

(X)¹

FRANCIS JEFFRY PELLETIER*

Katz's new clothes Katz's new view about language and linguistics comes in two parts: platonism and nonreductionism. Those of us who have been skeptical about the empirical basis for claims that have arisen from within linguistics will welcome Katz's defection to our side in respect of platonism, but we will be dismayed by his refusal to adopt the rest of our methodology concerning reductionism, and his continuing espousal of (what he has called) "Semantic Theory" (henceforth ST). It is these overall positions I wish to discuss; but along the way I shall take the liberty of making disparaging remarks about various of the specific proposals Katz puts forward within ST, with an eye to showing that the program can't be done in the manner he favors.

Katz's recent book [11] outlines what he takes platonism in linguistics to be, and vigorously argues against (what he calls) conceptualism in linguistics. His article in the present volume [12] follows this up with his reasons for rejecting (what he calls) reductionism in semantics, and gives his vision of the enterprise of semantic theory. "Conceptualism" as used in [11] is actually a cover term for a variety of theories which in one way or another have the common feature that a language is viewed as a social phenomenon, or as a psychological phenomenon, or as a cultural phenomenon, or as a biological phenomenon, or in some way depends upon or requires users of the language for its existence. In this way, conceptualism is contrasted with two other views: those that take a language to be the set of utterances which have heretofore been produced ([11] "nominalism") and those that take a language to be a timeless, unchangeable objective structure ("platonism"). Katz identifies nominalism with "the discredited doctrine of structuralist linguistics that

*Thanks to Bernard Linsky and John Heintz for discussion. Thanks also to the editors of *NDJFL* for their patience.

grammars are tabulations of the noises produced in speech" ([11], p. 2), and takes conceptualism to encompass such disparate theorists as Quine, Searle, Grice, and Chomsky (against whom much of [11] is directed).

This marks a radical departure from his earlier works, which he would now call conceptualist. The turning point seems to be his belief that a conceptualist view cannot adequately characterize necessary truth ([11], pp. 6ff), and that only a platonistic theory can. The general line of argument here is that a conceptualist can at best offer an account of a statement's being necessary relative to human psychological laws, but this falls short of absolute necessity (which is not an account of any creature's cognitive capacities, but rather is an account of a certain feature of reality).

It is, of course, undeniable that people *use* language, that one and the same sentence might be used differently on different occasions (or by different speakers), that the language used by English speakers has changed, and that the only way to investigate a language is to consult the knowledge or products of speakers of the language. But as Katz is at pains to emphasize, all this is compatible with the platonic view wherein

the language is a timeless, unchangeable, objective structure; knowledge of a language is temporal, subject to change, and subjective. Someone becomes a speaker of a language by virtue of acquiring a set of tacit beliefs or principles that stand in the relation 'knowledge of' to a member of a set of such linguistic structures Language change [takes] place when speakers within a certain line of linguistic development come to have a system of grammatical knowledge . . . of two different sets of abstract objects There is an infinite range of such classes [of abstract sets of sentences constituting a language] including English, French, Sanskrit . . . and infinitely many other languages, living, dead, unborn, conceivable and inconceivable The study of languages is the study of these linguistic structures. As such, this study is distinguishable from the study of human (or other) knowledge of them, its acquisition, use, or change. The former study is linguistics, the latter psychology. ([11], p. 9)

Many philosophers engaged in formal semantics subscribe to this view, even if only unconsciously. Or at least, it is difficult to see how one could view their enterprise if it were taken as a contribution in the conceptualist framework. Since the conceptualist framework is avowedly tied to the dirty empirical world of psychology or sociology, a formal semanticist who advocated that metatheoretical position should find him- or herself embarrassed by the lack of regard paid to investigating what semantic intuitions people actually have and what sociological functions semantic phenomena actually fulfill. Instead, such theorists proceed as if they had a direct (but not to be confused with infallible) access to the semantic facts about some abstract object: the language in question.

When linguistic theories that were inspired by formal semantics (e.g., Montague grammars) started being taken seriously by linguists, there was heard (in some quarters) the remark that at last linguists would stop trying to endow their (syntactic) descriptions with "psychological reality", for if anything was clear about Montague grammar it was that speakers have not internalized the sort of representations called for in this view of how to describe a language. Alas, it didn't take long But perhaps Katz can succeed in furthering a

conception of doing linguistics and formal semantics without implicitly believing you're doing psychology.

Now, just as there are many arguments that might be brought to bear against platonism in mathematics, so too one can adapt them to oppose Katzean linguistic platonism. This is not the place to discuss such issues; I have instead prefaced my remarks on [12] with this summary of [11] so that one does not mistakenly think that a certain sort of argument popular in the literature can be effectively wielded against the new Katz. In broad outline, this sort of argument goes: a semantic theory has to account for how people can understand/use/evaluate/etc., sentences in a variety of circumstances. ST does not do this and so it is wrong. Now, taking this conceptualist view of semantics and arguing that ST doesn't meet the adequacy criterion maybe is correct against Katz's earlier views of ST, but it is no longer so simple to refute him. For, in Katz's earlier days, when he was a conceptualist, the fact that ST in reality had nothing to say about how language was used or understood amounted to showing an internal incoherency in the Katzean view of language. But it no longer does so. What would be needed first would be an argument against the *overall* viewpoint of platonism in linguistics. And to make this overall argument stick, one must go back to [11] and face the arguments given there in support of linguistic platonism and in support of the view that the relation between linguistic semantics on the one hand and the use/understanding/evaluation of the language on the other hand is to be accounted for by the separate discipline of "psychology".

According to [12] "meaning is meaning and not something else"—a statement with which we can all agree. And we might further agree that semantics is (in some way to be specified) the study of meaning. And yet we might disagree with the Katzean position that the semantics of a language is to concern itself only with certain features of the abstract system, the language. We might want to insist, for instance, that a semantic theory make claims about how, or explain why, the language can be used in certain ways. So for example, Katz complains that Searle's "attempt to formulate a theory of meaning as use" must fail because it confounds "aspects of the context of use that a speaker normally or usually takes into consideration when using language" with "aspects of English verbs and hence aspects of the language". This type of criticism hits the mark only if one is antecedently convinced that it is not the role of semantic theory to account for such "aspects of the context of use". Otherwise, this is merely the criticism "your theory is wrong because it doesn't concern itself with exactly the range of phenomena mine does". And as far as I can see, Katz nowhere has argued for this view—his remarks simply *assume* it.² Similar remarks could of course be made about the reverse criticism of Katz's theory not making claims about how speakers use the semantics of a language. I do not want to prejudge the issue; rather, I want to first ask whether the Katzean goal has been achieved by Katz before asking whether it's a reasonable or useful goal to strive for.

Modesty in semantics It is not widely known that, besides giving us the semantic conception of truth, Tarski also gave us the modest conception of semantics.

It is perhaps worth while saying that semantics as it is conceived in this paper (and in former papers of the author) is a sober and modest discipline which has no pretensions of being a universal patent-medicine for all the ills and diseases of mankind, whether imaginary or real. You will not find in semantics any remedy for decayed teeth or illusions of grandeur or class conflicts. Nor is semantics a device for establishing that everyone except the speaker and his friends is speaking nonsense. ([19], p. 17)

A modest conception of semantics is one which carves out a subportion of what had been hitherto believed to be semantics, and claims that this subportion will be what shall henceforth be treated by the theories under consideration. The remainder, "while it may be important", is no longer to be considered "real semantics", but to be accounted for by some other theory (usually called "pragmatics").

Semanticists since Tarski have, on the whole,³ become more and more modest. For example, in a textbook exposition of Davidsonian semantic theory [18], it is said that besides the well-known Davidsonian position that *means that* should be replaced by *is true if and only if*, none of the following are phenomena to be accounted for in a semantic theory: lexical ambiguity, the difference between '*a = a*' and '*a = b*', the illocutionary force of an utterance, whether a given name is proper, the interpretation of indexicals, lexical synonymy and entailment, how we can know whether a semantic relation is satisfied ("verification conditions"). As one recent review of this work puts it [16]: "A theory of truth can indeed pretend to be a theory of meaning if one only wants what Platts proudly calls a 'modest' theory of meaning."

Katz's view in [12] is even more modest than Platts's view. Platts at least assigns to his semantic theory the task of pairing "the indicative elements *p* and *q* which are gained from applying a theory of force to a native utterance" with some metalanguage sentence which "enables us to say that the speaker is asserting that *p*, or asking whether *q*, and so on". Katz's conception of language as "a mathematical object" and his account of semantics as a description of a portion of this object, effectively cut him off from any of this kind of "pairing". Again, the modesty of the theory is to invoke some other theory—for Katz, psychology—to explain what might otherwise be thought of as semantic phenomena.

So what phenomena does Katz's theory explain? In a word: none. It's not designed to explain anything, but rather to *redescribe* a certain range of semantic phenomena. That is to say, it is assumed that there is a certain class of phenomena which are extant in one of the abstract objects, English say. We know that this is so because, says Katz ([11], Ch. 6), we have a faculty for intuiting this. The point of a semantic theory according to Katz, indeed the whole point, is to represent these phenomena in some systematic manner.

This way of putting Katzean semantics may appear to go against various of the things said in both [12] and [11], for Katz often makes remarks about the "predictions" made by his theory and the "explanatory power" of his theory, which together might be thought of as doing more than merely re-describing the phenomena in a systematic manner. But this last is not true, nor do I think Katz views his theory as being anything other than modest to the

point of denying connection with any use of language. The "predictions" made by the theory are about whether a person will agree that (say) a certain sentence is ambiguous when it has been described in markerese as ambiguous. And the "explanatory power" comes in when the same representation of a word in one sentence can be used in another sentence. I would grant that there is a sense in which these are "predictions" and are illustrative of some "explanatory power"—viz., the same sense in which any philosophical theory (whether it be in semantics, ethics, or metaphysics) makes "predictions" or has "explanatory power". The overriding question in all of these avowedly philosophical areas is: does the representation in the theory agree with my intuitions in these matters? What I, and I suspect most philosophers and scientists, would be unhappy with is any suggestion that we have done anything more than systematize the data to be explained (explained in some more ultimate sense). And I think Katz agrees with this.

Does ST work? The semantic phenomena Katz thinks his theory can adequately redescribe include lexical ambiguity, the lexical relation of superordination, analyticity, sentential ambiguity, sentential anomaly, and "analytic entailment". His method is well-known by now from his numerous previous works (see [9] especially), the only difference now is that he interprets ST differently as an account of the abstract object instead of as an account of a speaker's competence. It involves a *dictionary* wherein each distinct sense of a morpheme is associated with a group of "semantic markers". These latter are "uninterpreted" and not to be confused with the identically spelled English words. They represent the "conceptual structure" of the sense (by means of a tree or bracketing); and they are further subcategorized with co-occurrence restrictions so that (for example) sentences like 'Mice chase cats' and 'Cats chase mice' can be distinguished by means of "projection rules" which substitute other semantic markers into the tree to thereby give a semantic representation of an entire sentence.⁴ There are also "selection restrictions" which have the effect of blocking the substitution of semantically anomalous morphemes into certain trees.

The fact that Katz adopts in its entirety the theory of [9] means that all the old formal criticisms of that theory still hold. For example, Linsky ([15], p. 476) remarks that Katz might be thought to be open to the charge of "Quine's circle" in defining 'analyticity' in terms of the (undefined) antonymy relation among semantic markers. (This relation might be said to be in just as much need of explanation as analyticity.) Linsky in fact thinks that the construction of a list of antonymous markers lets Katz off the hook, because such a construction "is not a question which . . . needs to be answered within semantic theory". Once again we see the modesty of ST at work: ST succeeds only because some other theory does all the work. In a similar vein, Davis ([6], p. 114) remarks that Katz's definitions of semantic terms either "belong to a circular set of definitions or contain an undefined term in their definiens" depending upon whether one thinks that the notion of an antonymous n -tuple of semantic markers is part of the semantic theory or not.

A second class of criticisms has to do with the use of selection restrictions. A fundamental principle in ST is that the number of morphemes and number

of readings in a language is finite. Yet there seems to be a general way to construct new ones and extend the internal structure of readings indefinitely. For example, both the subject and the object of *chased* must be marked as “inherently spatio-temporal particulars”, says Katz in [12] (a change from [9], p. 106 where they had to be (animal)); yet as Harrison [8] has pointed out, “The thought of bed chased all concern with linguistic theory from my mind” is clearly grammatical. There seems to be no way of changing the selection restriction without also letting in Katz’s anomalous “Truth chased falsehood”. So we must posit another sense of *chase* which would be quite different from that of “The police chased the demonstrators”. Harrison [8], citing Weinreich [21], says that ST has no theoretical mechanism to prevent an endless proliferation of readings and markers, which proliferation is clearly not present in reality.

A third class of criticisms has to do with ST’s choice of syntactic theory. Crucially, ST makes use of some syntactic theory in order to state the selection restrictions and co-occurrence restrictions. Katz is *not* in a position to say that he will adopt whatever syntactic theory turns out right in the end, for his ST is strictly tied to a syntactic parsing which will yield the sort of semantic trees he has presented us with. One wonders what level of syntactic description ST operates on: whether it is trace-theoretic logical form of Chomsky [2], more recent Chomskian levels as thematic role structure [3] or lexical structure [4], functional-structure of Bresnan [1], surface structure in the sense of Gazdar [7], or perhaps a more traditional “deep structure” in the sense used in the original formulations of ST. This question cannot be bypassed by Katz because the precise syntactic forms allowed has to be encoded at the beginning when stating the semantic markers for lexical items, or else there is no way to evaluate the claims about ST’s predictions. Much of the motivation for recent syntactic proposals has evolved from studies of “unbounded dependencies”, where an element has been “dislocated” from its “normal” position. One wonders what mechanism Katz would find acceptable in describing the semantic marker for sentences like “Sandy used to and Kim still does do dope” or “The girl wondered who John believed that Mary claimed the baby saw”.

Another class of criticisms of ST concerns itself with the expressive power of ST. In ST, one can “read off” all semantic relations merely by looking at the orthography of the expressions. Thus, analyticity is defined in terms of “the representation of the predicate being included in the representation of the subject”, and semantic equivalence amounts to “having identical representations”. By this is *not* meant any transformation (logical or otherwise) upon the representations, but rather that the physical symbols themselves are identical. *Every* semantic relation in ST is defined in this sort of way. Thomason [20] noted that this concept of semantic equivalence is then a recursive property, and remarked that *logical* equivalence (as traditionally viewed in predicate logic) is not recursive. He concluded that “unless natural language is not as powerful as predicate logic (which seems very unlikely), ST cannot adequately represent its semantics”. It seems to me that [12] and [10] present the claim that the semantic relations Katz wishes to capture are not those of predicate logic,⁵ and thus this type of criticism is not quite to the point. Katz is free to define any relations he thinks are important. And although it might have been

wiser to use different names for his relations so they aren't confused with the better-entrenched relations, in the final analysis all that matters is whether his relations perform any useful service in understanding the semantics of language. So to avoid confusion with pre-existing notions, I shall use such locutions as K-entails, K-synonymous, etc., in discussing his views. And the question becomes whether K-entails, for instance, represents any pretheoretic notion we have. (After all, as Katz is at pains to emphasize especially in [12], it is these commonsense notions we wish to account for.) It seems to me, from the statements we have of them now,⁶ that they don't.

According to [12], K-analyticity occurs when "the concept expressing the attribution of a sentence . . . [and] the components that pick out the object(s) the sentence is about" stand in the relation where the former is a subtree of the latter (CSS). Thus in the sentence "Women are persons", the "component that picks out the object(s) the sentence is about" is *women*,⁷ and a subpart of the representation for *women* is the representation for *person*. In [10] (p. 391), sentence S_1 K-entails S_2 just in case the semantic trees for S_2 are all subtrees of some tree for S_1 (and they are "about the same things" in the sense that the variables in the trees are uniformly replaced). One final formal remark is that negation is understood as invoking the previously mentioned antonymy classes: if C is a semantic marker, then $\text{NEG} + C$ is understood as the disjunction over the class of those markers antonymous with C .⁸

It has been noted before (e.g., [15]) that these accounts of K-relations do not seem to mesh well with one another nor with any of the antecedently understood notions of similarly named relations. For example, 'A spinster is a woman' gets marked as K-analytic since the marker for *spinster* includes that of *is a woman*. Now every marker representing *person* is a subtree of one representing *spinster* (it must be, else 'A spinster is a person' wouldn't be marked as K-analytic). So therefore the semantic tree representing 'A person is a woman' is a subtree of 'A spinster is a woman'; hence the K-analytic 'A spinster is a woman' K-entails the non-K-analytic 'A person is a woman'.⁹ Linsky, bringing the account of negation into the picture, points out that 'My father is not a queen' is K-analytic (on one reading) and that it K-entails 'Someone is not a queen' which is non-K-analytic.

Thus the notions of K-analytic, K-entails, etc., diverge from the more normal ones. It is for this reason that the Thomason criticism doesn't hold directly: perhaps the notion of K-equivalent can be defined in some recursive manner. Of course, Katz still owes us the explanation of why this new notion is of any interest. I doubt that the alleged fact that it can represent *some* of our pretheoretic views about "the subject being included in the predicate" will win many people over.

These examples show that K-analyticity is distinct from logical truth, and K-entailment from logical implication—we needn't wait for the (in preparation) work Katz mentions. But the question of legitimacy of these new notions is more pressing. According to Katz, showing legitimacy requires: (a) showing that K-analyticity and K-entailment can be determined without knowledge of states of affairs of the world, and (b) showing that the K-analyticity of a sentence is sufficient for the statement it makes to be true in all worlds, and K-entailment to be a sufficient condition for the entailed sentence to be true

whenever the entailing one is. The above examples show that K-analyticity and K-entailment are not even K-legitimate, much less helpful and enlightening.

But perhaps these are mere matters of detail? Perhaps the definitions of K-analyticity and K-entailment can be fixed so as to become K-legitimate? The answer is no: given what the common sense which Katz is so concerned to preserve tells us about "true in all worlds where the object(s) it is about exists", it is impossible for there to be some property *X* which can be defined on orthographic shape of semantic markers that will turn out to be K-legitimate. For the notion defined by K-legitimacy is precisely the notion of logical truth as traditionally understood, and cannot be defined by any recursive predicate. So Katz needs to find another concept of legitimacy if he wants to continue with his program of ST.

Modesty and reductionism I mentioned before that Platts's theory was modest in not trying to account for the analyticity of 'Bachelors are men', although it did give an account of the analyticity of sentences like 'If *X* and *Y*, then *X*'. (That is, where the analyticity depended upon the "logical vocabulary".) It is well-known that Katz rejects the logical/nonlogical vocabulary distinction. One would therefore expect him to give an account of analyticity that would treat them on a par; surprisingly, Katz chooses (in [12] anyway) to reject the apparent requirement that he account for the traditional logical particles. Katz's theory is modest in the sense that it does not recognize as K-analytic such sentences as 'If John is a bachelor, then John is a bachelor'. Some other theory is to do that.¹⁰ Again one wonders whether there is any interesting notion being hinted at here; my suspicion is that there is not.

Suppose one had a theory of some phenomenon where the theory invoked some technical concept *X*. Why would one want to "reduce" *X* to something? Two types of reasons come to mind: (Type 1) If the technical notion *X* could be defined as a function of some antecedently understood notions *Y*, *Z* . . . , then one would now claim to have a complete understanding of the former theory; (Type 2) If the technical notion *X* can be used to explain some other technical notion *Y* of another theory, which in turn helps explain the technical notion *Z*, . . . , which can be employed to explain *X*. These two different attitudes toward the point of a "reduction" correspond to two different reactions about explanation: Type 1 corresponds to "I see how it is that *X* is really some sort of abbreviation for features *Y* and *Z*, which abbreviation systematizes certain apparently unrelated phenomena"; Type 2 corresponds to the attitude "I understand a theoretical term when my overall theories are comprehensive enough to encompass a sufficiently large number of phenomena. I understand if the circle is big enough".

When one "reduces" some technical notion (say 'meaning') to other notions (say 'the conditions under which it would be true') and one discovers that the "reduction" gives incorrect results in certain cases, one possible reaction is Katz's: conclude that the "reduction" is completely wrong and must be scrapped. But another, more reasonable, reaction is that the reduction is not completely correct, and one merely needs to make some alterations. After all, the notions of "reduction" mentioned above used the phrases "defined as a function of" and "can be used to explain". Perhaps all that would be called for

is a fancier “function” or a different “use”. For example, to overcome Katz’s objection that the above definition makes all logical truths have the same meaning, we might want to say that in addition each constituent makes its own contribution to the overall meaning of the sentence and that two sentences are synonymous only when all constituents contribute the same in each sentence. These sorts of moves are very common in the literature (for this one, see e.g., [13], pp. 182 ff), and Katz’s argument for ST is not furthered by his pretense that this type of opposition to truth-conditional accounts has not been discussed and that the believers do not at least think that they have an adequate solution. Does anyone really believe, for instance, that philosophers working within the Searle or the Grice traditions have not considered the claim that they are “confounding linguistic meaning with conditions of appropriate use”?

Katz’s superargument, the argument implicit in Katz’s “meaning is meaning and not another thing”, is that *any* attempt to explain or explicate meaning is doomed to failure. Indeed, he says that the chief virtue of his theory is that he does not try any explanation of ‘meaning’. One is reminded here of the discussions of the last 70 years about “the paradox of analysis” and the “naturalistic fallacy”. Given a proposed analysis of p in terms of $f(q, r, \dots)$ one might: (a) point out that they can always ask whether the reduction is correct, thereby “showing” p and $f(q, r, \dots)$ not to *mean* the same thing; (b) point out that if the reduction is correct so that p and $f(q, r, \dots)$ mean the same, then no information has been imparted, hence if it’s an informative reduction then it’s incorrect. If Katz really views his superargument as decisive, he would do well to consider the literature on these old saws.

In any case, Katz’s insistence on nonreductionism and the purely formal nature of semantic markers cuts ST off from performing an explanatory role of any sort. First, we would not be allowed to use any explanation in terms of antecedently understood notions along the lines of Type 1 reasons for reduction. One is not even to understand the semantic markers on analogy with the English words to which they bear an orthographic similarity. (In all fairness, Katz should replace these by some unrelated symbols, say numerals, so that we (and maybe he) aren’t misled into believing that (animate) has something to do with being animate or with ‘animate’). And secondly, there is no provision in ST, when construed as systematizing a part of the structure of some abstract object, for using Type 2 reasons for reduction. As Katz has been at pains to point out in many places, ST is distinct from notions of context, use, reference, psychology, culture, and sociology. There just is no question of using each of these theories as a partial explanation of the others.¹¹

NOTES

1. The name ‘Katz’ obviously lends itself to a variety of cute plays on common sayings such as “The Katz pyjamas”, or “The Katz meow”, or “Katzenjammer”, or “101 uses for dead Katz”, or “This Katz has nine lives”, or “It’s raining Katz and dogs”. The reader’s first problem is to construct a cutesy title for this paper and insert it into the uncategorized semantic marker at the head of this paper.

2. See for example Loar [17]. Katz nowhere addresses the issues brought out in these sorts of works, but simply assumes (because it's consistent?) that it is proper to say that the "user" phenomena are matters for psychology.
3. Possible exceptions: Montaguesque "formal pragmatics" and Barwise and Perry "situation semantics", although these are still very modest compared to what Tarski was opposing.
4. From a suggestion of Weinreich's. See the editorial introduction in [21].
5. See also his earlier statement in ([9], pp. 180 ff).
6. [12], footnote 60, remarks that [9] is "an early statement", that [10] is "in simplified form", and that the full statement is "in preparation". But unless the full statement completely contravenes the published statements, it too will be subject to these criticisms.
7. I shall forego discussing how quantifiers are to work in ST. I think that ST cannot in fact handle them, but will here ignore this and use the examples Katz gives us. Second problem for reader: what is the representation of quantifiers in ST? Third problem for reader: Try, in ST, to account for the validity of such elementary arguments as "Every substance has a solvent, Gold is a substance, so Something is a solvent" or "Every substance has a solvent, All solvents are substances, Gold is a substance, so Gold's solvent has a solvent" or "Every person has a mother, so Every person has a grandmother".
8. Fourth problem for reader: Does the added qualification in [9], pp. 161 ff, for 'antonymous marker' make any difference to the following examples?
9. This is non-K-analytic because the marker for *person* does not uniquely have (female) on each path. It rather has the disjoint (female v male) marker.
10. Problem five for reader: Does it follow from the discussion in [9], p. 190 footnote, that this kind of analyticity is to be accounted for by quantification theory?
11. Sixth problem for reader: Construct a snappy ending in keeping with your answer to problem 1. For example "The Katz pyjamas are like the emperor's new clothes" or "This dead Katz has no uses". If you want to get esoteric, try such things as "You thought this paper would end with my saying that the Katz pyjamas are like the emperor's new clothes, but actually it's more a beetle in a box".

REFERENCES

- [1] Bresnan, J. and R. Kaplan, "Lexical-functional grammar," in *The Mental Representation of Grammatical Relations*, ed., J. Bresnan, MIT Press, Cambridge, Massachusetts, 1980.
- [2] Chomsky, N., "On WH-movement," pp. 71-132 in *Formal Syntax*, eds., P. Culicover, T. Wasow, and A. Akmajian, Academic Press, New York, 1977.
- [3] Chomsky, N., "On binding," *Linguistic Inquiry*, vol. 11 (1980), pp. 1-46.
- [4] Chomsky, N., *Lectures on Binding and Government*, Foris Publishing Company, Dordrecht, Holland, 1981.
- [5] Davidson, D. and G. Harman, *Semantics for Natural Languages*, D. Reidel Publishing Company, Dordrecht, Holland, 1972.

- [6] Davis, S., "Katz on contradiction," *Synthese*, vol. 26 (1973), pp. 113-121.
- [7] Gazdar, G., "Unbounded dependencies and coordinate structure," *Linguistic Inquiry*, vol. 12 (1981), p. 2.
- [8] Harrison, B., "Critical notice of Katz's *Semantic Theory*," *Mind*, vol. 83 (1974), pp. 599-606.
- [9] Katz, J. J., *Semantic Theory*, Harper & Row Publishing Company, New York, 1972.
- [10] Katz, J. J., "The advantage of semantic theory over predicate logic in the representation of logical form in natural language," *The Monist*, vol. 60 (1977), pp. 380-405.
- [11] Katz, J. J., *Language and Other Abstract Objects*, Rowman & Littlefield, Totowa, New Jersey, 1981.
- [12] Katz, J. J., "Common sense in semantics," *Notre Dame Journal of Formal Logic*, vol. 23, no. 2 (1982), pp. 174-218.
- [13] Lewis, D., "General semantics," pp. 169-218 in [5].
- [14] Linsky, L., *Semantics and the Philosophy of Language*, University of Illinois Press, Urbana, Illinois, 1952.
- [15] Linsky, L., "Analytic/synthetic and semantic theory," pp. 473-482 in [5].
- [16] Linsky, B., "Critical notice of Platts's *Ways of Meaning*," *Canadian Journal of Philosophy*, vol. 10 (1981), pp. 513-523.
- [17] Loar, B., "Two theories of meaning," pp. 138-161 in *Truth and Meaning*, eds. G. Evans and J. McDowell, Clarendon Press, Oxford, 1978.
- [18] Platts, M., *Ways of Meaning*, Routledge & Kegan Paul, London, 1979.
- [19] Tarski, A., "The semantic conception of truth," *Philosophy & Phenomenological Research*, vol. 4 (1944); reprinted in [14] (page references are to the reprint).
- [20] Thomason, R., "Recursive unsolvability and theories of semantics," unpublished ditto, 1973.
- [21] Weinreich, U., *Explorations in Semantic Theory*, Mouton Publishing Company, The Hague, 1972.
- [22] Weinreich, U., "On arguing with Mr. Katz: A brief rejoinder," *Foundations of Language*, vol. 3 (1967), pp. 284-287.

Department of Philosophy
University of Alberta
Edmonton, Alberta
Canada T6G 2E1