Syntax of Language,London,1937.
7)Chomsky,N., Logical Syntax and Semantics,Language,1955.
8)Chomsky,N., Syntactic Structures,Janua Linguarum,1957.
9)Chomsky,N., Aspects of the Theory of Syntax,
Cambridge,Mass.,1965.
10)Chomsky,N., Deep Structure,Surface Structure ,and Semantic
Interpretation,
in Steinberg & Jakobovits(eds) Semantics,
Cambridge,1971 .
11)Colby et al. Artificial Paranoia,Artificial Intelligence,1971.
12)Gabbay,D., Representation of the Montague semantics as a form of
the Suppes semantics,in
13)Hesse,M., On defining analogy,Proc.Aristotelean Soc.,1959-60.
14)Jaskowski, On the rules of supposition in formal logic,
in McCall(ed),Polish Logic,Oxford,1967.
15)Kneale,W.& M., The Development of Logic,Oxford,1962.
16)Lakoff,G, Linguistics and Natural Logic,
Studies in Generative Semantics #1,
Univ.of Ann Arbor,Mich.,1970.
All references here are to this version which is still
the most widely available,I think.It also appears in:
Synthese,1971, and in
Hintikka,Moravcsik and Suppes (eds)
Approaches to natural language,forthcoming.
Davidson & Harman(eds) Semantics of Natural Language
(in press).
17)Lakoff,G., Generative Semantics,in Steinberg & Jakobovits(see 10).
18)Lindsay,R.,& Kibbens,M.,
What are the facts of Language?
mimeo,Mental Health Research Institute,Univ.
of Ann Arbor ,Mich.,1972.
19)Matthews,G.H., Analysis by Synthesis of Natural Languages,
Proc.1961 Internat.Conf.On Machine Translation,
H.M.S.O.,London,1961.
20)Moore,G.E., Philosophical Studies,London,1922.
21)Moravcsik,J., & Gabbay,D.,
Negation and Ambiguity,mimeo,Stanford Philosophy
Dept.,1971.
22)Passmore,J., Philosphical Reasoning,London,1961.
23)Quine,W.V.O., The Ways of Paradox,New York,1966.
24)Sandewall,E., The representation of conceptual information
in the Predicate Calculus,mimeo,Uppsala Univ.,1972.

25)Schnitzer,M., Presupposition,Entailment and Russell's Theory of Descriptions,Foundations of Language,1971.

26)Simmons,R.,& Bruce B., Some relations between Predicate Calculus and semantic net representations of discourse,Proc. 2nd.Internat.Conference on Artificial Intelligence, London,1971.

27)Strawson,P.F., An Introduction to Logical Theory,London,1952.

28)Strawson,P.F., Meaning and Truth,Oxford,1970.

29)Wilks,Y., One small head---on models in Linguistics, Proc.Internat.Conf.on Computational Linguistics, Debrecen,Hungary,1971.

30)Wilks,Y., The Computer Analysis of Philosophical Arguments, (in press)

31)Wilks,Y., Decidability and natural language, Mind,1971.