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THE VALIDITY OF DEFINITIONS

City University of New York PH.D. 1984

THE VALIDITY OF DEFINITIONS

by

Renison J. Gonsalves

A dissertation submitted to the Graduate Faculty in Linguistics in partial fulfillment of the requirements for the degree of Doctor of Philosophy, The City University of New York.

1984

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April 30, 1984
date

Jerrold J. Katz
Chairman of Examining Committee

April 30, 1984
date

Executive Officer

Prof. Thomas Bever

Prof. Arnold Koslow

Prof. D. Terence Langendoen
Supervisory Committee

The City University of New York

Abstract

THE VALIDITY OF DEFINITIONS
by

Renison J. Gonsalves
Adviser: Professor Jerrold J. Katz

In this dissertation I argue for the validity of definitions for semantic representation from the point of view of Jerrold J. Katz's semantic theory. I argue that definitions are valid regardless of whether one looks at language as a psychological object or an abstract object, or if one is interested in a psycholinguistic theory of language performance. I examine a number of recent criticisms of a definitional account of meaning and present various replies to them. I develop a semantic marker account of causative verbs in order to illustrate the explanatory adequacy of a definitional theory.

ACKNOWLEDGEMENTS

This dissertation would not have been possible without the help of a number of people. Much thanks is owed to Professor Jerry Katz, my dissertation adviser, who spent much time in the careful reading of several early drafts, and who gave me invaluable advice and assistance throughout the entire project. Of course, the subject matter of the dissertation itself is built on his semantic theory and his extensive and brilliant work in the field. In addition, it was as a student in his class that I first learned about semantics, and most of my knowledge of semantics came from classes that I subsequently took with him. Professor Tom Bever guided me through some early misunderstandings of the psychological material and helped me to avoid some serious errors. Professor Arnie Koslow was very helpful in explaining to me some of the difficult philosophical material and was always willing to discuss various aspects of the dissertation with me. Professor Terry Langendoen was responsible for helping me to get started in the Ph.D. Program in Linguistics several years ago and has helped me much since then. He read several drafts of the dissertation

and discussed many of the problems with me. Professor Sam Levin, although not a member of my dissertation committee, assisted me through my qualifying examinations and on several occasions discussed various aspects of the dissertation with me.

Much thanks is also owed to my wife, Marcia Babbitt, with whom I discussed many of my developing ideas on the subject, and to my three-year-old daughter, Elisabeth Eta for her inspiration and affection. They both contributed an incalculable amount of time, patience, support, and encouragement, without which I would not have been able to complete this work.

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CHAPTER I

INTRODUCTION: THE NATURE OF THE ISSUE

Over the years the question of the validity of definitions for a theory of meaning has been raised. For example, Fodor, Fodor, and Garrett claim to have demonstrated the psychological unreality of semantic markers, definitional structures crucial to the theory of meaning of Jerrold J. Katz.[1] Similarly, in a more recent paper, Fodor, Garrett, Walker, and Parkes have challenged the validity of definitions on philosophical and grammatical as well as psychological grounds.[2] Here the authors claim to have presented psychological evidence discrediting the view that the meanings of words or lexical items are represented in the mind as having an internal structure that mirrors the definitions of those words or lexical items. And again, in his book Representations, Fodor repeats this criticism of a definitional view, attributing such a view to empiricists who claimed that the

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[1] Janet Dean Fodor, Jerry A. Fodor, and Merrill F. Garrett, "The Psychological Unreality of Semantic Representations," Linguistic Inquiry 6 (1975): 515-531.

[2] J(erry) A. Fodor, M(errill) F. Garrett, E. C. T. Walker, and C. H. Parkes, "Against Definitions," Cognition 8 (1980): 263-367.

primitive elements of definitions are sensory in nature, and that the meanings of lexical items are constructed out of such sensory primitives by induction from experience.[3] On this empiricist view only the primitive sensory concepts are innate, and these concepts constitute a relatively small set when compared to the set of lexical items in a language. Opposed to this view Fodor describes as a rationalist view one which holds that the concepts of all lexical items are primitive and that the mental representations of lexical items do not have internal structure.[4] Fodor claims to present evidence for this latter nativist view, citing frequently the "Against Definitions" paper. So the attack on definitions is presented as a key issue in the larger rationalist-empiricist debate, with the definitional view seen as belonging in the empiricist camp.

This dissertation shall present an argument for the validity of definitions from the point of view of the theory of meaning proposed by Katz, henceforth referred to as ST. In this chapter I will lay the foundation for the argument by answering the questions "What are definitions?" and "What does it mean to ask 'Do definitions exist?'?," and by trying to establish a neutral basis

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[3]Jerry A. Fodor, Representations: Philosophical Essays on the Foundations of Cognitive Science (Cambridge, MA: M.I.T. Press, 1981), pp. 257-316.

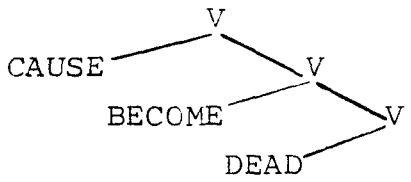
[4]Ibid., pp. 273-83.

for evaluating theories of meaning.

What Are Definitions?

Let us use the term "lexical definition" to refer to the everyday notion of definition on which, for example, "bachelor" is defined as "unmarried man."[5] A lexical definition is an informal paraphrase of the definiens by a phrase itself consisting of other lexical items. There are three important characteristics of such lexical definitions. First, their elements are not part of a formal theory. Second, such definitions lack any hierarchical structure other than the syntactic structure of the defining phrase. And, finally, such definitions do not represent the internal semantic structures of the words they define.

A second type of definition, this one to be found in the theory of meaning most commonly known as "Generative Semantics," would represent the meaning of, for example, "to kill" as follows:[6]



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[5]Jerrold J. Katz, Semantic Theory (New York: Harper and Row, 1972), p. 244.

[6]Janet Dean Fodor, Semantics: Theories of Meaning in Generative Grammar (New York: Thomas Y. Crowell Co., 1977), pp. 77-81.

The elements of this type of definition are part of a formal theory. Moreover, they can be thought of as formal representations of predicates rather than as lexical items themselves. In addition, the structure of this type of definition is hierarchical in a formally significant way. However, this structure is syntactic in nature, as indicated by the labels on the nodes. To make this point more clearly note that the formal representation of the meaning of the verb "kill" when it occurs in a sentence is obtained from the underlying representation of that sentence by a series of transformational rules which use syntactic structure in specifying the structural description and the structural change parts of the rules. In fact, it is because the structures of the meanings of lexical items in Generative Semantics are essentially syntactic that its proponents were forced to disavow any clear-cut distinction between syntax and semantics.[7]

A third type of definition is the semantic marker type of definition developed in ST. This third type of definition differs from lexical definition on all of the three crucial points already mentioned. First, the elements of such semantic marker definitions are elements in a formal theory of meaning over which semantic properties and relations are defined. Second, the structure of this

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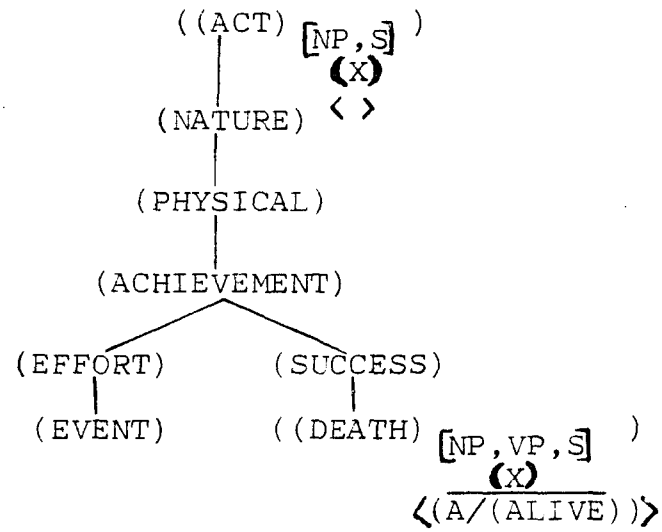
[7]James D. McCawley, "The Role of Semantics in a Grammar," in Universals in Linguistic Theory, eds. Emmon Bach and Robert T. Harms (New York: Holt, Rinehart, and Winston, 1968), pp. 167-68.

type of definition is hierarchical. And, finally, the internal elements in semantic marker definitions are semantic in nature. That is, the structures of semantic marker definitions cannot be confused with syntactic structures since the crucial formal elements defining the structures of semantic markers are distinct from the formal elements which define syntactic structures. Thus we can compare the following semantic marker definition of the verb "to kill" with the generative semantics definition of the same verb given above:[8]



I will explicate this semantic marker definition later. I present it here only to make the point that its structure is determined by semantics and not by syntax. So we note that the interpretive conventions of ST specify that each node of a semantic marker represents a component

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[8]Jerrold J. Katz, Propositional Structure and Illocutionary Force (New York: Thomas Y. Crowell Co., 1977), p. 72.

concept of the sense of the word that the entire complex marker represents.[9] Other interpretive conventions of ST state that the structure of a semantic marker is determined from the uppermost node downward by way of further specifying the concept at a higher node. Hence, the hierarchical structure of a semantic marker represents superordinate and subordinate relations between concepts within the structure. The only aspects of syntactic structure that appear in semantic markers are specifications of grammatical relations in catergorized variables. These specifications of grammatical relations are needed by ST to correlate representations of senses with representations of syntactic structure, so that ST can reconstruct the sound-meaning relation in language.

What it Means to Ask the Question "Do Definitions Exist?"

In their paper "Against Definitions," Fodor, Garrett, Walker, and Parkes state that they understand the question "Do definitions exist?" as being equivalent to the question "Are definitions psychologically real?"[10] They do not give any argument for claiming that these two questions are equivalent. I shall argue here very briefly that the two questions are not at all equivalent.

First, it is obvious that the questions "Do/Does X

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[9] Ibid., pp. 62-69.

[10] Fodor, Garrett, Walker, and Parkes, p. 517.

exist?" and "Are/is X psychologically real?" are not equivalent. For example, the question "Does a non-Euclidean space exist?" is clearly not equivalent to the question "Is a non-Euclidean space psychologically real?" Therefore, the reason for taking these question frames to render equivalent questions in the special case of definitions must have something to do with the case itself, and in particular the fact that it concerns a theory of language. Now the theory which holds language to be a psychological object, a theory which we might attribute to Chomsky and which we might call competencism, is particularly relevant here. When we think of this theory, a theory which Chomsky sees as a branch of cognitive psychology,[11] and, consequently, of biology,[12] the missing argument for the equivalence of the questions "Do definitions exist?" and "Are definitions psychologically real?" would seem to run somewhat as follows. Grammars, according to Chomsky, represent the tacit knowledge the ideal speaker-hearer has of his language, and since this knowledge is psychological--that is, this knowledge exists in the mind--then whatever is part of the grammar exists in the mind. Then, if definitions are part of the grammar, then definitions must exist in the mind, must be psychologically real.

If this or something very close to it is the argument

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[11] Noam Chomsky, Rules and Representations (New York: Columbia University Press, 1980), p. 4.

[12] Ibid., p. 226.

that Fodor, Garrett, Walker, and Parkes have in mind, then their reasoning makes the mistake of confusing the object of knowledge with knowledge itself.[13] That is, Chomsky holds that a grammar is both a theory of the structure of a language and of the knowledge that the ideal speaker-hearer has of the structure of this language. But this is like taking one's knowledge of things to be the same as the things that one knows--for example, our knowledge of Abraham Lincoln to be Abraham Lincoln. But this is clearly false. Moreover, this mistake underlies Chomsky's equating of a grammar, which is a theory of a language, with some ideal knowledge of that language.

Katz notes that there are several possible positions available to competencism for replying to this argument.[14] In particular, Chomsky has suggested the use of the term "cognize" to replace the term "know," intending thereby that "knowledge should be analyzed at least in part . . . in terms of possession of certain mental structures."[15] Now the "knowledge of" objection assumes that these mental structures that constitute knowledge reflect an external,

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[13] Jerrold J. Katz, Language and Other Abstract Objects (Totowa, New Jersey: Rowman and Littlefield, 1981), pp. 76-92.

[14] Ibid., pp. 79-81.

[15] Chomsky, Rules and Representations, p. 99.

objective reality called language. But Chomsky might reply that there is no such coherent notion of language; in fact, according to Chomsky, "'language' is no well defined concept of linguistic science."[16] In addition, instead of taking these mental structures that constitute knowledge of language to be representations of an abstract, objective reality, we might take them to be, in the mature language user, the result of the lifetime growth and development of innate structures, analogous to the development of any organ of the body.[17] The argument on Chomsky's side, then, would be that Katz's "knowledge of" objection assumes that language has its cause outside of the mind, while it is just this assumption that Chomsky's radically nativist view denies.

Later I will examine the consequences of this nativist view of language for an understanding of the development of knowledge of word meaning. I shall argue that taking language to be a psychological object forces competencists into claiming a priori that there is a one-to-one correspondence between definitionally primitive concepts and developmentally primitive concepts. On the other hand, taking language to be other than a psychological object allows for the possibility that developmentally primitive concepts may be more "fine grained" than the definitionally primitive concepts, a possibility suggested by the empiri-

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[16]Ibid., p. 217. [17]Ibid., p. 241.

cal evidence.[18]

So as not to prejudge the question of what is or is not innate, I shall construe the question "Do definitions exist?," within linguistic theory, as asking whether definitions are a legitimate part of a theory of language. That is, the question is whether or not definitions serve a crucial role in the grammars of natural languages. In this dissertation I shall argue that definitions do serve such a crucial role.

A Neutral Basis for Evaluating Theories of Meaning

Before deciding the answer to the question "Do definitions exist?," we ought to establish a neutral basis for resolving the issue. Above we interpreted the question as asking whether or not definitions are a crucial part of a theory of language. The issue now becomes, then, how do we decide whether something is part of such a theory.

In Aspects of a Theory of Syntax Chomsky explicates two notions of adequacy of grammars: descriptive adequacy an explanatory adequacy.[19] However, Chomsky's notions of adequacy here are tied up with the identification of a grammar as a psychological theory. As I have suggested above, such a claim leads to the unsupported conclusion

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[18]Susan Carey, "Semantic Development: the State of the Art," in Language Acquisition: The State of the Art (Cambridge, MA: Cambridge University Press, 1982), pp. 347-389.

[19]Noam Chomsky, Aspects of a Theory of Syntax (Cambridge, MA: M.I.T. Press, 1965), pp. 24-27.

that language is an entirely innate structure and that whatever is in the grammar must be psychologically real. We would want to stay away from such notions of adequacy since they have a heavy psychological bias.

Fortunately, there is an alternative notion of adequacy already available to us in Katz's Language and Other Abstract Objects.[20] Combining Katz's D5 and D5' we get:

> A grammar G is an optimal grammar for a language L if, and only if, G implies every true evidence statement about L, G provides the grammatical basis for explaining every grammatical phenomenon in L, and no other grammar that does this is simpler than L.

The notion of explanation here is simply the standard notion of providing reasons for a certain phenomenon or telling how a certain phenomenon comes about. For example, suppose a sentence is two ways ambiguous. Now the grammar should predict this ambiguity by assigning the sentence two different semantic representations in accordance with the definition of ambiguity in ST. However, this assignment of two semantic representations would not constitute an explanation of the ambiguity of the sentence. Rather, the explanation would lie in how or why it came about that the sentence was assigned these two semantic representations. So if there is a lexical item in the sentence that is assigned two semantic representations in the dictionary, and if these two semantic representations of this lexical item enter into the construction of the two semantic

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[20] Katz, Abstract Objects, pp. 66-67.

representations of the sentence in the appropriate manner, then we have an explanation of the ambiguity of the sentence.

In addition, the notion of simplicity is to be understood as a notion that is not specific to linguistics, but rather as the notion of simplicity that applies to all scientific theories, what is called "Occam's razor."

In general, the notions of explanation and simplicity are derived from standard scientific methodology.[21] As such they are not biased in favor of any one theoretical position within linguistics. Now the notion of evidence statement here is vulnerable to partisan interpretation. In particular, Chomsky has suggested that evidence from psychology and neurophysiology can be as relevant as evidence from native speakers' intuitions about the grammatical properties and relations of the sentences of their languages.[22] But to construe "evidence statement" so broadly would bias the issue at the outset in favor of a psychological interpretation. Therefore, I shall construe "evidence statement" to refer to statements of native speakers regarding the grammatical properties and relations of the expressions of their languages. This I shall take as the primary source of evidence in deciding the issue of the validity of definitions in a theory of

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[21] Katz, Abstract Objects, pp. 62-65.

[22] Chomsky, Rules and Representations, p. 200

language. I shall take psychological evidence as bearing on a psycholinguistic theory. However, I shall argue that given the psychological evidence to date, definitions are also a valid part of such a psycholinguistic theory.

Essentially, then, my argument shall support the following three claims: From the points of view of

a. a psycholinguistic theory of language;

b. a competencist view of language; and

c. a platonist view of language,

a definitional account of the meanings of words is valid and preferable to a non-definitional one. The reason for presenting the argument in this form is specifically so that the validity of definitions can be established independently of whether one takes a platonist view of language as an abstract object, or a competencist view of language as a psychological object, or in case one is primarily interested in a psycholinguistic theory.

The only metatheoretical position from which I do not argue for the validity of definitions is the position that a grammar is a model of language performance--that the grammar is directly involved in language processing. This position, which, in so far as definitions are concerned, is the position taken by Fodor, Fodor, and Garrett, and by Fodor, Garrett, Walker, and Parkes, would entail that understanding a sentence requires the retrieval of its complete semantic representation. However, this position is not motivated, as I will argue, by attempts to relate

other aspects of grammar to sentence processing. Moreover, it runs counter to the competence/performance distinction that has been largely accepted by linguists working in the tradition of generative grammar, particularly as this distinction is expressed in Chomsky's Aspects of a Theory of Syntax.

CHAPTER II

THE INCEPTION OF THE CONTROVERSY

Criticism of a definitional approach to a theory of meaning goes back at least to Wittgenstein's Philosophical Investigations where he argues against the possibility of stating definitions for complex concepts, offering instead a theory of family resemblances.[1] Similarly, Putnam also considers strict definitions to be impossible and develops the notion of a "cluster concept."[2] However, since in this dissertation we are looking at the question of the validity of definitions from the point of view of a particular theory, namely ST, and since, moreover, many of these earlier criticisms of a definitional approach to complex concepts are also discussed in more recent criticisms, we shall begin our discussion of the controversy of the validity of definitions by looking at a paper criticizing the definitional approach that specifically cites Katz's semantic marker theory, and which presents psycho-

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[1]Ludwig J. J. Wittgenstein, Philosphical Investigations (New York: Macmillan, 1953), pp. 32-34.

[2]Hilary Putnam, "The Analytic and the Synthetic," in Minnesota Studies in the Philosophy of Science, eds. H. Feigh and G. Maxwell, Vol. III (Minneapolis: University of Minnesota Press, 1962), p. 378.

logically based as well as philosophically based arguments against a definitional view. This paper is a paper by J. D. Fodor, J. A. Fodor, and M. F. Garrett, "The Psychological Unreality of Semantic Representations."[3] We shall look at Katz's reply to this, "The Real Status of Semantic Representations,"[4] and I shall develop a reply of my own directed particularly to the psychological evidence.

In brief, Fodor, Fodor, and Garrett's paper (henceforth FFG) presents a psychological reality constraint on semantic representations and goes on to provide intuitive as well as experimental evidence which they claim show that semantic representations do not meet the psychological reality constraint. They go on to argue, therefore, that the definitional type of semantic representation does not exist, but rather that "to each morpheme of the surface vocabulary of a natural language corresponds a primitive expression in the vocabulary of the representational system."[5] They continue to argue, furthermore, that "if our arguments are sound, then it appears practically mandatory to assume that meaning postulates mediate whatever entailment relations between sentences turn upon their lexical content."[6]

In Katz's reply to FFG, he first attacks the validity

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[3]Linguistic Inquiry 6 (1975): 515-531.

[4]Linguistic Inquiry 8 (1977): 559-584.

[5]Fodor, Fodor, and Garrett, p. 515. [6]Ibid., p. 525.

of their psychological reality constraint, citing the existence of other theoretical options, and summarizes an argument of his own against the adequacy of meaning postulates. Katz also rejects FFG's psychological evidence, which is concerned with the relative semantic complexity of words, as being irrelevant to a theory of the semantic properties and relations of the sentences of a language. Katz completes his reply by suggesting reasons for the "controversial and unsettled state" of semantics, citing its relatively recent beginnings, and its need to contend with extensionalist philosophies of meaning.[7]

In this chapter I shall examine two of the issues discussed in these papers, FFG's psychological reality constraint and their psychological evidence. I shall reserve for the next chapter a discussion of the meaning postulate option and the extensionalist-intensionalist debate.

The Psychological Constraint

FFG's psychological constraint is stated as follows:

> A. Semantic Representations are psychologically real in the sense that, given appropriate idealizations, understanding a sentence requires the recovery of its semantic representation.[8]

The sort of definitionally based theories of meaning that FFG are critizing assume also, they claim, the following two conditions:

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[7]Katz (1977), pp. 582-83.

[8]Fodor, Fodor, and Garrett, p.515.

> B. A variety of properties and relations that are pretheoretically identified as semantic are formally definable over representations at the semantic level.
> . . .
> C. Not every lexical item of the natural language corresponds to a syntactically simple expression at the semantic level. That is, there are definable lexical items of L, and these are to be represented by defining phrases of the language in which semantic representations are couched.[9]

FFG present the following arguments in favor of condition A. First, they argue that A is a consequence of a general requirement at all levels of grammar that structural descriptions must be psychologically real. They claim that this is the weakest psychological reality requirement on grammars since, as Fodor et al. have shown,[10] claims for "the psychological reality of grammatical operations entail claims for the psychological reality of structural descriptions, but not vice versa." In addition, they argue, the only other of the possible "psychological states or computations"--i.e., other than comprehension--in which semantic representations could play some part is memory for sentences. However, they claim, it has been fairly well established that "what is recalled in remembering a sentence corresponds to none of its linguistically motivated representations." Finally, they argue that condition A would provide for "an extremely natural account of

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[9] Ibid..

[10] J. A. Fodor, T. G. Bever, and M. F. Garrett, The Psychology of Language (New York: McGraw Hill, 1974).

communication exchanges between speakers and hearers."[11]

In Katz's reply he notes that condition A does not allow for the competence/performance distinction of Chomsky. That is, while A might be a possible constraint on a performance model of a speaker-hearer's actual use of a sentence, it should not be a condition on a model of the ideal speaker-hearer's tacit knowledge of his language. That is, such a competence model makes no claims as to how it is incorporated in a performance model. The connection between the performance model and the competence model may be an extremely abstract one, the performance model being allowed to utilize many types of heuristic devices. The existence of the competence theory, which is attributed to Chomsky and which Katz calls competencism, is, Katz claims, an embarrassment to FFG's claims for condition A.[12]

In addition, Katz presents an alternative theory concerning the ontological status of grammars, namely his platonist theory. Platonism holds that the grammar of a language is a theory concerning the grammatical properties and relations of the sentences of the language, the sentences being abstract objects like numbers. Moreover, whereas we use the intuitions of native speakers of a

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[11]Fodor, Fodor, and Garrett, p. 516.

[12]Katz (1977), pp. 159-164.

language as a source of the evidence which the grammar must predict and explain, these intuitions are not what the grammar is about--they are not the grammar's import. Katz makes this source-import distinction to rebut an FFG argument that since linguistic theory uses native speakers' intuitions as its primary source of evidence, then it would be incoherent to claim that a grammar is about abstract objects. That is, according to FFG, a theory about abstract objects should not make use of intuitions as its primary source of evidence. Katz points out, however, that mathematics, which is widely (and not implausibly) considered by many mathematicians and philosophers to be about abstract objects, also depends on intuitions. In any event, since the platonist view of the ontological status of grammars is a real alternative, in addition to competencism, condition A is not well established as a condition on grammars. Rather, A represents a third theory concerning the status of grammars, a theory which claims that a grammar should represent the psychological structures involved in the on-line processing of sentences, a theory which Katz calls performancism.[13]

I think that it is worth taking a close look at FFG's argument for condition A because it will help to clarify what is wrong in their argument and the way in which psychological evidence of the type that FFG cite affect the

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[13]Ibid..

development of theories of grammar. FFG claim that according to Fodor, Bever, and Garrett claims for the psychological reality of grammatical structures do not entail claims for the psychological reality of grammatical operations. This idea stems from earlier experiments to test the psychological reality of transformations. The idea was that sentences that involved more transformations, that were transformationally more complex, should be more difficult to understand, should take more time to process and should use up more memory space. The experiments, as is now well known, though for a time apparently positive, ultimately failed to support this view. Other experiments, however, offered some evidence for the psychological reality of the constituent phrase structure of sentences.[14] FFG seem to be suggesting, then, that while these experiments failed to support the notion of the psychological reality of operations like transformations, they left us with the notion that grammatical structures were psychologically real.

However, if we look at the logic of the findings of these experiments we will find that they weigh against the psychological reality of grammatical structures just as much and in so far as they weigh against the psychological reality of grammatical operations. Thus, in the case of

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[14]Jean Aitchison, The Articulate Mammal (New York: Universe Books, 1976), pp. 159-176.

the psychological experiments that failed to support the notion that transformations were involved in sentence comprehension, we could also say that these experiments failed to show that the structural description and the structural change parts of these same transformations played any such psychological role either. That is, what these experiments failed to show regarding certain grammatical operations they also failed to show regarding the structures corresponding to the grammatical operations.

If, as FFG seem to think, such experiments leave us with the possibility (not to speak of the requirement) that grammatical structures are psychologically real, then it is completely ad hoc which grammatical structures we must force to conform to this requirement. That is, the psychological experiments did not leave us with the claim that whereas grammatical operations were not psychologically real, grammatical structures were. Indeed, such a claim would be untenable. Rather, what such experiments left us with was a notion of psychological reality different from the notion of being involved directly in sentence processing. This unmotivated claim for the direct participation in sentence processing of grammatical structures, it seems to me, is the mistake of FFG in proposing condition A as a psychological reality condition, and, in fact, as "the weakest that a grammar can satisfy if it is to contribute to a psychological model of the speaker/hearer at all."[15]

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[15]Fodor, Fodor, and Garrett, p. 516.

FFG take psychological reality to be decided by whether some aspect of a grammar is actually involved in performance. That is, FFG seem to see Chomsky's competence grammar as equivalent to a model of a sentence processing mechanism. Of course, it has been repeatedly pointed out by Chomsky himself that a competence grammar should not be taken to be a processing model, but rather that the relation between a competence grammar and a processing model might be a very abstract one,[16] the processing model consisting, as suggested by Fodor, Bever, and Garrett, of a set of heuristic devices.[17]

In general, then, one of the theories that emerges from the psycholinguistic experiments regarding the relation between a competence model and a performance model is somewhat as follows. A competence model represents a speaker's tacit knowledge of the structure of his language. A speaker has this knowledge in his mind but does not necessarily use it in language performance. Rather, he has a separate performance mechanism which he uses for language performance, resorting to the competence grammar only when the performance mechanism proves inadequate. The performance mechanism itself is made up of various heuristic devices that are directly involved in sentence processing. There are aspects of grammatical sentences that might impede the ability of the heuristic devices to

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[16] Noam Chomsky, Aspects of a Theory of Syntax (Cambridge, MA: M.I.T. Press, 1965), pp. 9-15.

[17] Fodor, Bever, and Garrett, pp. 367-72.

yield an account that constitutes an understanding of the sentence. An example of such an aspect of a sentence is self-embedding. Self-embedding goes counter to the heuristic devices for the normal processing of a sentence. Given a multiply self-embedded sentence, however, the ideal speaker-hearer, who is not constrained by the exigencies of memory space, has no trouble in rendering a description of the sentence that constitutes an understanding of it. For the real life linguist, of course, this may mean resorting to pencil and paper and filling in labeled bracketings.

Now it is important to note that the competence grammar in no way predicts that self-embedded sentences will be more complex than non-self-embedded sentences. What predicts the complexity of self-embedded sentences is the processing model, the set of heuristic devices for the parsing of sentences.

In this connection it is interesting to note that FFG cite an article by Schwartz, Sparkman, and Deese in order to claim that "it is demonstrable that intuitions of relative syntactic complexity are reliably correlated with experimentally derived rankings, so they have a prima facie claim to construct validity in the present case too."[18] When one takes a look at the cited article by Schwartz, Sparkman, and Deese, one finds, not surprisingly, that their experimental materials consist largely of right-

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[18]Fodor, Fodor, and Garrett, p. 519.

branching, left-branching, and center-embedded sentences. The results of their experiment showed that center-embedded sentences and right-branching sentences were more difficult to understand than left-branching sentences.[19] Now, the suggestion that these results support claims for the psychological reality of the syntactic structures which a competence grammar would posit for such sentences is an empty suggestion since a competence grammar in no way predicts the relative complexity of these structures (see Aspects of a Theory of Syntax, pp. 11-12). Again, it is the heuristic devices for processing and not the competence grammar that makes such predictions. Further, that intuitions of complexity are supported by experimental results says nothing for the competence model. For example, it certainly doesn't entail that we should have some device in the competence grammar for predicting the relative complexity of such structures. Rather, the best we can say is that the complexity intuitions and the experimental results both support a performance model--for example, the heuristic devices for sentence parsing in Fodor, Bever, and Garrett[20]--which predicts such complexity rankings.

Going from syntax to semantics, it would seem to be

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[19]D. Schwartz, J. P. Sparkman, and J. Deese, "The Process of Understanding and Judgements of Comprehensibility," Journal of Verbal Learning and Verbal Behavior 9, pp. 87-93.

[20]Fodor, Bever, and Garrett, pp. 344-61.

the case, similarly, that there is no need to impose on a competence theory of semantic representation the requirement that it predict relative complexity rankings. Rather, what such semantic representations predict are intuitions regarding the semantic properties and relations, relative complexity in FFG's sense of relative difficulty in processing not being one of them.[21]

The Psychological Evidence

FFG present two classes of psychological evidence to support their claims against the psychological reality of definitions. In the last section I argued that their condition A should not be a constraint on grammars as theories of competence. Since the use of their psychological evidence in arguing against definitions is based on an acceptance of condition A, by removing condition A as a valid constraint on competence grammars I have also rendered their psychological evidence inconsequential to the question of psychological reality. Here I shall argue that based on a semantic marker definitional account of semantic representation in a competence grammar, a performance hypothesis that I will suggest can give an adequate account of their psychological evidence.

The first type of psychological evidence presented by FFG they call intuitive evidence. According to FFG, if a definitional account of semantic representation is

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[21] Katz (1977), pp.567-574.

correct, then the sentence "cats chase mice" should be more complex than the sentence "cats catch mice." However, according to FFG, native speaker intuitions hold these sentences to be equally complex. Such intuitions, then, weigh against a definitional account in FFG's view.[22]

But FFG's argument here is not at all convincing. That "cats chase mice" and "cats catch mice" are equally difficult to understand might be taken to show simply that relative difficulty in comprehension is not a function of definitional complexity. To conclude on the basis of this that definitions are not psychologically real is to make certain assumptions about the nature of the accessing routine. In particular, the assumption must be that the component parts of the meaning of a word are accessed one by one. But this assumption could be wrong. It could be that the entire complex structure of a word's meaning is accessed all at once as a single complex chunk of information with hierarchical structure. If this were the case, the relative internal complexity of word meanings would not correlate with relative difficulty in comprehension. Therefore, FFG's intuitive evidence proves nothing about the question of the psychological reality of definitions.

The second type of psychological evidence presented by FFG they call experimental evidence. This type of evi-

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[22]Fodor, Fodor, and Garrett, pp. 518-520.

dence involves four types of negatives: explicit negatives, e.g. "not married"; implicit negatives, e.g. "doubt," "deny," "fail"; morphological negatives, e.g. "unmarried"; and pure definitional negatives (PDNs), e.g. "bachelor." Of these, implicit negatives and PDNs need some explication. An implicit negative is a word that is "negative in sense and whose scope exhibits such typical reflexes of negatives as any, much, give a damn, etc., but which has no obvious morphological decomposition in terms of explicit negation." PDNs are "words that have 'negative' as an element of their definitions, but which contain no negative morpheme, and which do not constitute syntactic environments for negative polarity items and so are not implicitly negative by that criterion."[23]

The experimental procedure involved having subjects make judgements regarding the validity of arguments represented by sentences containing an explicit negative element or a quantifier together with one of the four classes of negative elements, and measuring response times for correct and incorrect answers. The following is an example of such a sentence as provided by FFG: "If practically all of the men in the room are $\left\{\begin{matrix}\text{not married}\\ \text{bachelors}\end{matrix}\right\}$, then few of the men in the room have wives." FFG summarize their results as follows:

1. Both morphological and implicit negatives tended

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[23]Ibid., pp. 520-22.

> to be easier than their explicit counterparts, though in neither case is this tendency statistically significant on our current data.
>
> 2. The difference between morphological negatives and explicit negatives is quite comparable to the difference between implicit negatives and explicit negatives.
>
> 3. Arguments containing PDNs, however, were significantly easier than the paired arguments containing explicit negatives. Moreover, and most important, the difference between PDNs and explicit negatives was significantly greater than the difference between explicit negatives and either implicit or morphological negatives. We take this result to suggest strongly that PDNs do not act as though they contain a negative element in their linguistic representation; and therefore, that PDNs are not semantically analyzed at any level of linguistic representation.[24]

FFG thus seem quite convinced that both their intuitive and their experimental evidence constitute fairly convincing evidence for excluding definitions of lexical items from the semantic representations of the grammar. Above I have argued that their intuitive evidence does not in fact support such a conclusion. Before turning to their experimental evidence I would like to point out one important way in which ST differs from the sort of definitional theory implied by their condition C.

Condition C states that some "definable lexical items of L . . . are to be represented by defining <u>phrases</u> of the language in which semantic representations are couched." It is crucial for us to note, then, that condition C does not represent ST since semantic markers are not phrases but

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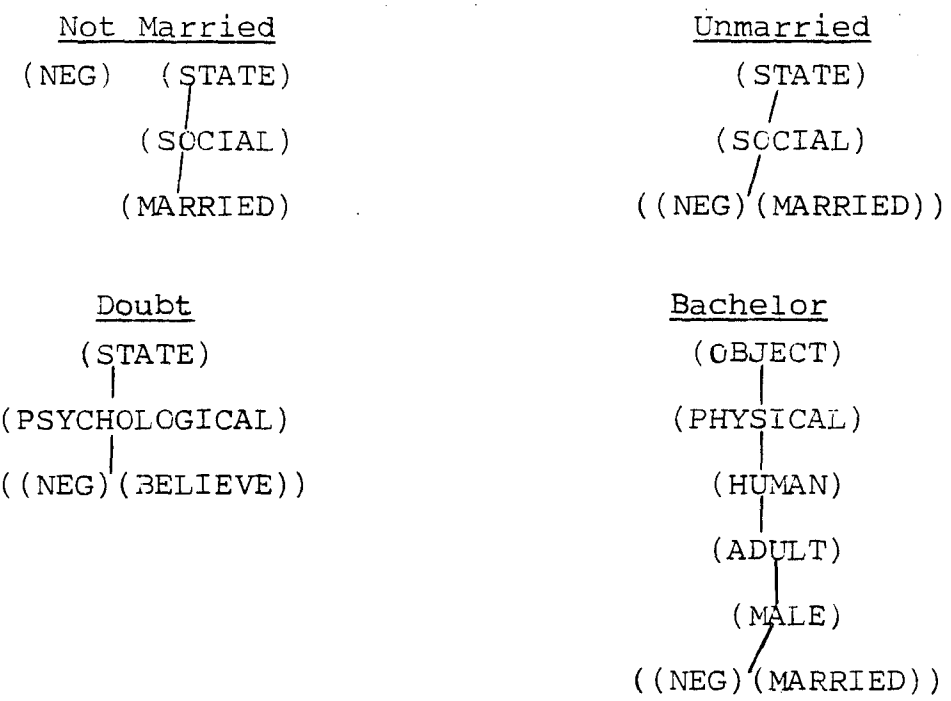
[24]Ibid., p. 522.

are hierarchical structures, and since, moreover, as Katz has pointed out, semantic representation in ST does not constitute a language any more than does syntactic or phonological representation. FFG do not seem to be aware of these aspects of a semantic marker theory that, as I will now show, enable a performance hypothesis incorporating a semantic marker competence theory to explain FFG's psychological evidence.

Suppose we construct the following semantic marker definitions for the four test items, "not married," "unmarried," "doubt," and "bachelor":



Let us say that these are rough accounts of the semantic representations of these lexical items in the competence grammar. I now propose the following hypothesis, which I shall refer to as the distance hypothesis:

D. The further away a negative element in a semantic

> marker is from the topmost node, the less affect this negative element will have on difficulty of comprehension.

My contention then is that the distance hypothesis D provides a well motivated explanation of FFG's experimental results. For notice that in semantic markers for PDNs like "bachelor" the negative element is buried deep in the semantic marker so that it can be expected to have negligible effect on semantic interpretation. However, in the case of implicit and morphological negatives, the negative element in the semantic marker is sufficiently close to the topmost node to have a significant effect on semantic interpretation, almost as much of an effect as if this negative element were in the topmost node, as is the case with explicit negatives. This accounts for FFG's findings that test sentences with morphological and implicit negatives were only slightly easier to understand than sentences with explicit negatives.

Notice that the notion of distance is not new to linguistics, having been used to account for various linguistic phenomena. For example, Chomsky's locality principle, although related to competence directly and only indirectly to performance, makes crucial use of a notion of distance. And in phonology the environments for rule application are usually not at all far from the forms that undergo the rule change. Distance, then, is an all too natural idea in linguistics.

Finally, this semantic marker approach offers a

principled semantic distinction between what FFG call "implicit negatives" and what they call "PDNs." The distinction is simply that in the case of PDNs the negative element is low down in the hierarchical structure, whereas in implicit negatives the negative element is very close to the topmost node.

CHAPTER III

THE DEBATE CONTINUED

As I have already mentioned, the Fodor, Garrett, Walker, and Parkes paper, "Against Definitions" (henceforth FGWP), and Fodor's chapter, "The Present Status of the Innateness Controversy," continue the line of criticism of a definitional theory of meaning as set forth in the FFG paper discussed in the preceding chapter. Moreover, recent research into the acquisition of concepts, as represented, for example, by the work of Susan Carey,[1] as well as by the work of Sharon Armstrong, Lila Gleitman, and Henry Gleitman,[2] argues against a defitional theory of meaning, relying largely on criticisms of the kind presented by FGWP. In fact, such research, while failing to support any clearly worked out theory as to the psychological representation of the meanings of words and the acquisition of such representations, has suggested that given the work of FGWP a definitional theory is no longer a viable option, at least not from a psychological point

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[1] Susan Carey, "Semantic Development: The State of the Art," in Language Acquisition: The State of the Art, eds. Eric Wanner and Lila Gleitman (New York: Cambridge University Press, 1982)

[2] Armstrong, Gleitman and Gleitman, "What some Concepts Might Not Be," Cognition 13 (1983), pp. 263-308.

of view.

Moreover, apart from dealing with the psychological evidence, in order to establish the validity of definitions we must deal with a number of philosophical arguments against a defitional theory of meaning. The articles mentioned above all refer in different ways to these philosophical arguments. In particular, FGWP attempt to set forth such arguments against a definitional theory of meaning quite explicitly.

Thus, FGWP argue specifically against a complex of views which they call "The Standard Picture," and which they see as supported by the notion that defitional constructs representing the meanings of words have philosophical as well as psychological validity. They cite five claims as coming under the heading "The Standard Picture" or "TSP," and present arguments against four of them. The four claims against which they provide arguments are as follows:

1. "The definition of a word determines its extension";
2. "Definitions underwrite the validity of informally valid arguments";
3. "To understand a word is to recover its definition";
4. "Definitions express the decomposition of concepts into their elements" and thus help to explain concept learning.

The fifth claim which they see as coming under the heading of TSP but which they do not provide arguments against is as follows:

5. "Definitions guarantee the necessity (or unrevis-

> ability, etc.) of certain general truths, e.g. that bachelors are unmarried or that F=MA."

This latter claim they leave to a footnote, stating that "it is pretty thoroughly discredited as the result of work by such philosophers as Duhem, Putnam, and especially, Quine."[3]

In addition to providing such arguments against the plausibility of TSP "independent of questions about the empirical status of the definition construct," they provide experimental evidence that, according to FGWP, makes "a case that some important predictions which flow naturally from the view that definition is a basic notion in the theory of language are strikingly disconfirmed." While conceding that this is not a "crucial experiment," they go on to say that "probably nothing could be."[4]

In this chapter, therefore, I will reply to FGWP. I will argue that TSP is a stacked deck, stacked, moreover, so that it is vulnerable to some of the arguments they present. I will show that the more thorough and integrated theory of meaning presented by Katz, which incorporates that aspect of the notion of definitions on which definitions are decompositions of the meanings of words, is not vulnerable to FGWP's arguments. I will show that Katz's theory of meaning accounts for and justifies the intuitive

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[3]Fodor, Garrett, Walker, and Parkes, pp. 265-78.

[4]Ibid., p. 264.

appeal of definitions and that FGWP's psychological data does not weigh against such a theory.

The Goals of a Theory of Meaning and The Intensionalist-Extensionalist Debate

In presenting their arguments against the first of the claims mentioned as being part of TSP--that is, the claim that the defintion of a word determines its extension--FGWP explain the importance of the definition construct for this claim. Definitions provide decompositions of the meanings of words. The attractiveness of such a notion for this particular claim, therefore, is, according to FGWP, that it delays the interpretation of expressions. The process of assigning "interpretations" to expressions, according to FGWP, works somewhat as follows: words are decomposed into the simplest units of their meanings by definitions. At the final point in the decomposition process the elements of the meanings of words are primitive elements of the definition theory. That is, these elements are elements out of which complex meanings are created, and they cannot themselves be further decomposed. But at this stage, according to FGWP, in order to have a true theory of the connection between language and the world, we must have an "interpretation" of the primitive elements of definitions. By "interpretation" they mean an assignment of extensions to these primitive elements. One major problem of such a definitional theory, however, is that such an "interpretation" has never been

convincingly provided by any form of a definition theory without further appeal to definitions. The only theory providing such an interpretation is one which claims that the items in the primitive vocabulary express sensory/motor properties. The extensions of the primitive items, this theory claims, can therefore be established on the basis of a causal connection between their extensions and the sensory/motor traducers. But, as FGWP point out, it is difficult to see how most words could be defined by sensory/motor terms, and so this theory is implausible.

On the basis of such considerations FGWP come to the conclusion that the definition theory is of no help in providing a theory of the connection between language and the world. Furthermore, a definition theory without "interpretation" provides only "a theory of a relation between uninterpreted linguistic forms."[5]

It is at this point in their paper that there is an equivocation on the meaning of the expression "interpretation" between two quite distinct meanings. On one meaning interpretation may be construed as a relation holding between the expressions in a language and their senses. This is the meaning of interpretation made use of in a semantic theory that sets as its goals the prediction and explanation of such semantic properties and relations as meaningfulness, anomaly, ambiguity, redundancy, synonymy,

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[5]Ibid., pp. 265-68.

contradictoriness, and so on. In such a semantic theory, interpretation can be seen as existing in terms of the correlation between the predictions and explanations of semantic properties and relations in the theory and native speaker intuitions about the same. The reference of utterances is excluded from such a theory and left for a separate theory of token reference, itself a part of the broader theory of language use.

What I am suggesting here is that if we are to take seriously the proposed requirement that certain elements in the vocabulary of definitions be assigned extensions in some particular world in order for definitions to have an interpretation, then we would be confusing grammar with pragmatics. Extensions in some particular actual world can only be assigned to words in an actual utterance--they cannot be assigned to words in a dictionary or to the primitive elements of the meanings of words. At a minimum, incorporating such assignments of extensions in a dictionary or elsewhere in the grammar of a language would mean incorporating extra-linguistic, encyclopedic knowledge. However, since Chomsky it has been the tradition to distinguish extra-linguistic knowledge from linguistic knowledge on the basis of the competence/performance distinction. Therefore, if we incorporate this distinction in a theory of reference we can provide the definitional theories account of the connection between language and

the world.[6]

The distinction made by Katz between type reference and token reference serves to delimit the domain of the theory of meaning with respect to reference. Thus, type reference is the reference of linguistic types in the language (hence reference without the aid of contextual information based solely on meaning in the language). On the other hand, token reference is the reference of expressions in actual utterances, taking both the meaning in the language and contextual information into consideration. It is only the former, type reference, that is properly considered to be in the domain of a theory of language meaning. Of course, type reference and token reference might not coincide just because of the additional contribution of contextual information to the meaning of a sentence token. For example, the type reference of the expression "the man in the green suit" is to some man wearing a green suit. But when spoken by an individual who is color blind and who points to the man intended, the expression may well token refer to a man in a blue suit. Because of this discrepancy between type and token reference, the former making use exclusively of linguistic information while the latter also includes contextual information, token reference is left to a theory of

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[6]Jerrold J. Katz, "The Neoclassical Theory of Reference," in Contemporary Perspectives in the Philosophy of of Language, eds. P. French, T. Uehling, Jr., and H. Wettstein (Minneapolis: University of Minnesota Press, 1979), pp. 103-22.

pragmatics. A theory of type reference is necessarily prior to a theory of token reference, since the latter will need to appeal to reference in the language, among other things.

This neo-classical theory of reference breaks down the problem of the reference of utterances into two components, one included in and necessary for the other. The idea is simply that since the reference of utterances depends on two sorts of information, one relying on the other, such reference cannot be handled directly without an intervening level of type reference. The hypothesis behind the theory is that without such a level of type reference, a theory of reference would fail at the level of explanatory adequacy.

For example, Putnam[7] and Kripke[8] have both suggested a causal theory of reference. Putnam suggests that natural kind words succeed in referring by application of the formula "same natural kind as _____," with the blank space filled in with a representation of an exemplar determined in some pragmatic fashion. The problem with this theory, however, is that the formula must be constrained, in certain cases, to pick out just those features of the exemplar which are crucial to its being a member of a

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[7]Hilary Putnam, "The Meaning of Meaning," in Minnesota Studies in the Philosophy of Science, ed. K. Gunderson (Minneapolis: University of Minn. Press, 1975).

[8]Saul Kripke, "Naming and Necessity," in Semantics of Natural Language, eds. D. Davidson and G. Harman (Dordrecht: Reidel, 1972).

particular natural class. This can be seen clearly if we consider how such a formula would succeed in picking out reference in the case of members that are on the periphery of a natural class or non-members that might bear superficial resemblances to an exemplar. Specifically, we may consider how successful such a formula would be in identifying a duck as a member of the natural class of birds, or how successful it might be in identifying a whale as a non-member of the natural class of fish. My objection to Putnam's theory, then, is just that it must include definitional terms in order to avoid mistaken reference, or embrace an inexact notion of reference. If, as seems likely, Putnam's theory chooses the latter course, particularly in view of the empirical finding that people do in fact tend to make the types of mistakes in reference considered here, we might object that Putnam is failing to consider the competence side of language, the notion of the ideal speaker-hearer, whose referential apparatus must be as exact as the other aspects of his linguistic competence. In the unlikely case that Putnam's theory chooses to incorporate definitional components to account for reference on the periphery of a class, we can claim that Putnam's theory reduces to a definitional theory after all.

So for the purpose of reference we need the senses of words and expressions and a definitional theory which expresses these senses in an explicit manner that provides

necessary and sufficient conditions for exact type reference. FGWP's criticism of TSP is valid only because TSP offers a naive intensionalist account of reference. Moreover, FGWP's claim that only a stimulus-response theory has been offered to explain the connection between language and the world is simply false. There is the neoclassical theory of reference presented by Katz and sketched here which does not require the counter-intuitive notion that all primitive definitional components represent sensory-motor properties. Instead, primitive definitional components represent components of the senses of expressions, these senses serving to pick out type reference.

Another severe weakness of TSP that makes it vulnerable to the claim that it offers no connection between language and the world is that TSP, as represented by FGWP, makes use of lexical definition, or definitions that are simply paraphrases in the language. But clearly for a semantic theory within the framework of generative grammar what we need is an explicit, theoretical notion of definition, one which "defines terms on the basis of constructs from linguistic theory."[9] Lexical definition, which takes us from the language back into the language, gets us nowhere.

This notion of theoretical definition allows us to make a transition at this point to a discussion of another

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[9]Katz, Semantic Theory, p. 244.

of the claims of TSP rejected by FGWP, namely claim (5), which concerns the existence of analyticity. Katz presents the distinction between lexical and theoretical definition in connection with Quine's criticism of the analytic-synthetic distinction. Although FGWP do not discuss claim (5) at much length, mentioning it only in a footnote,[10] they do cite the work of Quine and Putnam as thoroughly discrediting this claim. We will therefore turn at this point first to Quine's and then to Putnam's criticism of analyticity to see whether, in fact, they have succeeded in thoroughly discrediting it.

What Quine's criticism of analyticity amounts to is an argument intended to show that the intuitive notion of propositions that are true only because of their meanings is a phantom, an illusion. He claims to demonstrate this by showing that given all the available means of defining analyticity, we cannot arrive at an adequate, noncircular definition. Rather, we are forced into appealing to notions like snynonymy or necessity that are equally vague and in need of definition themselves, or to definition by arbitrary stipulation which, by its very arbitrary nature, offers no genuine account of analyticity.[11]

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[10] Fodor, Garrett, Walker, and Parkes, p. 265.

[11] Willard Van Orman Quine, From a Logical Point of View (New York: Harper and Row, Inc., 1953), pp. 20-46.

On the basis of such considerations Quine concludes that there is no such thing as an analytical proposition, one that is true no matter what only on account of its meaning. In fact, Quine asserts that even the logical laws are vulnerable to the vicissitudes of experience. In place of a notion of scientific knowledge with necessary truths, Quine suggests an image of scientific knowledge as a "field of force whose boundary conditions are experience," and whose propositions are all subject to revision based on such experience, with those propositions on the periphery of the field being more vulnerable to such revision than propositions closer to the center of the field. Quine says that given certain disconfirming experiences "truth values have to be redistributed over some of our statements."[12]

Katz has replied to Quine's criticism of analyticity and offered a criticism of his own against Quine's view of scientific knowledge.[13] Together Katz's reply and his criticism can be seen as affirming the existence of analytical propositions.

Against Quine's criticism of analyticity, Katz presents the notion of theoretical definition.[14] In so far

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[12]Ibid., p. 78.

[13]Jerrold J. Katz, "Semantics and Conceptual Change," The Philosophical Review 88 (1979), pp. 335-41.

[14]Semantic Theory, pp. 243-61.

as a theory of meaning is concerned, Katz's notion of theoretical definition can be seen as facilitated by Chomsky's development of the conception of a generative grammar constructed to account for the linguistic knowledge of the ideal speaker-hearer of a language. Katz notes that Quine's skepticism is based on the view that taxonomic substitution tests exhaust the possible sources of a definition of analyticity. But a generative grammar offers a new solution that does not rely on substitution tests.[15] Such a generative grammar is based on the linguistic intuitions of native speakers regarding the grammatical properties and relations of the sentences and expressions of their languages. It is a formal system that is designed to generate the structures of all and only the well-formed sentences of a language, and to define in terms of such formal structures the grammatical properties and relations attested by native speakers. Such theoretical definition, therefore, offers an explicit grounding for the notions of semantic properties and relations like analyticity, thus avoiding Quine's argument of circularity of definition.

Quine's arguments against analyticity, then, only assert a skepticism about meaning; they do not, given the possibility of theoretical definition, give us reasons for maintaining such skepticism. Moreover, the field-of-

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[15]Katz, Language and Other Abstract Objects, pp. 149-51.

force view of scientific knowledge is, as Katz has pointed out, an incoherent one. For if certain propositions in the interior of this field must be revised on account of a chain reaction resulting from revisions in propositions on the periphery of the field, then we do in fact have a notion of necessity, namely the "must" in "must be revised." But this view of knowledge, as we have noted, claims that there are no necessary truths. Therefore, it claims both that there are necessary truths and that there are no necessary truths, which is incoherent.[16]

Turning now to Putnam's criticism of analyticity we find that this, as Katz points out,[17] is based on Donnellan's insight that the classical theory of reference, from which claim (1) of TSP is derived, did not provide for a notion of meaning being fixed in advance.[18] This is because the classical theory's notion of meaning is essentially extensionalist, so that there is no non-empirical notion of meaning whereby meanings can be fixed in advance. But with an intensionalist notion of meaning whereby meanings are the senses of expressions there is a notion of meaning that allows meaning to be fixed in advance of an assignment of extensions. Therefore, the type

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[16]Katz, "Semantics and Conceptual Change," p. 341.

[17]Ibid., pp. 341-43.

[18]K. S. Donnellan, "Necessity and Criteria," in Rosenberg and Travis, pp. 42-52.

reference/token reference distinction can account for Putnam's robot cat examples. We can say that the sentence type "cats are animals" is analytic on the basis of its meaning, which is determined on the basis of the semantic intuitions of the speakers of a language in a generative grammar set up to predict and explain such intuitions. The meanings of the words fix the analyticity of the sentence in advance. Putnam's robot spy device scenario is irrelevant to this notion of analyticity, which is a property of types, not tokens. Therefore, claim (5) is far from being discredited--in fact, a thorough examination of the arguments involved strongly affirms the existence of analytic propositions.

Before going on to examine the FGWP arguments against claim (2)--that "definitions underwrite the validity of informally valid arguments"--it should be pointed out that what we have been discussing are arguments and issues which are part of a broader debate between intensionalists and extensionalists. The main points in this debate will help in making the transition to our discussion of what FGWP have to say about claim (2).

The main point of the debate between intensionalists and extensionalists is that intensionalists claim that a theory of language must posit a level of representation in grammars at which the senses of expressions in the language are represented. Furthermore, it is on the basis of the representation of the senses of expressions at this

level that the logical and the meaning entailments of the expressions can be worked out. That is, at this level we represent the logical forms of the expressions of the language. On the other hand, extensionalists deny the existence of such a level of representation, and claim that the logical forms of expressions can be specified without appeal to senses or meanings. For the extensionalists, moreover, a theory of meaning is reduced to a theory of reference.[19]

I have already indicated that it is my position that no satisfactory theory of reference is possible without a level at which the senses of expressions are represented. At this point I would like to take up the claim made by FGWP that inference relations of expressions can be satisfactorily handled by using some form of Carnap's meaning postulates, thus avoiding the need for a level of representation at which the senses of expressions are represented. We will leave for the next section their arguments against the idea that definitional constructs can be used to explain the validity of inferences based on the meanings of expressions since these arguments can best be dealt with by looking at semantic markers for the examples involved.

First, it is to be noted that as in previous cases the theory which they set out to argue against is largely

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[19]Jerrold J. Katz, "Logic and Language: An Examination of Recent Criticisms of Intensionalism," in Language, Mind, and Knowledge, ed. K. Gunderson, Minnesota Studies in the Philosophy of Science, vol. 7 (Minneapolis: University of Minnesota Press, 1975), p. 39.

a strawman theory, specifically designed, it seems, so that it is vulnerable to their arguments. In particular, we should note the weaknesses in the following schematic representation of the theory they are rebutting:

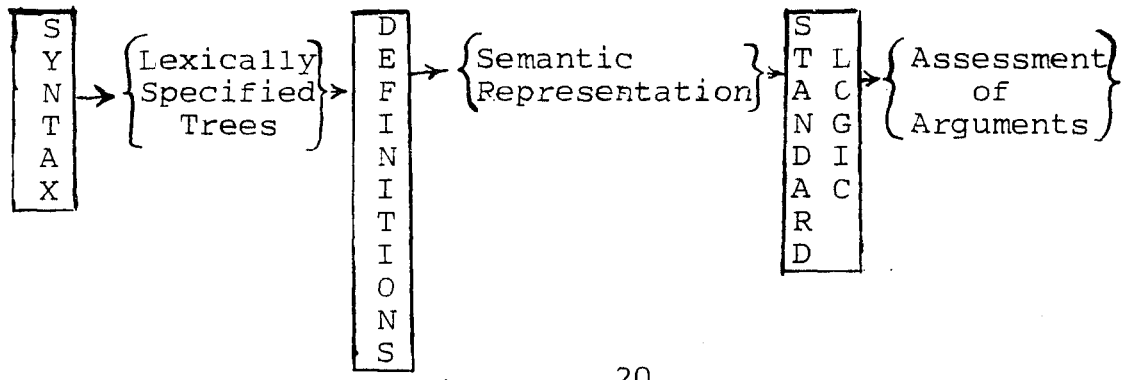

Figure One[20]

I offer instead the following schematic representation of a theory that would distinguish between meaning dependent inference or semantic entailment and inference due to the postulates of standard logic:

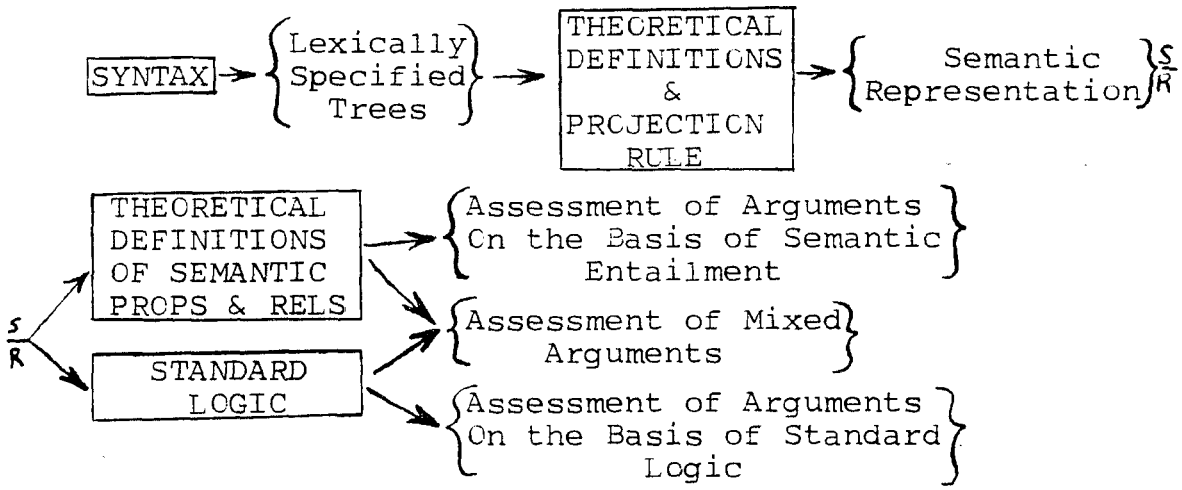

Figure Two

I include their own schematic representation of their

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[20]Fodor, Garrett, Walker, and Parkes, p. 273.

theory, the meaning postulate theory:

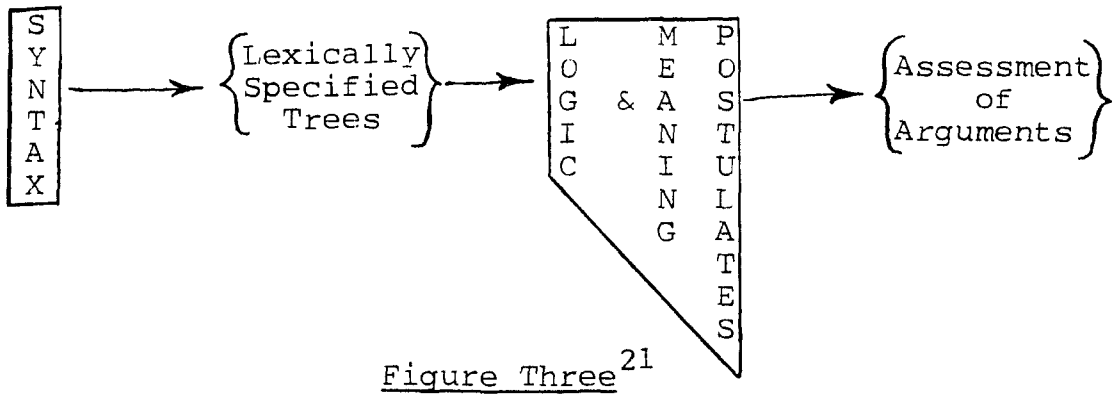

Figure Three[21]

I present above these three schematic representations so that I can point out some necessary corrections in their account of a definitional theory of inference and in order to show clearly one important point of contrast between their theory and ST. First, in my figure two above I make clear that ST uses theoretical rather than lexical definition for dictionary entries as well as for the definitions of semantic properties and relations. Second, I note that in my account of a definitional approach as shown in figure two there are two different ways of assessing the validity of arguments depending on whether or not the inferences are cases of semantic entailment. Finally, I note the important distinction between their theory and ST as displayed in figures three and two that whereas their theory is forced to represent all implications as being of one type, ST, as just pointed out, makes a distinction between semantic entailment and implication due to some

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[21]Ibid..

postulate from standard logic. I note that it is not open to FGWP to claim that their theory could make the same distinction by distinguishing between implications due to meaning postulates and implications due to the postulates of standard logic. Such a position is not open to them because the distinction between meaning postulates and postulates in standard logic would not be motivated by any difference in formalism, meaning postulates being only designations of meaning relations using the formalism of standard logic.[22] In other words, any such distinction that a theory of their type would attempt to make would be purely ad hoc and non-explanatory. On the other hand, such a distinction falls out naturally from a semantic marker theory since its theoretical definition of semantic entailment is distinguished by its formalization from the postulates of standard logic. Katz points out that the two theories thus make different claims about the nature of implications, and that, therefore, evidence can be made to bear on the question of the relative adequacies of the two theories. He argues:

> either PCT (Predicate Calculus Theory or the meaning postulate approach) denies the distinction between intraconstant and interconstant logical structure and claims that the ground of every implication is definitional in the sense that what needs to be said about the logical forms of the two propositions is what is required to determine whether one implies

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[22]Jerrold J. Katz, "The Advantages of Semantic Theory Over Predicate Calculus in the Representation of Logical Form in Natural Language," New Directions in Semantics, The Monist 60, no. 3 (July 1977), p. 395.

> the other, or else PCT must remain neutral on this distinction and confine itself to marking implication as such.[23]

The empirical problem then reduces to finding non-definitional implications, or implications between propositions where their respective logical forms are not sufficient to ground the implication. As a range of examples of non-definitional implications Katz cites "implications that turn on the transitivity of relations expressed by comparative constructions." His example is as follows:

5. Sue is {smarter than / as smart as} Moe and Moe is {smarter than / as smart as} Lem.

6. Sue is {smarter than / as smart as} Lem

Katz points out that a meaning postulate theory can predict the implication from 5 to 6 only on the basis of the following meaning postulate:

7. $(x)(y)(z)((Sx,y \ \& \ Sy,z) \ \ Sx,z)$.

Katz further points out, however, that relying solely on such a meaning postulate falsely predicts that the logical form of 6 is deducible from the logical form of 5 independently of any logical principles. But nowhere in 5 are Sue and Lem arguments of the same predicate. That is, the logical form of 5 does not by itself sanction the implication to 6. Instead, to arrive at 6 we need 7 and the logical representation of 5, and we can then derive 6 by modus ponens. In other words, the implication of 6 by 5

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[23] Ibid..

is a result of what Katz calls interconstant logical structure rather than solely intraconstant logical structure.[24]

Unlike the meaning postulate theory, a semantic marker approach succeeds in explaining different types of implication as a function of the logical structures of the propositions involved. Why is this the case? Why is it that the meaning postulate approach fails at this level of adequacy? The answer appears to be that the predicates of meaning postulates lack internal structure, so that they do not truly represent intraconstant logical structure. Let us look at a possible meaning postulate for one of the examples that FGWP cite in their arguments against definitions:

8. $(x)(Red_x \rightarrow Colored_x)$

This meaning postulate does not tell us what the internal structure of the concept "red" is--it only represents, in a purely ad hoc fashion one of the implications of "red." The predicate elements on either side of the logical connective are atomic. Even if we had a complete list of meaning postulates for "red," we would still not know what the internal structure of the concept red is. It is because of this lack of internal structure that meaning postulates fail to capture the distinction between semantic and what might be called logical implication.

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[24]Ibid., pp. 395-96.

A further consequence of this failure to display the internal structures of concepts is that meaning postulates do not explain the entailment relations that they mark. This is the essence of Quine's criticism of meaning postulates when he says, concerning a hypothetical recursive enumeration of the analytic statemants of a language L on the basis of Carnap's semantical rules, that "we understand what expressions the rules attribute analyticity to, but we do not understand what the rules attribute to those expressions."[25] That is, meaning postulates are simply not explanatory. For example, the postulates (9) to (11) below say that "bachelor" entails "unmarried," "male," and "adult." However, these meaning postulates do not explain that the reason why "bachelor" has these entailments is simply because the concept bachelor contains each of these entailed concepts. Meaning postulates do not offer an account of what it means for one expression to entail another by virtue of meaning alone.

9. $(x)(\text{bachelor}_x \rightarrow \text{unmarried}_x)$

10. $(x)(\text{bachelor}_x \rightarrow \text{male}_x)$

11. $(x)(\text{bachelor}_x \rightarrow \text{adult}_x)$

On the other hand, a semantic marker theory provides a formal definition of the entailment relation independently of a particular set of expressions or a particular

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[25]Quine, From a Logical Point of View, p. 33.

language. That is, ST meets the requirement for an account of entailment--as well as for other semantic properties and relations--that Quine says, in the following statement, is needed for an understanding of the notion of analyticity:

> In short, before we can understand a rule which begins 'A statement S is analytic for language L if and only if . . .', we must understand the general relative term 'analytic for'; we must understand 'S is analytic for L' where 'S' and 'L' are variables.[26]

What Quine is describing here is the basic notion of explanatory adequacy. ST satisfies this notion of explanatory adequacy by defining semantic properties and relations in terms of configurations of semantic representations in the theory. On the other hand, a meaning postulate theory, as Quine points out, fails this requirement of explanatory adequacy.

Now a meaning postulate theory might define other semantic properties and relations like contradictoriness, analyticity, anomaly, synonymy and so on in terms of the entailment relation marked by meaning postulates. But since, as we have just argued, meaning postulates do not explain such entailment relations, meaning postulate definitions of such semantic properties and relations are themselves ultimately non-explanatory. For example, the meaning postulate definition of synonymy will amount to saying that two expressions are synonymous just in case

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[26]Ibid..

they have the same set of entailments. But, in fact, this is a consequence rather than the essence of synonymy. Meaning postulates, therefore, are inadequate, not only because they do not explain semantic entailment, but also because they define every other semantic property and relation in terms of this unexplained relation.

Semantic Markers and their Ability to Account For Semantic Entailment Relations

In the last section I pointed out that one of the crucial distinctions between the semantic markers of Katz's theory and the predicate symbols in the meaning postulates of FGWP's theory is that while the former can have internal structure, the latter do not. We might say that while semantic markers are descriptions of the concepts they represent, the predicates occurring in meaning postulates are merely designations.[27] We saw that because of this difference semantic markers could but meaning postulates could not mark different types of implication differently. In addition, the semantic marker theory explains speaker-hearer intuitions concerning the semantic properties and relations of sentences such as meaningfulness, ambiguity, redundancy, anomaly, contradictoriness, synonymy, and so on. On the basis of explaining in a non-ad hoc manner such speaker-hearer intuitions regarding the semantic aspects of a language, the semantic marker theory can make

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[27] Katz, "The Advantages of Semantic Theory . . .," pp. 384-85.

claim to psychological reality. We again note that it is precisely because semantic markers have structure that the theory succeeds in this way.

It is part of TSP that definitions in this theory do not have hierarchical structure. We are forced to come to this conclusion about TSP definitions because otherwise the arguments given against the idea that definitions explain semantic entailment would not have any force.

According to FGWP one of the main considerations against this idea is that "it is by no means certain that all informally valid arguments will be revealed as formally valid (as subsumed by the inferential apparatus of standard logic) even if couched at the (putative) level of semantic representation."[28] Looking back at figure one, their representation of a definition type theory, we can see why this inability to reduce informally valid arguments to formally valid ones would be a serious problem for such a theory. It would be a problem because according to this theory all assessments of validity of implication are made by standard logic. But if we look also at figure two, my representation of how ST accounts for implication, we see that rather than being a problem this would be one of the expected consequences of such a theory. The reason for this is that in this theory assessments of the validity of implications are made by two different sets of mechanisms marking two

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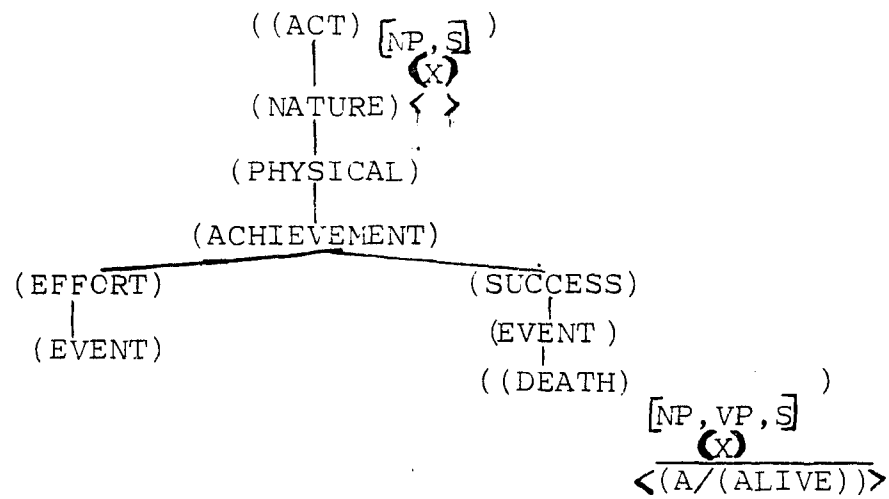
[28]Fodor, Garrett, Walker, and Parkes, p. 270.

different types of logical relations. Validity of semantic entailment, an intraconstant logical relation, is assessed by the definition of semantic entailment in ST. Inferences that must depend on the postulates of standard logic are assessed by the postulates of standard logic. To reduce one type of relation to the other by marking them in the same way would rob the theory of its ability to mark these two different types of logical relations differently.

So we see that one reason given by FGWP for rejecting a definitional approach is instead a reason in its favor. In addition, the example they give in support of this argument will help to illustrate how ST handles semantic entailment. The example is as follows:

12. "John killed Mary" entails "Mary died."

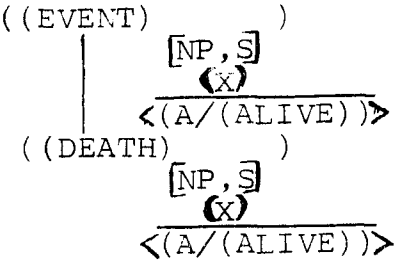
The semantic marker for kill might be represented in the following manner:[29]



[29]Katz, Propositional Structure and Illocutionary Force, p. 72. EVENT has been added below SUCCESS.

This semantic marker tels us that the concept of the verb "kill" expresses a physical act of achievement. The concept of achievement is broken up into effort and success. Here the effort is an event and the success is the death of the referent of the term catergorized as the object of the verb. The semantic roles of agent and recipient are defined in the theory as a function of semantic markers or readings and that in accordance with these definitions the agent would be the referent of the subject of the verb and the recipient would be the referent of the object of the verb.

We might represent the semantic marker for the verb "die" as follows:



We might add here a definition of an additional semantic role that seems to be needed: that of the patient reading:

R is the patient reading in the sentence reading Rs=df.
(a) the part of Rs that represents the propositional content of the sentence S contains a semantic marker with a branch of the form:

(EVENT) $((M_{i_1}) \ldots (M_{i_s}))—(M_{j_1})—(M_{j_2})-\ldots(M_{j_n})$

(b) R is the reading $(M_{i_1}) \ldots (M_{i_s})$ [30]

The need for such a semantic role, which we have called the patient, may be seen in the following examples:

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[30] This definition follows the form of Katz's definitions of the agent and recipient readings in Katz(1977), pp. 84-85.

13. The stockmarket collapsed.
14. The man fainted.
15. A baby was born.

The subject NPs in these sentences are clearly not agentive, and we cannot include them under some broader definition of a recipient reading since no act is involved.

It should be clear from the semantic markers for the two verbs "to kill" and "to die" as given above that a derived reading for the sentence "John killed Mary" will include the derived reading for the sentence "Mary died" in a manner that will allow the definition of semantic entailment in ST [31] to predict the entailment relation cited in 12 above.

We note that the recipient reading in "John killed Mary" becomes the patient reading in "Mary died." We might expect the same thing to happen with similar pairs of sentences such as the following:

16. "John broke the glass" entails "the glass broke."

This is a direct consequence of the theory, and in so far as it reflects our intuitions about the shift in semantic roles involved, it demonstrates the explanatory adequacy of such a theory.

The second argument used against the idea that a definitional account can be given for semantic entailment is the FGWP claim that "there appear to be some informally

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[31] Katz, "The Advantages of Semantic Theory . . .," pp. 390-91.

valid arguments which cannot be reconstructed by appeal to the definition relation." In support of this claim they cite the following example:

17. "X is red" entails "X is colored."

However, their reason for believing that this implication "cannot be reconstructed by appeal to the definition relation" is simply that their notion of a definition is that of a simple conjunction of definitional elements. We see this clearly in their comments leading up to this example:

> Suppose that "bachelor--unmarried" is valid in virtue of the definition of "bachelor." Then we can be sure that there will be some predicate P (in fact, the predicate "man") such that "unmarried & P--bachelor" is also valid. Quite generally, if an informally valid argument turns on a definition, then there will be some clause that can conjoin to the consequent which will make the corresponding biconditional true.[32]

But this argument does not take into consideration that definitions are hierarchical structures of concepts rather than mere conjunctions of concepts.[33] To illustrate this distinction we may look at some possible configurations of semantic markers involved in their example.

We might propose the following semantic marker for the adjective "red":

(COLOR)
N,NP/NP,S
X
(A/(ABSTRACT OBJECT))
(RED)

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[32] Fodor, Garrett, Walker, and Parkes, p.271.

[33] Janet Fodor also errs: Semantics, p. 150.

What this semantic marker says is that the word "red" predicates the concept COLOR with the particular value RED. It should be clear that with this semantic marker, given an appropriate marker for the verb "to be," ST will correctly predict that "X is red" entails "X is colored."

We note that the reason why semantic markers can account for such implications is simply that they are highly structured constructs and not simply conjunctions of elements. That they are highly structured reflects the notion that the concepts they represent are equally highly highly structured, the semantic markers being intended to mirror the structures of the concepts. While the particular form of this semantic marker may be shown to be in need of revision, it is clear that some similar marker will be able to provide us with what we need to mark the implication relation cited.

Underlying the FGWP claim that implications such as (16) cannot be handled on a definitional theory is the opinion that there are many words that cannot be given a definition that specifies necessary and sufficient conditions for reference. We have already cited Katz's distinction between type and token reference and his revision of Frege's notion that sense determines reference to one which says instead that sense determines type reference or the reference of linguistic types. Katz has suggested that the need for a separate system for token reference can be understood in part as being due to pragmatic factors, the

senses of words being often too abstract for the purpose of picking out reference during the on-line processing of sentences.[34] It seems to me that the abstractness of the senses of words is also responsible for the mistaken conclusion that some words cannot be given adequate definitions.

For example, in his book Representaticns Fodor suggests that his own failed attempts to construct a definition for the verb "to paint" is evidence, first, for the non-existence of an adequate definition of this verb, and, furthermore, evidence that many other words may similarly lack definitions.[35] But clearly failure to find a fully adequate semantic analysis of a word does not constitute an argument that an adequate analysis is not possible. Anyone who has taught introductory syntax to a bright undergraduate knows that one will also fail to find a fully adequate syntactic analysis of any standard, twenty-word English sentence. But no one would take this failure as grounds for saying that syntactic analyses are not possible. Fodor's argument fundamentally misunderstands science. Science does not try to give complete analyses of particular objects, but rather looks for general laws on the basis of partial analyses.

Moreover, in this matter semantic analysis should be

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[34]Katz, "The Neoclassical Theory of Reference," pp. 115-17.

[35]Fodor, Representations, pp. 285-88.

approached in the same hypothesis-construction fashion with which we approach syntactic or phonological analysis. That is, we simply present the best analysis that accounts for the data that we are aware of at a given time. When such analysis is shown to make incorrect predictions about adddi-tional data not considered initially, we correct our analysis so that it does, in fact, account for the new data. This, it seems, is the generally accepted pattern of development in linguistic analysis. There is no obvious reason why this pattern of theory construction should not apply to semantic analysis in the same manner as it applies to syntactic and phonological analysis.

Moreover, one can give a pretty good approximation to a definition of the sense of "to paint" on which it refers to the activity of covering a surface with paint, namely as provided in figure four on the following page. What this semantic marker says is that the verb "to paint" on this sense signifies a physical activity of causation involving a cause and a result. The cause consists of following a procedure of applying a liquid to the surface of an object with an instrument. The result consists of thereby bringing about a change in condition of the object such that it is covered with a liquid which adheres to its surface.

The claim of this semantic marker is that it accounts for type reference of this verb as well as the semantic properties and relations of the expressions it enters into

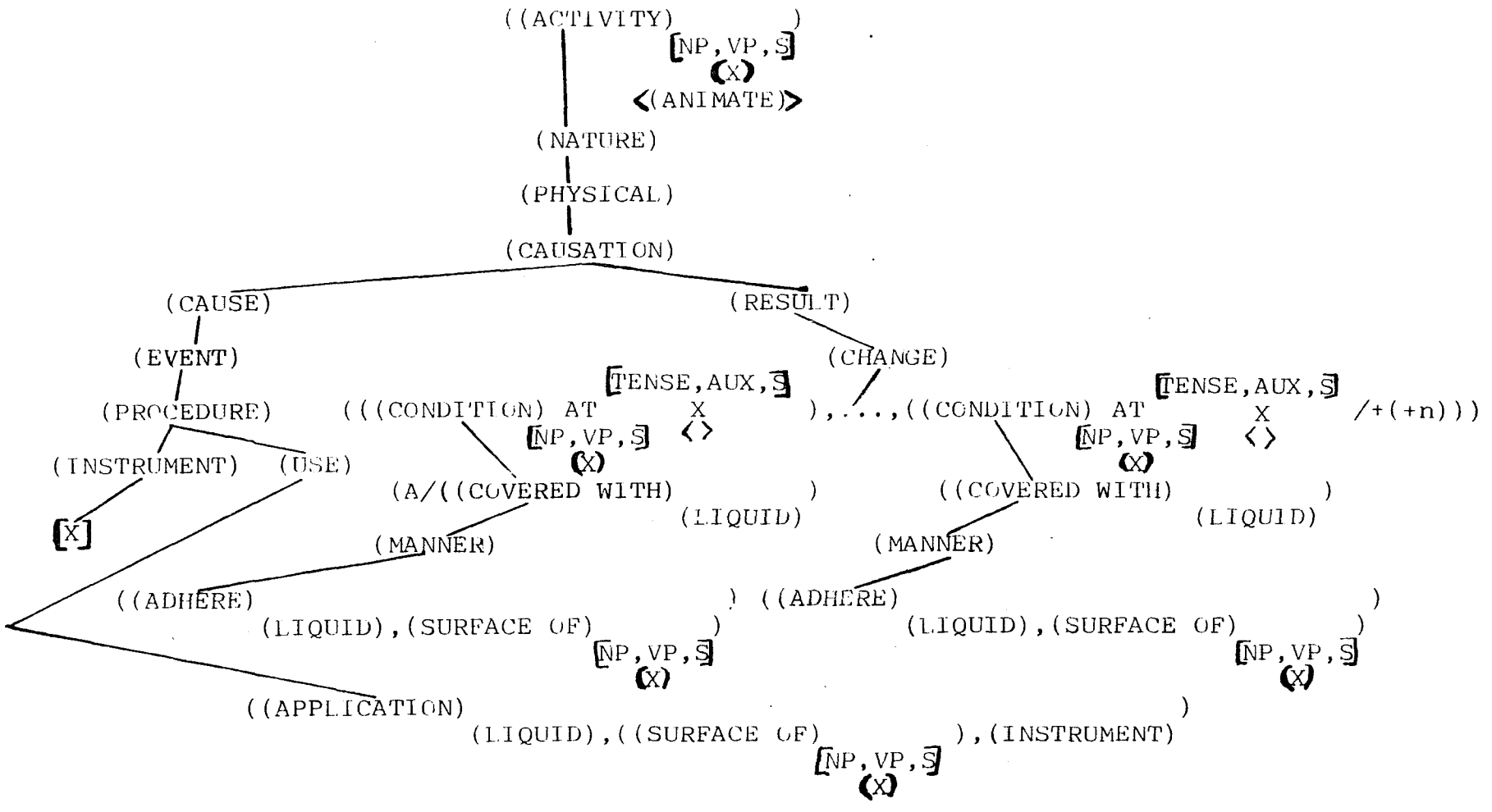
((ACTIVITY))
[NP,VP,S]
(X)
<(ANIMATE)>
(NATURE)
(PHYSICAL)
(CAUSATION)
(CAUSE)
(RESULT)
(EVENT)
(CHANGE)
[TENSE,AUX,S]
[TENSE,AUX,S]
(PROCEDURE)
(((CONDITION) AT X),...,((CONDITION) AT X /+(+n)))
[NP,VP,S] <>
[NP,VP,S] <>
(X)
(X)
(INSTRUMENT) (USE)
(A/((COVERED WITH))
((COVERED WITH))
(LIQUID)
(LIQUID)
[X]
(MANNER)
(MANNER)
((ADHERE)) ((ADHERE))
(LIQUID),(SURFACE OF))
(LIQUID),(SURFACE OF))
[NP,VP,S]
[NP,VP,S]
(X)
(X)
((APPLICATION))
(LIQUID),((SURFACE OF)),(INSTRUMENT)
[NP,VP,S]
(X)

FIGURE FOUR

in the language. Counter examples to this semantic marker would constitute strong evidence against ST only if they could not be handled by suitable revisions.

Psychological Reality and Psychological Evidence

According to FGWP, one of the claims of TSP, the theory they are arguing against, is that "to understand a word is to recover its definition." Although "there might be any number of heuristic procedures for avoiding the recovery of definitions in special circumstances . . .," according to TSP "understanding a sentence token involves recovering the definitions in, as it were, the systematic cases." According to FGWP, TSP claims that "the output of the sentence comprehension system is the semantic representation of the sentence . . .," and "this output in turn provides a domain for such further transformations as logical and inductive inferences, comparison with information in memory, comparison with information simultaneously available from other perceptual elements, etc." Moreover, according to TSP, "some inferences (like 'bachelor--unmarried man') must be drawn on pain of failure to understand the sentence."[36]

These, according to FGWP, are some of the essential claims of TSP. Moreover, there seems to be an implication in their paper that any theory depending on definitions is forced into making such claims. I will show that this is not the case, illustrating my argument by looking at the

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[36]Fodor, Garrett, Walker, and Parkes, pp. 274-78.

semantic marker approach.

To begin with, even if a definitional theory were to claim that understanding a word means recovering the definition of the word, such a theory would not be forced into making the further claim that in understanding a word "some inferences (like 'bachelor--unmarried man') must be drawn." Such a theory could claim that while understanding a word involves recovering its definition, making inferences involves the further step of ascertaining that the definition of analyticity is met by some conjectured inference--for example, "bachelors are unmarried men," That is, given a definition, in order to establish its potential inferences, we must, using the definition of analyticity in the theory, establish what semantic inferences are valid inferences. Therefore, establishing valid inferences involves a step beyond recovering definitions. There is no reason for such a theory to conflate recovering definitions with establishing valid semantic inferences. In fact, such a theory offers a distinction between recovering definitions and establishing valid semantic inferences via the non-application and the application of the definition of analyticity to the inferences we want to test.

But even more importantly, it is not at all clear that a definition theory is forced to claim that understanding a word means recovering its definition any more than a syntactic theory is forced to claim that understanding a sentence means recovering its syntactic structure. The

question of the psychological reality of definitions does not depend on whether definitions are used directly in the on-line processing of utterances. Rather, psychological reality can be established by showing that the theory predicts and explains native-speaker linguistic intuitions. Demonstrating that the theory possesses such predictive and explanatory powers is what is involved in showing that the theory accurately represents the semantic competence of such speakers. The question of the extent to which definitions are made use of in the actual processing of sentences is a question about performance, not competence, and any answer to such a question could not impugn the adequacy of a definitionally based theory of semantic competence.

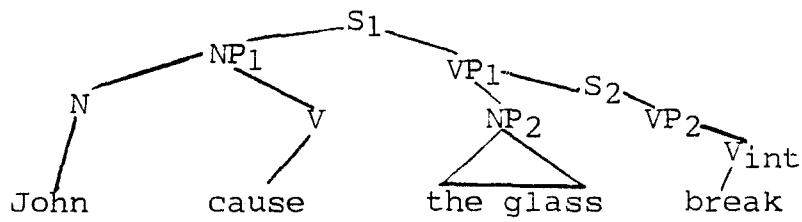
The role of performance heuristics in this competence-performance distinction was mentioned in chapter two where I pointed out that it is the heuristic devices of the performance model and not the rules and representations of the competence grammar that should predict the greater difficulty in comprehending self-embedded as opposed to left-branching sentences. In the light of this view of a processing model the following comments by FGWP are surprising:

> Nobody has to claim that <u>every</u> case of understanding a sentence token involves recovering definitions, even when the sentence contains definable terms; there might be any number of heuristic procedures for avoiding the recovery of definitions in special circumstances. All that has to be claimed is that understanding a sentence token involves recovering

the definitions in, as it were, the systematic cases.[37] These comments seem surprising because at the level of processing, for semantics as well as for syntax, it is the set of heuristic devices themselves that constitute the relevant system. At this level sentences that are not available to comprehension through the use of such devices make up the unsystematic cases. That is, we might hypothesize that grammatical competence might be appealed to when the heuristic devices, which constitute the way in which we normally process sentences, fail to render a meaning for a sentence. That is, it is precisely in the unsystematic cases that we might expect definitions to be recovered, not in the systematic ones as claimed by FGWP.

We now turn to the psychological experiment carried out by FGWP, the results of which they claim constitute evidence against the psychological reality of definitions.

Their experiment makes use of causative constructions and turns crucially on such a structure, supposedly representing a definitional account of the semantic representation of the sentence "John broke the glass," as follows:[38]



[37] Ibid., p. 275. [38] Ibid., p. 285.

In their paper, FGWP recognize that generative semantics and interpretive semantics differ in some ways. However, they seem to contend that the above structure or something very much like it is an acceptable account of the semantic representation of the sentence in an interpretive semantics theory.[39] But this is simply not the case. The structure presented here represents well the conflation of syntax and semantics characteristic of a generative semantics grammar. Such structures simply do not exist in interpretive semantics grammars as described by Katz. It is precisely the conflation of semantics and syntax represented by such a structure and rejected by interpretive semantics that allows FGWP to make the following claims:

> Consider-- "John" and "the glass" in "John broke the glass." These items are related as subject and object of the verb "break" in the surface sentence . . . ; but they are not so related in the putative semantic representation. In fact, on the definitional analysis, there is no verb of which "John" and "the glass" are both arguments at the semantic level. Rather, "John" turns up as the subject of the underlying verb "cause," and "the glass" turns up as the subject of the underlying verb "$break_{intransitive}$." Another way of putting this is as follows: given what is presumably the intended interpretation of the logical syntax, "John broke the glass" does not express a relation between John and a glass. On the contrary, it expresses a relation (of causing) between John and an event (viz., the glass breaking). Similarly, according to the definitional analysis, "John killed Mary" does not express a relation between John and Mary . . . etc.[40]

This talk of relations, from the point of view of interpre-

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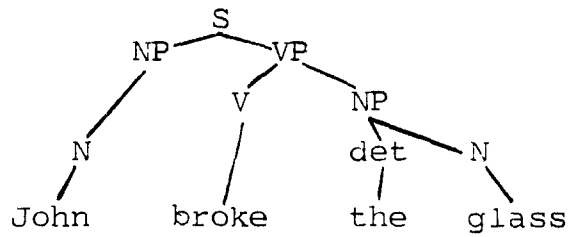
[39] Ibid.. [40] Ibid., p. 286.

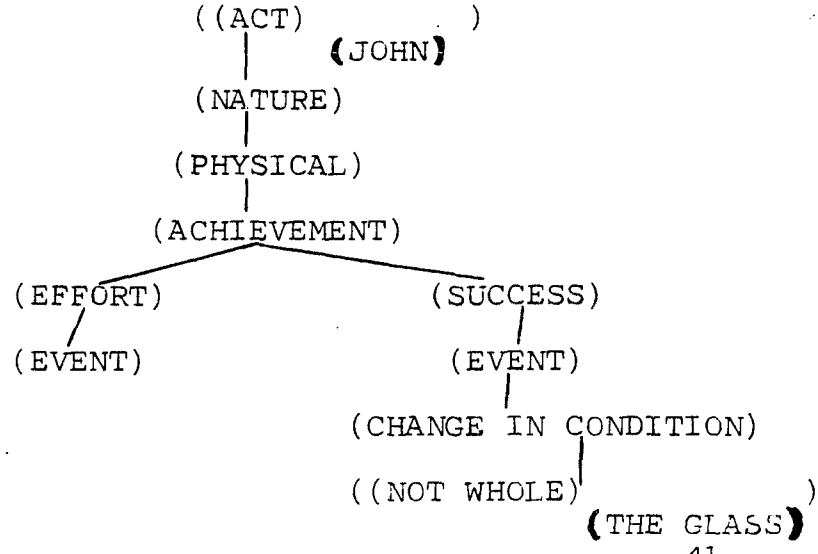
tive semantics, is extremely confusing. Are we talking about syntactic relations or semantic relations? The question of whether elements of a complex semantic marker bear such syntactic relations to each other as subject of the verb or object of the verb is not a meaningful question since such semantic markers are intended to show semantic rather than syntactic relations. It is only in generative semantics that we can thus conflate syntactic and semantic relations. The fact of the matter is that in an interpretive semantics account of such sentences as "John broke the glass," "John killed Mary," etc., it is never the case that "John" is ever anything but--in terms of grammatical (syntactic) relations--the subject of the verb, and "the glass", "Mary," etc., anything but the objects of their respective verbs. Moreover, throughout the account of their experiment, this confusion between grammatical relations and semantic roles remains prominent, and I will argue that their experimental results do not weigh against a theory with semantic marker definitions--i.e., ST--even when taken as being concerned with semantics.

For sake of illustration I show here the ST account of "John broke the glass." The deep structure and the surface structure are the same:



Of course, the grammatical relations do not change in going from deep to surface structure.

The derived reading for the sentence might look somewhat as follows:



Note that the relevant definitions of semantic roles[41] will tell us that "John" has the semantic role of agent of the act, and "the glass" has the semantic role of recipient of the act. These semantic roles are, of course, quite different from the grammatical relations of subject and object. However, the fact that in this case these two pairs of relations are parallel, the subject expressing the sense of the agent and the object the sense of the recipient, ensures that no legitimate claim can be made that relations have shifted in going from the surface syntactic to the definitional semantic levels as stated by FGWP.

This same confusion between grammatical and semantic

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[41] Katz, Propositional Structure and Illocutionary Force, pp. 84-85.

relations carries over into the methodology of FGWP's experiment. They make use of an approach described by Willem Levelt in a series of papers.[42] In "A Scaling Approach to the Study of Syntactic Relations" Levelt describes a process that, simplified, goes somewhat as follows. Subjects are presented with a sentence and asked to rate the relatedness of pairs of words in the sentence with respect to some scale. From such ratings of relatedness a symmetrical matrix having as many rows and as many columns as there are words in the sentence, is derived. Using the clustering algorithm described in the paper "Hierarchical Clustering Algorithms in the Psychology of Grammar," a tree representation of the hierarchy of relatedness ratings can be derived. Such experiments carried out by Levelt showed that the trees representing the hierarchy of relatedness ratings corresponded to the deep structures of the sentences,[43] a remarkable result seeming to support claims for the psychological reality of deep structures.

Now it is clear that Levelt interprets his results as supporting claims for the psychological reality of syntactic structures, the deep structure, even though being the syntactic structure closest to exhibiting the meaning

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[42]Levelt, "Introduction--Hierarchical Clustering Algorithms in the Psychology of Grammar," and "A Scaling Approach to the Study of Syntactic Relations," in Advances in Psycholinguistics, eds., B. F. D'Arcais and W. J. M. Levelt (New York: American Elsevier Publishing Co., 1970), pp. 101-21.

[43]Ibid., pp. 112-20.

of the sentence in the standard theory, nevertheless remaining a level of syntactic description. That is, in ST there is a clear distinction between syntactic and semantic description, and the level that Levelt takes his experiments as being about is not a semantic level but a syntactic one.

But FGWP use essentially the same methodology as Levelt. That is, they ask subjects to rate the relatedness judgements of words in sentences.[44] They then proceed to claim that the results constitute evidence concerning the psychological reality of semantic structures. We can only conclude that the question of whether the results of this experimental methodology should be taken as bearing on syntactic or semantic structure within the grammar, or on something else is left unanswered.

I am not claiming that Levelt's scaling approach cannot be applied to the question of the psychological reality of semantic structures. In fact, it is noteworthy that in one of his papers cited by FGWP Levelt asks the following questions:

> Apart from syntactic structure, are there other domains of cognitive psychology where hierarchical representations are preferred to spatial ones? For instance, what about word meanings? Are meaning relations not often determined by semantic hierarchies like "physical object"-"living"-"animate"-"human"-"male" etc?[45]

Of course, my answer to Levelt is "Yes"--meaning relations

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[44]Fodor, Garrett, Walker, and Parkes, p. 317.

[45]Levelt, p. 110.

are determined by semantic hierarchies. And perhaps we can use Levelt's methodology or some modified form of it to investigate the psychological reality of semantic markers. For example, we could present subjects with pairs of words or sets of expressions in isolation, not in sentences, and ask them to judge their relative relatedness. Of course, such pairs of words or sentences would have to be chosen so that they are related in some theoretically interesting way. In connection with this matter, one glaring omission of the FGWP paper is that it neglects to mention earlier work done by George Miller using a similar relatedness type of experimental procedure, namely sorting. Using the hierarchical clustering algorithm used by Levelt, Miller obtained results, regarding nouns, which, though tentative, tended to support Katz's definitional approach, as Miller points out in his article.[46] The fact that Levelt, in a work cited by FGWP, refers to this article by Miller as providing evidence for hierarchical structure in semantics makes one wonder at the reason behind this omission. I will leave a closer examination of Miller's work for the next chapter.

Summary

TSP is a strawman definitional theory that is vulnerable to arguments to which ST, the more sophisticated definitional theory defended here, is not vulnerable. Moreover,

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[46]George Miller, "A Psychological Method to Investigate Verbal Concepts," Journal of Mathematical Psychology 6 (1969), pp. 169-191.

the experimental results described by FGWP do not weigh against the definitional ST approach.

CHAPTER IV

THE PSYCHOLOGICAL EVIDENCE

In this chapter I would like to consider the psychological evidence relevant to the question of the validity of definitions as part of a theory of the psychological representations of the meanings of words. I will divide the discussion into two parts mirroring the two large areas of psycholinguistic research, first dealing with evidence in adult semantic performance and then with evidence from concept development. Before dealing with the evidence, however, I would like to consider two important requirements for judging the significance of psychological evidence for making claims regarding the nature of the psychological representations of the meanings of lexical items. The purpose of making these requirements explicit will be just so that we can attempt to evaluate the evidence brought forward.

A Basis for Judging the Evidence

First, it should be clear that the experiments I am about to describe are experiments that set out to test a particular hypothesis, and that the hypothesis makes fairly specific claims about the nature of something. Our first requirement is that the experimental methodology should be

such that it provides good reason to believe that the results of the experiment are in fact about the thing in question, and not about something else. In order to satisfy this "aboutness" requirement, experiments that rely on indirect evidence must rely on the manipulation of certain discrete variables and an examination of the effects such manipulation has on the experimental results. The variables that are deliberately manipulated by the experimenter are called the independent variables and the variables that are affected by such manipulation are called the dependent variables. The experiment must show a clear relation between the independent and dependent variables--in particular, the independent variables must be carefully isolated from accidental variation that could be claimed to be responsible for the outcome of the experiment.

The second requirement for evaluating the psycholinguistic experiments relates to the competing hypotheses that the experiment is intended to test. This requirement is simply that the alternative hypotheses should make different predictions about the experimental results so that the results can be the basis for deciding between the hypotheses. For convenience we may call this the "decidability" requirement. Again it seems a fairly general and basic requirement for experimental evidence.

Experiments on Adult Semantic Performance

So far I have mentioned three sets of evidence--FFG's, FGWP's, and Miller's. I would like to discuss each

of these sets of evidence, as well as some more recent evidence from a study by G. Gergely and T. G. Bever which directly challenges the FGWP conclusions. [1]

First we will consider again briefly the FFG and FGWP evidence in relation to the requirements of aboutness and decidability. Regarding the FFG intuitive evidence we may say that the subjects' intuitions--or rather, their introspections--were not about the relative complexity of the mental representations of the meanings of the relevant verbs but about some aspect of the accessing routine. In particular, I suggested that word meanings might be accessed as units so that their internal complexity would not correlate with relative comprehension difficulty.

Concerning FFG's experimental evidence, I argued in effect that the evidence may be interpreted as being about one aspect of the structures of the mental representation of word meanings, but with a definitional theory making different predictions from those assumed by FFG, the complexity of PDNs functioning to hide the negative components of their meanings. I argued that my distance hypothesis D offered a more principled account of the evidence than their non-decompositional theory since it did not need to appeal to the syntactic reflexes of implicit negatives to distinguish them from PDNs or to account for their place

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[1] G. Gergely and T. G. Bever, "The mental representation of causative words," forthcoming in Cognition.

on the scale of difficulty as measured by reaction times. That is, the argument for my hypothesis over theirs can be seen as based on principles of parsimony and naturalness. It is based on parsimony since I do not need to appeal to syntactic notions in the account of implicit negatives but only to semantic ones. It is based also on naturalness because the idea of the relation between the distance separating elements in a linguistic description and the degree of their interaction is one that we find in phonology as well as in syntax. Thus, in Topics in Phonological Theory Kenstowitz and Kisseberth argue against the naturalness of the infinite schema approach of SPE to the phenomenon of vowel harmony and in favor of an iterative rule application approach partly because the infinite schema approach abbreviates rules some of which say that a vowel has an effect on another vowel which can be many syllables removed.[2] What Kenstowitz and Kisseberth are saying seems to imply that distance is a natural consideration in grammatical rules. In addition, as I mentioned in Chapter II, Chomsky's syntactic principle of locality can be seen as another instance of the use of distance in linguistic rules.[3] I am suggesting that the distance hypothesis D of Chapter II is a similarly natural notion that fits well with

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[2]Michael Kenstowicz and Charles Kisseberth, Topics in Phonological Theory (New York: Academic Press, 1977), pp. 186-87.

[3]Chomsky, Rules and Representations, p. 91.

linguistically theory generally.

Turning now to FGWP's experimental evidence, my argument there can be taken in part as saying that the experiment does not meet the decidability requirement with respect to a semantic marker hypothesis since such a hypothesis does not entail the type of grammatical shift that is the source of FGWP's claims against a definitional theory. That is, the semantic marker and non-decompositional hypotheses do not in fact make different predictions about the experimental results, if, as Levelt thought, such experimental results reveal the deep structures of sentences. Moreover, if Levelt's belief is brought into doubt and it can be shown that the results of such relatedness judgements experiments do bear on semantic notions, then the prediction of the semantic marker hypothesis would be that variation of relatedness judgements would be some function of variation of semantic markers. However, we as yet do not have a theory of what aspects of semantic structure cause variations in relatedness judgements.

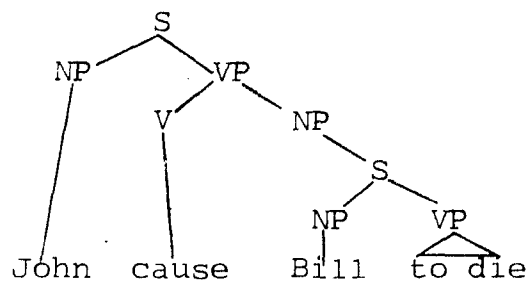
However, the forthcoming paper by Gergely and Bever mentioned above offers both the experimental evidence that relatedness judgements concern aspects of semantic rather than syntactic structure, as well as the beginnings of a theory of what aspects of semantic structure are relevant. FGWP used pairs of sentences like the following to test the

shift in grammatical relations that they thought was a necessary consequence of any decompositional analysis of causative verbs:[4]

1. John kicked Bill.
2. John killed Bill.

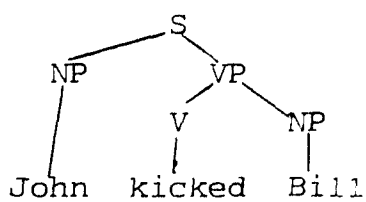
The idea was that sentences like (2) with causative verbs would have an underlying semantic structure like the following:

2'.



In this structure the NP "Bill" is not object of the verb as it is in the surface structure. On the other hand, in sentence (1) in the underlying structure "Bill" remains the object of the verb as it is in the surface structure:

1'.



If, then, relatedness judgements are sensitive to such shifts from surface to deep structure, as FGWP claims their evidence establishes with respect to such sentence pairs as (3) and (4) below,[5] then the decompositional

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[4]Fodor, Garrett, Walker, and Parkes, pp. 284-93.

[5]Ibid..

theory that posits (2') as the underlying semantic structure of (2) predicts that the degree of relatedness between "John" and "Bill" in (2) should be less than the degree of relatedness between "John" and "Bill" in (1).

3. John persuaded Mary to leave.
4. John expected Mary to leave.

Having found that the relatedness judgements do not bear out this apparent prediction of the decompositional theory, that instead the relatedness judgements are the same for the relevant NPs in (1) and (2), FGWP claim that this is evidence against decompositional structure generally.

We have already seen that ST does not posit anything like (2') for the underlying structure of (2) and so does not predict the sort of shift FGWP's evidence supposedly disputes. But, beyond this, if FGWP were correct in claiming such relatedness judgements to be sensitive to this sort of shift in grammatical relations, and if they were also correct in saying that (2) does not have the structure attributed to it by (2'), then there should be a variation in judgements of relatedness between the subject and object nouns in the following pair of sentences:

5. John killed Bill.
6. John caused Bill to die.

However, when Gergely and Bever tested for this they found no such variation in relatedness judgements.[6] This result

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[6]Gergely and Bever, p. 10.

shows that relatedness judgements are not sensitive to such shifts in structure--they are not at all about what FGWP take them to be about.

It may be instructive to go back to our aboutness requirement and examine FGWP's evidence to see what went wrong. FGWP wanted to show that relatedness judgements, while sensitive to structural shifts, were not sensitive to structural shifts supposedly inherent in a decompositional semantics hypothesis. So they assumed that the independent variable in their experiment would be the property of sentences having or not having an underlying structural shift from surface structure. So, in test pairs like (3) and (4) they were careful to keep what they assumed to be the crucial other variable, surface syntactic structure constant. However, in test sentences like (3) and (4) FGWP ignored another crucial variable, namely meaning. That is, while the surface syntactic structures of (3) and (4) are the same, their meanings are quite different. In fact, one crucial aspect of the difference in meaning between (3) and (4) is that (3), but not (4), involves an interaction resulting in a change of intention. Thus, it could be the case that the variation of the dependent variable, the relatedness judgements, are caused by such variations in semantics rather than by variations in structures of the sort proposed by FGWP. Gergely and Bever's evidence, after testing relatedness judgements for the relevant noun phrases in sentences like (5) and (6), where the semantics

is held virtually constant while the underlying structure is varied, establishes that the testing device is not in fact sensitive to the independent variable of underlying structure.

But in order to establish that relatedness judgements are sensitive to aspects of semantic structure rather than to aspects of syntactic structure, it is necessary to have a hypothesis as to what aspects of semantic structure are the relevant ones and to conduct experiments that vary these aspects while keeping syntactic structure constant. This Gergely and Bever did. For example, they hypothesized that the testing device was sensitive to the semantic difference between the definite and indefinite articles. So they tested relatedness judgements for the subject and object NPs in sentences like the following:

7. Harry saw a man
8. Harry saw the man.

They found that, as hypothesized, the relatedness judgements for the NPs in (8) with the definite article were stronger than the relatedness judgements for the NPs in (7) with the indefinite article. This is strong evidence that the semantic aspect of definiteness produces stronger relatedness judgements.[7]

Gergely and Bever also hypothesized that the relatedness judgements would be stronger between two concrete nouns rather than between one abstract and one concrete

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[7]Ibid., p. 12.

noun. They tested this hypothesis with pairs of sentences like the following:

9. The reporter broke the news.
10. The reporter broke the glasses.

Again their results were positive, showing significantly stronger relatedness judgements for the "reporter"-"glasses" pair in (10) than for the "reporter"-"news" pair in (9). This is evidence that the semantic aspect of concreteness produces stronger relatedness judgements.[8]

Gergely and Bever also hypothesized that for sentence pairs with verbs like "expect" and "persuade" the relevant variable affecting the dependent relatedness variable was not underlying structure but the semantic notion of an interaction between the referents of the relevant noun phrases which results in an intentional change on the part of the referent of the object noun phrase.

To establish this intentional change hypothesis over the structural shift hypothesis of FGWP, Gergely and Bever tested relatedness judgements for a number of relevant contrasting pairs. Thus they contrasted pairs of sentences where each member of the pair had the "expect" type underlying structure but where one sentence did not involve such an intentional change while the other did involve such a change. An example of such a pair would be the following:

11. After the plane landed safely, the air controller allowed the assistant to make the phone call.

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[8]Ibid., p. 13.

12. After the plane landed safely, the air controller got the assistant to make the phone call.

While both (11) and (12) have noun phrase complement verbs like "expect," the meaning of (12) does but the meaning of (11) does not involve an interaction between the air controller and the assistant resulting in the assistant changing his intention. Using similar pairs of verbs Gergely and Bever found that the test results indicated that relevant noun phrases in the intentional change members of such pairs were judged to be more related than the relevant noun phrases in the other members of such pairs where there was no change of intention. In addition, sentence pairs in which neither member involved intentional change but in which there was a variation in structure shifting grammatical relations, the relevant independent variable according to FGWP, did not show variation in relatedness judgements. This is evidence that the semantic aspect of an interaction involving intentional change produces stronger relatedness judgements.[9]

Thus, by varying the independent variables of structure and semantics appropriately, Gergely and Bever have established strong evidence that relatedness judgements are sensitive to certain aspects of the semantic structures of sentences rather than to aspects of their syntactic structures. Moreover, Gergely and Bever also account for Levelt's

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[9]Ibid., pp. 15-17.

evidence on the basis of semantics rather than syntax.[10]

The question now is, then, whether Gergely and Bever's evidence and arguments support a decompositional theory of the meanings of lexical items over a non-decompositional one. The question is whether each of the theories is able to mark the relevant semantic properties: definiteness, concreteness, and an interaction bringing about an intentional change. We will look first at how a non-decompositional theory might try to mark these semantic properties.

A non-decompositional theory might say either that these properties are marked superficially or by meaning postulates. One of these alternatives seems possible for definiteness and concreteness, since we could possibly have a meaning postulate with definiteness and one with concreteness on the right hand side. The reason why this seems possible is just that the concepts of definiteness and concreteness can be represented without internal structure. We noted in Chapter III that the components on either side of the material conditional in meaning postulates did not have internal structure, and that this was one of the weaknesses of the meaning postulate theory. But with respect to definiteness and concreteness this does not seem to be a problem since no internal structure is necessary.

But how do you represent the concept of an interac-

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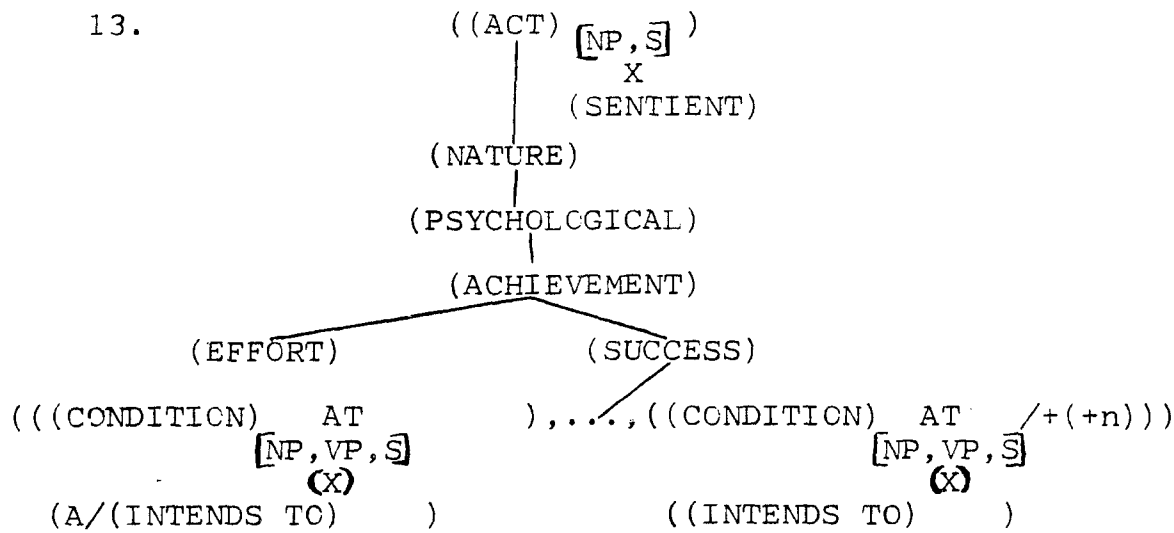
[10]Ibid., pp. 22-23.

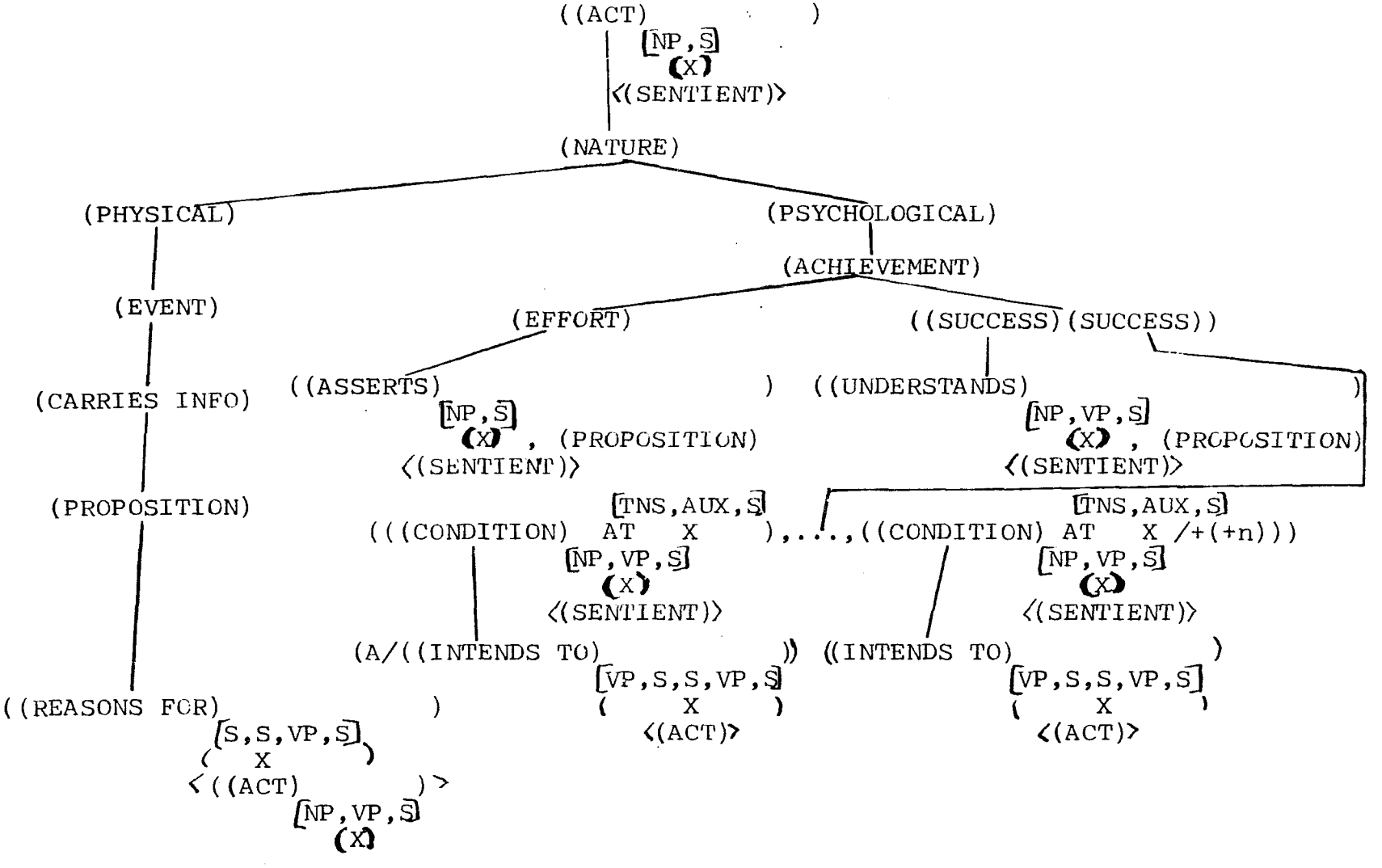
tion bringing about intentional change without internal structure? This, it seems, would be a problem for a meaning postulate theory. It would not, however, be a problem in a semantic marker theory where concepts are represented as having internal structure.

For example, we might propose the semantic marker in figure five on the following page for the relevant sense of the verb "to persuade" on which it involves bringing about a change in intention to do something, adopting it from Katz[11] with some changes by way of expanding the semantic marker (DECISION TO). From this semantic marker we can isolate the part of its structure representing an interaction resulting in an intentional change on the part of the referent of the object NP as the following structure:

13.



[11] Katz, Propositional Structure and Illocutionary Force, p. 164.


((ACT))
[NP,S]
(X)
<(SENTIENT)>
(NATURE)
(PHYSICAL)
(PSYCHOLOGICAL)
(EVENT)
(ACHIEVEMENT)
(EFFORT)
((SUCCESS)(SUCCESS))
(CARRIES INFO)
((ASSERTS))
[NP,S]
(X) , (PROPOSITION)
<(SENTIENT)>
((UNDERSTANDS))
[NP,VP,S]
(X) , (PROPOSITION)
<(SENTIENT)>
(PROPOSITION)
[TNS,AUX,S]
(((CONDITION) AT X),...,((CONDITION) AT X /+(+n)))
[NP,VP,S]
(X)
<(SENTIENT)>
(A/((INTENDS TO)))
((INTENDS TO))
[VP,S,S,VP,S]
(X)
<(ACT)>
((REASONS FOR))
[S,S,VP,S]
(X)
<((ACT))>
[NP,VP,S]
(X)

PERSUADE

It seems, then, that while a semantic marker theory could account for the variation in relatedness judgements found in the evidence of Gergely and Bever on the basis of a semantic marker having a structure like (13), a meaning postulate theory could not predict the evidence, such a theory being unable to represent concepts as having the necessary internal structure. This lack of internal structure seems again to be a crucial factor working against the meaning postulate theory.

Gergely and Bever's experiments satisfy both the aboutness and decidability requirements, and their evidence seems to weigh in favor of a decompositional over a non-decompositional theory of the meanings of words. It seems also that on my account the situation with respect to FFG's relative complexity evidence regarding explicit negatives, implicit negatives, morphological negatives, and PDNs parallels the situation with respect to the Gergely and Bever evidence. We saw that it was the distance hypothesis D that accounted for the evidence on the basis of a semantic marker account of the meanings of the relevant words. Similarly, with respect to Gergely and Bever's relatedness judgements evidence we see that it is a theory of what causes variation in relatedness judgements which accounts for the evidence on the basis of a semantic marker representation. This parallelism suggests that "a coherent theory of how introspections about language are related to lin-

guistic structure"[12] would have to posit the existence of intervening theories that relate different types of introspections about language to linguistic structure in different ways. The difficulty, then, of developing such a "coherent theory" is probably a matter of the gradual combining of efforts such as those of Gergely and Bever with respect to relatedness judgements.

Now we will move on to another major source of evidence in favor of a decompositional account of the psychological representation of word meanings: namely, the work of George Miller. Miller's work is relevant here because it makes use of the same hierarchical clustering scheme used by Levelt in the experiments cited by FGWP. In fact, it is remarkable that FGWP should not feel compelled to offer some account of Miller's work since it is cited by Levelt in connection with the question of the psychological representation of meaning in the Levelt paper referred to by FGWP.

Miller describes a relatedness judgement type of experiment using Johnson's hierarchical clustering scheme, basically the same algorithm employed by Levelt to derive tree structures from relatedness judgements of pairs of words in sentences.[13] However, Miller's experiment involves relatedness judgements of words presented in isolation, where the relatedness of words is

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[12]Ibid, p.25. [13]Miller, pp. 169-91.

derived from a sorting experiment in which subjects are asked to sort words specifically on the basis of their meanings. The experiment described by Miller involved having 50 subjects each sort 48 English nouns, the sorting carried out by putting cards into groups--a noun, a short definition of it, and an example of a sentence using the noun appearing on each card. Each subject was asked to sort the cards into as many piles as he or she wanted on the basis of similarity of meaning.

For each subject the results were translated into a 48 by 48 matrix with the columns and the rows headed by the 48 nouns. If the subject sorted noun i together with noun j in a single pile, cell (i,j) was given the value 1, and otherwise the value 0. The 50 matrices, one from each subject's sorting, were added together to form a single matrix in which cell (i,j) represented the number of subjects who put noun i and noun j in a common pile. This resultant matrix can be considered a similarity matrix and can be converted into a distance matrix using the formula:

$$D_{ij} = N - N_{ij}$$

where N is the total number of subjects, and N_{ij} is the number of subjects putting nouns i and j in common piles--N_{ij} being taken from the similarity matrix.

On the basis of the assumption that what is involved in sorting two nouns together is ignoring those features of the nouns' meanings which distinguish between them and the further assumption that these features form a hierarchy, it

is possible to interpret the distance matrix as encoding a hierarchy. Miller describes the algorithm for making this conversion from a distance matrix to a tree structure in detail, so we will not go into it here.[14] What seems to be important is that in the course of making such a conversion certain discrepancies can arise that can be resolved in either of two ways. By comparing tree structures derived separately in each of these two ways, we can have a basis for deciding whether or not the results reflect a hierarchical structure.

What happens is somewhat as follows. Ideally, if nouns i and j are merged in a tree cluster, then the distance between noun i and noun k should be the same as the distance between noun j and noun k. However, on account of what Miller calls "noise" in the data, this often is not the case. This discrepancy is resolved by constructing a tree taking the maximum distance in all such cases, and then constructing a second tree by taking the minimum distance in all such cases. The method using the maximum distance is called the "diameter method" and that using the minimum distance is called the "connectedness method." Comparing the results of these two methods for degree of similarity becomes a test of whether or not the data really do form a hierarchy. That is, if there is very little or no difference between the trees obtained separately by

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[14] Ibid., pp. 178-79.

the diameter and connectedness methods, then this can be taken as evidence that the data does constitute a hierarchy. On the other hand, if the two trees are very different, then we do not have a hierarchy.

With respect to this comparison test between the results obtained from these two methods, Miller quantified his results as follows:

> The maximally connected scheme contains 41 non-terminal nodes; the minimum diameter scheme contains 43 non-terminal nodes; 29 nodes . . . . represent clusters that are common to both schemes. Thus, about 70% of the clusters indicated by the two methods are common to both.[15]

Miller does say that "whether or not this degree of disagreement is compatible with the assumption that these 48 items represent a hierarchical conceptual subsystem is a matter of individual judgement."[16] However, he does go on to provide arguments from the point of view of the plausibility of the tree structures derived from the results, as well as from the point of view of the relevance of the tree structures to current linguistic theory, citing in particular the then current semantic marker theory of Katz and Fodor (1963). Moreover, Miller also notes that "judgements of similarity shared by more than half the judges seem to arrange themselves the same way according to both methods."[17] In addition, given the high probability that discrepancies were in many cases due to ambiguities in the

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[15]Ibid., p. 181. [16]Ibid.. [17]Ibid..

test items, and slight differences in meanings assigned to given test items by different subjects, the 70% correspondence figure seems significantly high. Miller's evidence thus seems to weigh in favor of the hierarchical interpretation.

With respect to our aboutness and decidability requirements it seems that Miller's experiment is adequate. The aboutness requirement seems to be met both by the nature of the experimental procedure and the high semantic plausibility of the resulting tree structures. I note again that the subjects were asked to sort the cards explicitly "on the basis of similarity of meaning."[18] Miller does note that association might have played a role in some clusterings and that the example sentences might have led subjects to put two items into the same pile on the basis of being able to think of a single sentence using the two nouns. However, such experimental noise--while certainly subject to attempts at minimization, as, for example, by removing the example sentences--seems an inevitable factor in experiments of this type. And the overall plausibility of the resulting tree structures strongly supports the view that the results are indeed about the subjects' mental representations of the meanings of the nouns.

In terms of our decidability requirement, it is hard to see how a non-decompositional theory could predict the

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[18]Ibid., p. 170.

type of hierarchical structure obtained. On the other hand, a semantic marker theory predicts the results directly in terms of the subjects having to ignore certain components in the hierarchical structure of the semantic markers representing the meanings of the nouns. As Miller explains,[19] when a subject uses this method in sorting words, the subject not only ignores a particular component in the meaning of a word, but also all other components dominated by that component. This is why the results, when interpreted by the clustering algorithm, produce a hierarchy. That is, the results reflect an initial hierarchical organization of the words according to features. This initial hierarchical feature organization itself reflects a hierarchical decompositional representation of the meanings of the words. The results can thus be interpreted as reflecting a semantic marker type of representation of the meanings of the words. On the other hand, lacking a system of features hierarchically arranged, the non-decompositional theory of FGWP, with its non-hierarchical meaning postulates, would have to go to great lengths to predict such hierarchical results.

Evidence from Concept Development

There was a time when the decompositional view of meaning had influence in the field of the study of semantic

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[19]Ibid., pp. 176-77.

acquisition. Thus, Susan Carey, in a recent article entitled "Semantic Development: the State of the Art,"[20] describes the work of Eve Clark in which Clark proposed a theory of semantic development which she called the "semantic feature hypothesis" and which Carey describes as "component-by-component acquisition." Carey explains this position as follows:

> The lexical entries in the adult lexicon consist of definitions in terms of semantic components, and these semantic components constitute the primitive base in terms of which the child's hypotheses about word meanings are formulated. Immature lexical entries differ from mature lexical entries in being incomplete; not all of the components of the adult definition have yet been included in the child's.

Moreover, Carey goes on to state that "this view--that semantic development involves building definitions component by component--was presupposed, if not explicitly stated, by most students of language development."[21]

The basis for this component-by-component view of concept acquisition was what Carey calls the classical view of concepts. This classical view shares many aspects of TSP, the definitional view of meaning attacked by FGWP as described in the previous chapter. As Carey goes on to argue in her paper, the evidence for component-by-component acquisition as based on this classical view of concepts does not hold up on close investigation, and some of the

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[20]Susan Carey, "Semantic Development: the State of the Art," in Language Acquisition: The State of the Art, eds. Eric Wanner and Lila Gleitman (Cambridge, MA: Cambridge University Press, 1982), pp. 347-89.

[21]Ibid., p. 347.

predictions of the component-by-component view as derived from the classical view are not borne out by further study.

In the context of a conceptualist view of language as a psychological object, the definitional view of meaning suggests a specific program in concept acquisition, and that specific program relates directly to the question of semantic primitives. According to Carey,

> concepts are seen as primitive in three different senses:
> Definitionally primitive: If there is a single set of concepts out of which all other concepts expressible in the language can be defined, then the concepts used in the definitions are primitive.
> Computationally primitive: If there is a set of concepts that is the final stage of comprehension and/or the first stage of production and/or the elements manipulated in thinking, then this set is primitive.
> Developmentally primitive: if there is a set of innate concepts, or at least very early-acquired concepts, out of which all other concepts are built, then this set is primitive.

Moreover, according to Carey, these three set of primitives "are assumed to be identical."[22]

It is easy to see why a conceptualist view of language would bring one to the conclusion that the sets of definitional and developmental primitives are identical. If language is a psychological object, as it is on the conceptualist view, then the source of language is psychological, and moreover, this source has an innate basis. With respect to concept acquisition, then, what this means is that the definitional primitives must be innate since,

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[22]Ibid., pp. 350-51.

by definition, they form the basis, together with what Fodor calls a "combinatorial system," for the construction of all humanly attainable concepts. Fodor himself states this plainly enough in his list of issues upon which empiricists and nativists both agree:

> Both sides assume that the space of concepts potentially available to any given organism is completely determined by the innate endowment of the organism. This follows from the assumption that (a) the set of potentially available concepts is the closure of the primitive concepts under the combinatorial mechanism; (b) the set of potentially available primitive concepts is innately fixed; and (c) the combinatorial mechanisms available are themselves innately specified.

Fodor is careful to point out that even empiricists "accept the innateness of the primitive conceptual repertoire." [23] In any event, a psychological view of language like the conceptualists' seems to require that the set of developmental primitives be the same as the set of definitional primitives.

Moreover, if one takes a performancist view of language comprehension, then similar reasoning will show that both of these sets of primitives must be the same as the set of computational primitives.

But with a Fregean view of concepts as abstract we do not have to assume an identity relationship between the set of developmental primitives and the set of definitional primitives. The set of definitional primitives is to be determined, on the basis of the notion of simplicity out-

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[23] Fodor, Representations, p. 277.

lined in Chapter I, as the smallest set that will account for the semantic properties and relations of the sentences of the language. However, as Katz has pointed out, the innate language mechanisms might involve a simplicity metric different from this standard sense of simplicity, it is possible that the set of developmental primitives might be, as Carey suggests, more "fine grained" than the set of definitional primitives.

In addition to providing an account of the classical view of concept development, particularly with respect to the three types of primitives as quoted above, Carey also presents three theories that are alternatives to the component-by-component theory and which illustrate weaknesses of the classical view of concepts. These theories all basically begin from some disagreement with the classical view and offer a theory intended to satisfy the supposed inadequacy in the classical view which is the basis for this disagreement. When we look at these purported inadequacies in the classical view, however, we see that the more fully developed definitional theory outlined here, namely ST, does not suffer from such deficiencies.

Thus, the first of these three theories, the cluster concept theory, proceeds from the objection that it is difficult to find definitions that provide necessary and sufficient conditions for reference. Objections of this type to the definitional theory are those of Wittgenstein and Fodor discussed at some length in the previous chapter.

I refer here back to my rebuttal of such objections, reiterating that such difficulty is only a function of the of the complexity of natural languages, and that the type reference/token reference distinction can take care of purported counter examples involving context.

The cluster concept theory thus proceeds from an objection to the classical theory that is remedied by the more sophisticated definitional theory outlined here. However, this cluster concept theory is related to important research into what is known as prototypicality structure. The work of Rosch is particularly noteworthy in this regard.[24]

Basically, the cluster concept theory says that instead of having the all-or-none situation of a definitional theory, whereby the component concepts in the definition of a word spell out sufficient and necessary conditions for reference, what you have is a system of family resemblances whereby members of the family share different components to different extents from a cluster of components that make up the particular concept. No single member of the class referred to by the word need have all of the features in the cluster concept representing the whole class.

This cluster concept theory thus leads naturally to

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[24]E. Rosch, "On the internal structure of perceptual and semantic catergories," in Cognitive Development and the Acquisition of Language, ed. T. E. Moore (New York: Academic Press, 1973).

Rosch's notion of a "prototypical" member of a catergory, which Carey defines as "that member sharing most underlying components with other members and least with members of contrasting catergories."[25] Rosch's experiments found psychological evidence for such prototypicality structure in terms of rankings of members of a catergory as to their degree of exemplariness as members of that catergory. In so far as prototypicality proceeds naturally from the cluster concept theory, such findings were taken as evidence for the cluster concept theory.

However, as Carey implies, it is questionable whether such prototypicality rankings reflect the nature of semantic representation in the mind. Rather, they may represent "the organization of knowledge about catergories, and/or the structure of access routines relevant to the use of this knowledge, in certain learning, memory, and problem solving contexts." Evidence that suggests such alternative sources of such prototypicality structure comes from Amstrong, Gleitman, and Gleitman, who have found that even clearly definable terms like "odd number" yield prototypicality results upon experimentation. Thus, the prototypicality results do not seem to satisfy our aboutness requirement.[26]

The second alternative is Fodor's theory that all lexical concepts are innate.[27] Carey points out the obvious

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[25]Carey, p. 352. [26]Armstrong, et al., pp. 263-308.
[27]Fodor, Representations, pp. 257-316.

implausibility of such a theory, which would entail the innateness of such concepts as MESON, TELEVISION, and LASER.[28]

Basically, Fodor's view can be seen as proceeding from two objections to the classical view: the Wittgensteinian objection regarding the difficulty of finding definitions that provide necessary and sufficient conditions for reference, which I have already discussed, and the objection that there is no theory other than an inadequate empiricist theory to account for the acquisition of word meaning. In fact, however, there is no reason why a definitional theory should have to assume the empiricist view of the innateness of only sensory concepts as opposed to the innateness of the set of definitional primitives or of some more "fine-grained" version of this set. Moreover, both of these latter alternatives to Fodor's view of the innateness of all lexical concepts avoid the implausibility of the notion of the innateness of such concepts as ENTROPY, MESON, TELEPHONE, and so on. And since the definitional primitives constitute a much smaller set than the set of lexical concepts, a definitional view of concept acquisition offers a more restricted view of the innate mechanisms needed for the acquisition of word meaning.

Another weakness of Fodor's theory is his notion of the "brute causal" triggering of innate concepts. If

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[28]Carey, p. 357.

concepts are taken to be abstract rather than psychological, then we cannot assume this sort of causal contact between concepts and the mind, concepts being beyond space and time. In his discussion of the faculty of intuition for the apprehension of language and other abstract objects, Katz suggests a solution to the question of how the mind could apprehend abstract objects. Although we need not go into this solurion here, Katz's discussion does suggest that Fodor's notion of the "brute causal" triggering of innate concepts is not a valid theory if concepts are taken to be abstract.[29]

An examination of E. D. Klemke's discussion of Frege's ontology reveals several reasons for taking concepts to be abstract.[30] However, the following passage from Klemke deals with those aspects of the issue that we are concerned with here:[31]

> Concepts are objective, but not actual. For "psychological Logicians" this would mean that they must be ideas. But Frege denies this. Ideas are subjective, and private to each person. But concepts are not; we can all grasp them in common. By grasping, Frege here means: grasping by the mind. Just as a pencil exists independently of my physically grasping it, the same holds for "that which we grasp with the mind"-- it exists independently of the activity of grasping. Since concepts are among the things that must be grasped by the mind, and since concepts are called properties,

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[29]Katz, Language and Cther Abstract Objects, pp. 202-205.

[30]E. D. Klemke, "Frege's Ontology: Realism," in Essays on Frege, ed. E. D. Klemke (Urbana: University of Illinois Press, 1968), pp. 157-77.

[31]Ibid., pp. 162-63.

> it follows that Fregean properties must be abstract rather than concrete.

The observations that concepts are objective rather than subjective and that they are not actual lead to the conclusion that concepts are abstract. Now this means, as Klemke notes, that they are not capable of acting directly on the senses since whatever is abstract cannot have temporal or spatial location. For this reason, Fodor's notion of the brute causal triggering of innate concepts is wrong; concepts, being abstract, cannot participate in the direct causal contact that Fodor's theory requires. Moreover, as Klemke also points out, concepts cannot, contrary to the "Psychological Logicians" and Fodor, be ideas or psychological objects. Rather, they are grasped by the mind, and exist "independently of the activity of grasping."

The third theory proposed as an alternative to the component-by-component model of concept acquisition is Putnam and Kripke's causal theory of reference, which I discussed in Chapter III. As I noted there, Putnam's formula for class membership of natural kind terms--namely, "same natural kind as _____" with the blank filled by some representations of exemplary members of the class--would not circumvent the need for definitional components since such definitional components would be implicit in tests for sameness, at least in cases of marginal members of a class.

These theoretical objections to the classical theory, then, can be adequately met by a view of concept acquisition

that is based on ST. However, we need to examine Carey's claims against the evidence for the component-by-component view to see how such claims bear upon an ST type theory, particularly with concepts taken to be abstract.

Carey discusses two types of evidence in favor of component-by-component acquisition, showing through her discussion that such evidence does not in fact support the component-by-component view. The first type of evidence is order of acquisition. The idea is that if concepts have definitional structures that are acquired by putting together the components of the definitions one by one, then more complex concepts, or concepts with more definitional components, should take much longer to acquire than less complex concepts, or concepts involving fewer definitional components. The second type of evidence is evidence of incomplete lexical entries: that is, incomplete acquisition of the full set of components in the meaning of a lexical item, resulting in incorrect intuitions of semantic properties and relations like synonymy.[32]

The problem with the first type of evidence is its assumption that relative definitional complexity should determine order of acquisition. This seems a very strong assumption to make in face of the likelihood that other factors may play a large role in determining order of acquisition. For example, Carey discusses the acquisition of

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[32]Carey, pp. 360-61.

comparative spatial adjectives--big, little; tall, short; long, short; wide, narrow; thick, thin; deep, shallow; high, low.[33] Carey points out that the attested order of acquisition of these concepts is at variance with their relative definitional complexities as indicated by Bierwisch's analyses of them. However, if we consider the factor of contextual salience as playing a major role in order of acquisition, we can account for the attested order, which is as follows: first big/little; then long/short, tall/short, and high/low; and finally wide/narrow, deep/shallow, and thick/thin.[34] That is, difference in over-all size is more salient than difference in one major dimension, and difference in one major dimension is more salient than difference in a minor dimension.

The argument here, then, is simply that the discrepancy between order of acquisition and relative definitional complexity is not itself evidence against a definitional view of the acquisition of word meaning. Such evidence only serves to show that factors other than relative definitional complexity may play a larger role in determining order of acquisition. In addition, given that with a view of concepts as abstract and not as psychological objects the developmental primitives may be more fine grained than the definitional ones, it would seem to be a mistake to impose complexity rankings determined on the

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[33]Carey, p. 365. [34]Ibid..

basis of definitional primitives on the developmental schedule.

Regarding the evidence for incomplete lexical entries, it would seem that the possibility that the set of developmental primitives might be more fine grained than the set of definitional primitives may also help account for what Carey sees as the failure of this evidence to stand up under further investigation. Carey notes that although there is evidence of the sort of incorrect lexical entries resulting in synonymies that a component-by-component view would predict, these synonymies do not hold up under within child task variation. Carey goes on to note that such incorrect lexical entries "may, indeed, differ from those of adults, but not in the way envisioned by the compositional view."[35] That is, the evidence that Carey has reviewed does not offer unqualified support for a definitional view like that of the classical, component-by-component view. However, it may not be necessary to completely abandon a definitional view. Perhaps such a view could be retained with certain revisions. These revisions have to do with the assumption that the set of developmental primitives are the same as the set of definitional primitives. Two problems arise:[36]

> The problems are twofold--I shall call them the "grain" problem and the "theory-laden" problem. One goal of componential analysis is to find the features needed to individuate all the words in a lexical domain,

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[35]Ibid., p. 366. [36]Ibid., pp. 366-67.

> that is, to capture the contrasts among related words. Such features are likely to be too coarse-grained to be developmental primitives. Also, such features often represent a systemization of knowledge, the linguistic community's theory building. As such, they depend upon knowledge unavailable to the young child, and they are therefore not likely to be candidates for developmental primitives.

There are two aspects of Carey's conclusion here, one with which I find myself in agreement, and the other with which I disagree. As indicated in the previous discussion, I agree that the developmental primitives may be more fine grained than the definitional ones. This is allowed for if we take the position that concepts are abstract rather than being psychological objects and that the innate evaluation metric may differ from the standard notion of simplicity. However, I do not go along with Carey's notion of the "theory-laden" problem. To get at this notion of Carey's and my objection to it I must say something about the empiricist view that there is no theory-neutral meaning.

I refer here again to a paper by Katz entitled "Semantics and Conceptual Change" in which he argues for a language-theory distinction built on an intensionalist notion of meaning and a semantic theory that sets out to give an optimal account that predicts and explains the semantic properties and relations of linguistic expressions in accordance with native speaker intuitions.[37] Katz notes that the failure of the logical empiricists' language-

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[37]Katz, "Semantics and Conceptual Change," pp. 327-65.

theory distinction, led to the view, held by the "new philosophers of science," that there is no theory-neutral meaning, but rather that meaning is always theory-relative. But the failure of the logical empiricists' language-theory distinction is due to arguments like those of Quine, Putnam, and Donnelan to which, as we have already seen, the intensionalist theory of meaning of Katz is not vulnerable. Fundamentally, both Quine's and Donnellan's, and, therefore, also Putnam's criticisms of meaning are built on the insight that, in the logical empiricist view of meaning, there is no independent notion of meaning upon which to ground such meaning notions as synonymy and analyticity. Such criticisms led to the view, as in Quine's holistic theory of knowledge, that "no statements are immune from revision," and that even logical laws are subject to change.[38] As we have seen, Katz points out the incoherence of Quine's holistic theory of knowledge and argues that ST, a fully intensionalist theory of meaning, does have an independent notion of meaning based on a semantic theory that predicts and explains the semantic intuitions of native speakers.[39] Therefore, there is an absolutist theory-neutral notion of meaning, a language-theory distinction.

My objection to Carey, then, is that her view of the "theory-laden" problem arises largely from Quine's holistic

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[38]Quine, From a Logical Point of View, p. 42.

[39]Katz, "Semantics and Conceptual Change," pp. 341-46.

theory of knowledge, which has been shown to be incoherent, and Quine's argument that there is no independent notion of meaning upon which to ground such semantic properties and relations as analyticity and synonymy which is under-cut by the collapse of taxonomic linguistics and its replacement by Chomsky's notion of a generative grammar set up to explicate native speakers' linguistic intuitions. In place of Carey's notion of the theory-laden problem, therefore, I suggest the "language-laden" problem. That is, in place of the view that knowledge of definitional primitives depends on scientific or theoretical knowledge, I hold that knowledge of definitional primitives depends on knowledge of semantic properties and relations in the language. That is, given that there is a valid language-theory distinction, for which I claim that Katz's arguments offer very strong support, then there is no reason to believe that a child's semantic knowledge of the language will depend on his or her getting to know certain scientific facts or theories. Rather, my claim is that such theoretical knowledge is separate from language knowledge.

My view is that there is tacit innate knowledge of the definitions of semantic properties and relations like synonymy, entailment, meaningfulness, anomaly, contradictoriness, and so on, and that it is this type of innate knowledge, together with a set of innate developmental primitives, that function to construct in intuition representations of the full definitional structures of word

meanings based on hypothesis testing over linguistic and contextual data. My view then of the way in which the child constructs definitions of words parallels Chomsky's earlier Aspects view of the way in which the child, as little linguist, constructs a grammar for sentences. In this view the theory-laden problem becomes the language-laden one.

In the next and final chapter I will return to this issue of concept development in a discussion of Melissa Bowerman's evidence regarding the acquisition of causative verbs. I will argue there that Bowerman's study is strong evidence for a definitional view of concept development. What I have shown in this chapter is that a definitional view of concept development based on ST is not vulnerable to the problems that beset a classical component-by-component view.

CHAPTER V

A SEMANTIC MARKER ACCOUNT OF CAUSATIVE VERBS

This chapter has three general purposes. The first is to dispell skepticism about definitions, such as Fodor's view that adequate definitions are impossible for a large class of words. I have dealt with this skepticism in Chapter III by presenting semantic markers for words for which it was claimed that a definitional account was impossible. In general it seems that the apparent impossibility of providing definitions for certain words can be attributed to two things: the complexity of the concepts involved, and the weakness of the analytical tools used to construct definitions. My point of view here is that what is needed for constructing definitions is a tool like that of semantic markers that can display the internal, hierarchical structure of concepts. Therefore, my presentation in this chapter of semantic markers for several English causative verbs is partly intended to show that skepticism about definitions is not justified.

The second purpose of this chapter is to demonstrate the explanatory adequacy of a semantic marker theory in accounting for semantic properties and relations of the expressions and sentences of a language. By explanatory

adequacy I mean adequacy in being able to provide explanations for the phenomena, where by explanation I intend the ordinary notion of explanation as discussed in Chapter I. I specifically do not intend here Chomsky's conceptualist notion of explanatory adequacy. I will provide illustrative examples to show that ST can account for such semantic properties and relations as entailment, analyticity, contradictoriness, synonymy, and anomaly. I will show that ST can account for semantic roles and can define the notions causative verb and causative proposition.

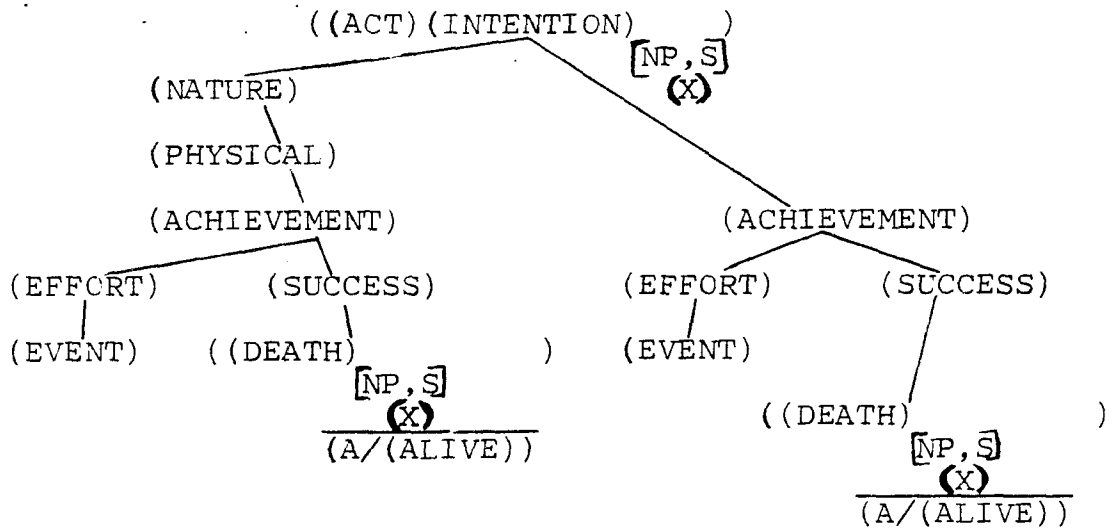
My third purpose is to show that a semantic marker theory can supply the grammatical structures needed by a psycholinguistic theory--that is, a theory about such things as native speakers' introspections about relatedness and complexity, the on-line production and comprehension of sentences, and the various aspects of language development--so that such a theory can provide predictions and explanations of psycholinguistic phenomena. I will therefore discuss the psycholinguistic evidence concerning causative verbs, both the evidence from adult performance as well as the evidence from child language development.

Introducing the Semantic Markers

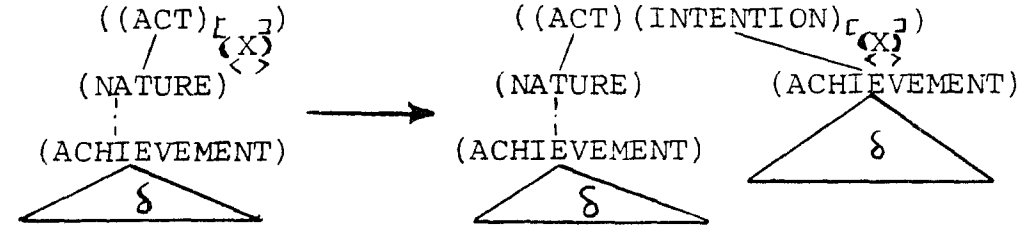
Before illustrating the explanatory adequacy of a semantic marker account of causative verbs there are several things that need to be said about the semantic markers that I will present.

These semantic markers are presented very tentatively. That is, given the state of development of semantic theory, it is very likely that the semantic representations I will suggest here will be considerably altered by further developments in the theory. The presented markers are then on a par with the phrase markers presented by Chomsky and others at the early stages of current syntactic theory.

Second, the following representations differ from Katz's representations of causative verbs in one important way. Katz presents the following semantic marker for "commit suicide":



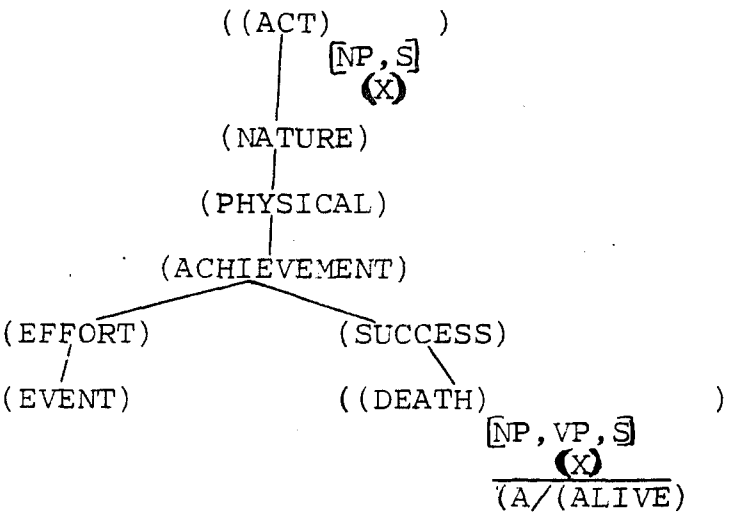
Katz also presents the following representation of the adverb "intentionally":[1]



[1]Katz, Propositional Structure and Illocutionary Force, pp. 72-73.

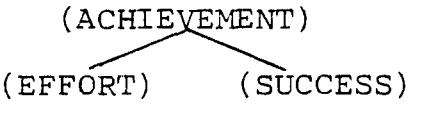
Katz provides these representations, together with other theoretical definitions, in order to give an account of the apparent disparity between the synonymy of "John killed himself intentionally" and "John committed suicide" on the one hand, and the difference in placedness of the two predicates on the other. In any event, this treatment implies the following representation of the verb "to kill":

1.



However, the following considerations lead to the conclusion that the structure in (2) below, which is part of the structure of (1), involves the concept of intention on the part of the agent of the act:

2.



That this structure involves intention makes it inadequate for causative verbs like "kill" which do not entail such intention, as Katz's own account of the synonymy relation between "commit suicide" and "kill oneself intentionally" seems to acknowledge. First, the following sentences seem to

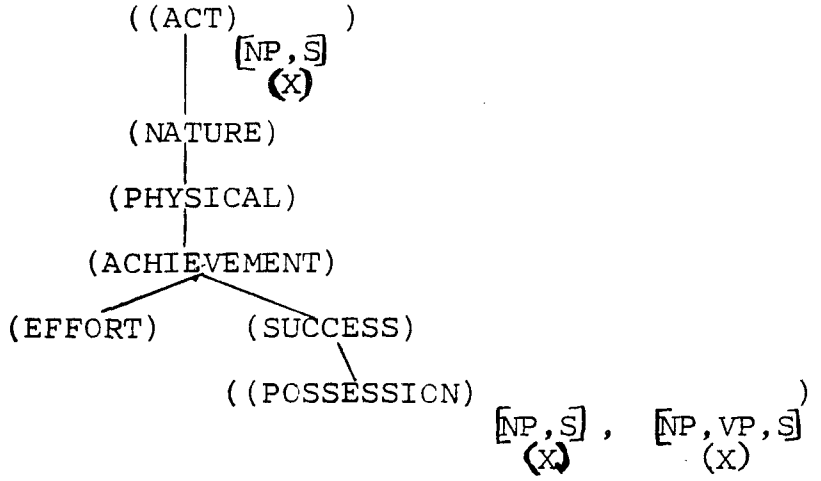
be contradictory:

3. John tried to get up without intending to.
4. John made an unintentional effort to get up.

Moreover, it seems that the reason for the contradictoriness here is the direct result of the fact that making an effort entails having the relevant intention. But if the concept EFFORT includes the concept of having an intention, then the concept of ACHIEVEMENT in (2) must also include having an intention since ACHIEVEMENT is the superordinate concept in this structure. Therefore, it seems that this is not the right structure for representing causative verbs like "kill."

In addition, notice that the verb "to achieve" is itself a causative verb. An attempt to provide a semantic marker for this verb using (2) would result in something like the following:

5.



This structure would run into the same intention problems of verbs such as "kill" in accounting for sentences with the verb "achieve" which do not involve intention, such as the following:

6. He achieved great wealth without intending to.

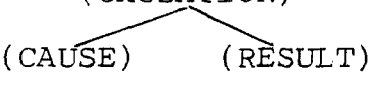
In addition, (5) is unrevealing since it does not seem to afford us the possibility of explaining the entailment relation between (6) above and the following:

7. He caused himself to have great wealth.

In other words, the structure does not show the relation between *achieving something* and *causing*. But this is the crucial relationship that we must capture if we are to isolate what is common to all causative verbs in English.

With these considerations in mind, I will substitute the structure in (8) below for the structure in (2) above in the semantic markers for the causative verbs that I will discuss.

8.



(8) both avoids the implication of intention that is part of (2), and explains the causative status of causative verbs as a consequence of their inclusion of the concept of causation.

In addition, since it seems to be a general characteristic of causative verbs that they involve a change as part of the result branch of the concept, I will incorporate Katz's representation of a change in condition as presented in his semantic marker for the sense of "persuade" on which it involves changing someone's belief.[2] The purpose of incorporating this representation is to achieve generality.

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[2]Ibid., p. 164.

It is to be noted that this representation explains the need for the selection restriction in the above marker for the verb "to kill."

A further point worth noting is that in many cases my semantic markers include information that Gergely and Bever have attributed to a stereotype, and which they claim or would claim to be part of knowledge of the world rather than of language.[3] They argue, for example, that the following sentence is anomalous:

9. The dentist dried my mouth by giving me a fast-acting antihistamine.

They attribute what they claim to be the anomalousness here to a stereotypic notion of directness. They argue that this notion of directness must be stereotypic since the following sentence, which is not anomalous, does not involve a method of drying that is any more direct from an objective point of view than the method involved in (9):

10. The dentist dried my mouth by blowing air in it.

However, while the directness of the method of drying involved in the meaning of the verb may be non-objective, we cannot on this basis conclude that the information concerning the method of drying is non-linguistic. The problem is just that this type of scientific objectivity is not a criterion for what is linguistic information, especially in the context of what we have already said about the language-theory distinction. Another example from Gergely and Bever

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[3]Gergely and Bever, pp. 29-37.

will help to make the point that information about the method of causation in causative verbs can be linguistic:

11. *Floyd dropped the glass by tickling Mary, who was holding it.

The anomaly in (11) seems to be explained by noting that dropping something implies releasing it, which in turn implies that one was holding it in the first place, an implication that the sentence contradicts. Moreover, such implications should be part of the linguistic meaning of the verb in order to derive the appropriate truth conditions for assertive sentence types in which the verb occurs. That is, the implication from "Floyd dropped the glass" to "Floyd was holding the glass" is valid for the sentence types.

This leads us directly to consider the criteria that decide whether some information is linguistic information or not. These criteria have already been supplied by Katz and are summarized in the following three definitions:

> (IP) The information I is semantic information for the linguistic expression "X" if, and only if, the semantic representation of "X" in the true semantic description of the language represents I as part of the meaning of "X".[4]

> (C) r is an optimal semantic representation of the expression e just in case, relative to the definitions of the semantic properties and relations in semantic theory, r predicts each semantic property and relation of e and of every expression e occurs in, and there is no simpler semantic representation of e that is different from r and makes the same predictions as r.[5]

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[4]Katz, "The Neoclassical Theory of Reference," p.107.

[5]Katz, "Semantics and Conceptual Change," p. 343.

> (D) X is the type referent of the expression "W" just in case X has each of the properties represented in the best hypothesis of the meaning of "W" in the language; if nothing has each of these properties, the expression "W" has null type reference.[6]

In (C) above "expression" is to be understood in the null context.

These are the criteria, then, that I will use in deciding what information should be included in semantic markers. What this amounts to is that inclusion is decided on the basis of whether the information is needed to predict and explain the semantic properties and relations of sentence types, or to account for correct type reference.

One last item needs to be mentioned before discussing the adequacy of a semantic marker treatment of causative verbs: namely, the interpretive conventions for semantic markers. These interpretive conventions, as so far developed, are spelled out clearly in Katz's Propositional Structure and Illocutionary Force, particularly pages 62 to 69. These are the interpretive conventions that I will assume in the following discussion.

Explanatory Adequacy

One important aspect of the explanatory adequacy of a semantic marker theory is its ability to identify in a non-arbitrary and explanatory manner classes of lexical items and propositions that share some important semantic properties. Thus, in Propositional Structure and Illocu-

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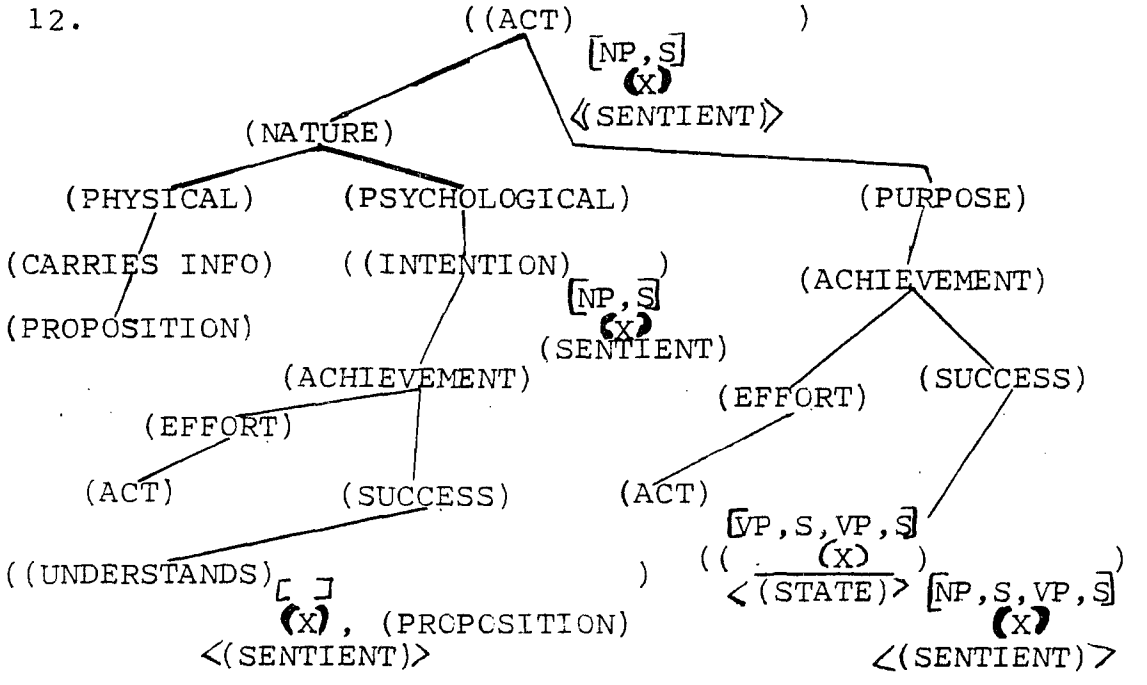
[6]Katz, "The Neoclassical Theory of Reference," p.110.

tionary Force, Katz defines the class of performative propositions partly in terms of those propositions which share the following structure for the verb "request," except for a possibly different subtree under the semantic marker (PURPOSE):[7]

12.



And we could identify Austin's original notion of a class of performative verbs as just those verbs that have the above structure with the exception of a possibly different subtree under (PURPOSE).

In a similar manner, we can define what we shall call the class of causative-act verbs as just those verbs having minimally the structure implicit in the following schema, with semantic markers (M_1) and (M_2) appropriately specified.

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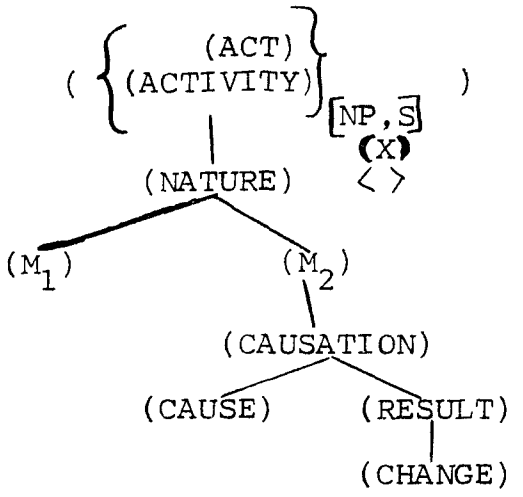
[7] Katz, Propositional Structure and Illocutionary Force, p. 142.

13.



The notation $\left\{\begin{matrix}(A)\\(B)\end{matrix}\right\}$ is used here as it is used in generative phonology to indicate the occurence of either (A) or (B) in the position thus marked. (M_1) and (M_2) can be (PHYSICAL) OR (PSYCHOLOGICAL) and (M_1) can also be null.

In addition, we might extend our treatment of causatives beyond the lexical level to the level of propositions by proposing a definition of the class of causative-act propositions. This class of causative-act propositions cross-classifies with constative and performative propositions since the class is defined by its propositional content rather than its propositional type.

The purpose of defining such a subclass of causative-act propositions is two-fold, both grammatical and psychological. First, from a grammatical point of view there is the need to account for propositions whose meanings involve a causative act, and to distinguish such propositions both from propositions which involve the notion of causation without the notion of an act, and propositions which involve no concept of causation whatever. That is, we want our

semantic theory to be able to clarify and explain the differences in meaning which we apprehend intuitively between sentences like the following:

14. I broke the glass.
15. I caused the glass to break.
16. The glass broke.

Thus, (14) involves the notion of a causative act; (15) involves the notion of causation without the notion of an act; and (16) involves neither the notion of causation nor the notion of an act--it simply asserts an event. From the point of view of grammatical adequacy, then, our theory should be able to distinguish these meanings.

From a psychological point of view our grammatical theory should provide the grammatical structures that are needed by a psycholinguistic theory so that it can predict adult introspections like the relatedness judgements noted by Gergely and Bever and to explain child language acquisition with respect to learning the meaning distinctions in (14), (15), and (16) above. In fact, psycholinguists have noted quite definitively various aspects of the development of causative and non-causative concepts. Our grammatical account of causative verbs, then, would benefit in terms of plausability if it could provide the basis for explaining the developmental evidence in a more perspicuous manner than has so far been available.

Before defining this causative-act subclass of propositions, however, I would like to discuss briefly one aspect of Katz's definition of performative propositions

which is needed in order to define causative-act propositions.

This is the formalization of the unconverted condition. It is achieved partly by correlating the sequence of terms in the reading of the propositional content of a proposition with the sequence of predicates or conditions. The notion of the unconverted condition for an assertive proposition can be understood as the satisfaction by the sequence of terms of the sequence of conditions, with quantity and time appropriately specified. One of the members of the reading of an assertive proposition must therefore be a semantic marker of the following form:[8]

17. $(((\text{Satisfies})_{\substack{[Q_n] \\ X \\ \langle\rangle}} \text{of DES}(t_1), c_1^1, \ldots, c_k^1, {}_{\langle\rangle}^{X}[T^1]) \& \ldots \&$
$((\text{Satisfies})_{\substack{[Q_n] \\ X \\ \langle\rangle}} \text{of DES}(t_n), c_1^n, \ldots, c_m^n, {}_{\langle\rangle}^{X}[T^n]))$

Q_n here represents the catergorized variable for a quantifier term, $\text{DES}(t_1)$ represents the designatum of term t_1, "$c_1^1, \ldots, c_k^1$" represents the conditions applied to term t_1, and T represents the catergorized variable for the temporal term that indicates the time at which the condition holds for the relevant term. The & represents logical conjunction. 17 thus characterizes the truth conditions of an assertive proposition as the conditions that the appropriate number or proportion of the designata of each term satisfy the

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[8] Ibid., p. 132.

conditions attributed to them by the proposition at the time or over the temporal interval specified in the proposition.

For other propositional types, however, the converted conditions, although having the same formal structure as (17), will differ from the truth conditions of assertions in very specific ways. For example, the fulfillment condition of obligatives is defined as follows:[9]

> 18. The fulfillment condition for an obligative proposition P is that the agent of the communicative act perform some (future) act that dispences the obligation undertaken in the communicative act.

In the case of obligatives, then, this converted condition will be formalized in the form of (17), with a term specifying the concept of the agent of the communicative act, with conditions specifying the concept of his dispensing the obligations undertaken, and with the time specified as after the speech point.

Now while a performative proposition can be taken as announcing the speaker's act in saying something, a causative-act proposition can be taken as simply involving the concept of a causative act. That is, causative-act propositions, unlike performative propositions, do not have any special extensional properties. Causative-act propositions simply have unique intensional properties. Our definition of causative-act propositions, then, should be such that it would cover both performative and constative propo-

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[9]Ibid., p. 233.

sitions that involve an act of causation. That is, it should identify both (19) and (20) below as causative-act propositions:

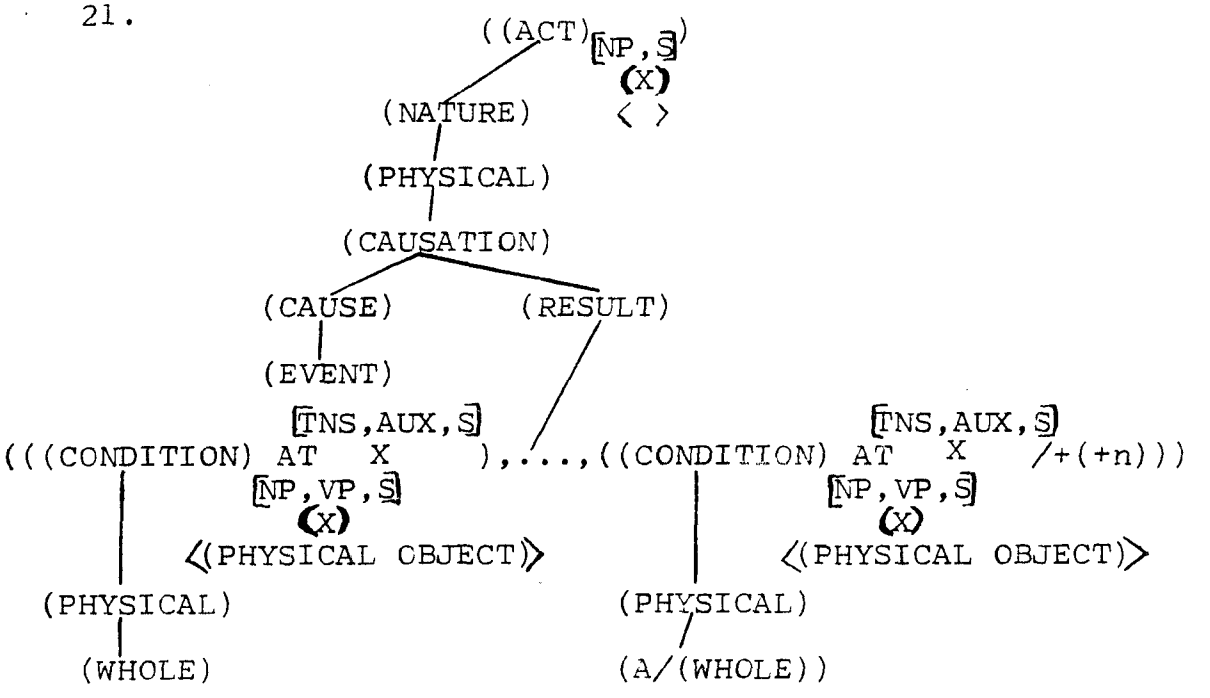
19. I promise to break the glass.
20. I broke the glass.

I therefore suggest the following definition of a causative-act proposition:

A. A proposition P is a causative-act proposition just in case (i) the reading of P contains a semantic marker of the form (17) and also a semantic marker representing the propositional content of P having minimally the form of (13) with (M_1) and (M_2) appropriately specified, and (ii) no catergorized variables occur in the reading of P.

This definition identifies (19) and (20) as causative-act propositions since they both involve structures having the form of (13) and satisfying the remaining requirements of the definition, the verb "to break" which occurs in both sentences having the following structure:

21.



It should be fairly clear that causative-act propositions have great functional utility in language, as is attested by the relatively large class of causative verbs in any language. For these reasons it seems important for a semantic theory to be able in principle to offer some such definition as (A) of this class of propositions. Moreover, the notion that causative verbs all have semantic representations in some form of (13) allows for a fair amount of simplification in defining causative verbs. This is explanatory adequacy of a purely formal character, in conformity with the platonist notion of explanatory adequacy described in Chapter I.

In addition, such definitions help to simplify the work of a psycholinguistic theory that would attempt to account for the acquisition of causative concepts. That is, acquisition of causative concepts can be seen as involving acquisition of the structure common to all causative concepts. In fact, as we will see when we look at the evidence from language development concerning causative verbs, certain aspects of the learning of causative verbs can be attributed to a mistaken hypothesis concerning this structure--an incomplete acquisition of the structure. Once this common causative structure is acquired, acquisition of particular causative concepts can be understood as acquisition of the variable portion of the definition of causative-act verbs as specified by (13).

Another important aspect of the explanatory adequacy

of this account of causative verbs is its ability to predict and explain semantic properties and relations like analyticity, contradictoriness, anomaly, meaningfulness, entailment, synonymy, and so on. I have already offered two examples of this aspect of explanatory adequacy in Chapter III, where I argued that, contrary to FGWP, a definitional theory could predict the entailment relations between the members of the following pairs of sentences:

22. a. John killed Mary.
    b. Mary died.

23. a. The ball is red.
    b. The ball is colored.

In addition, relative to Katz's definition of semantic entailment,[10] an examination of the semantic markers for "amuse," "anger," and "clean" below, and of "break" above (21), will show that they predict and provide the basis for an explanation of the similar entailment relations between the members of the following pairs of sentences:

24. a. The juggler amused the children.
    b. The children had fun.

25. a. John angered the instructor.
    b. The instructor became angry.

26. a. John broke the glass.
    b. The glass broke.

27. a. The waiter cleaned the table.
    b. The table became clean.

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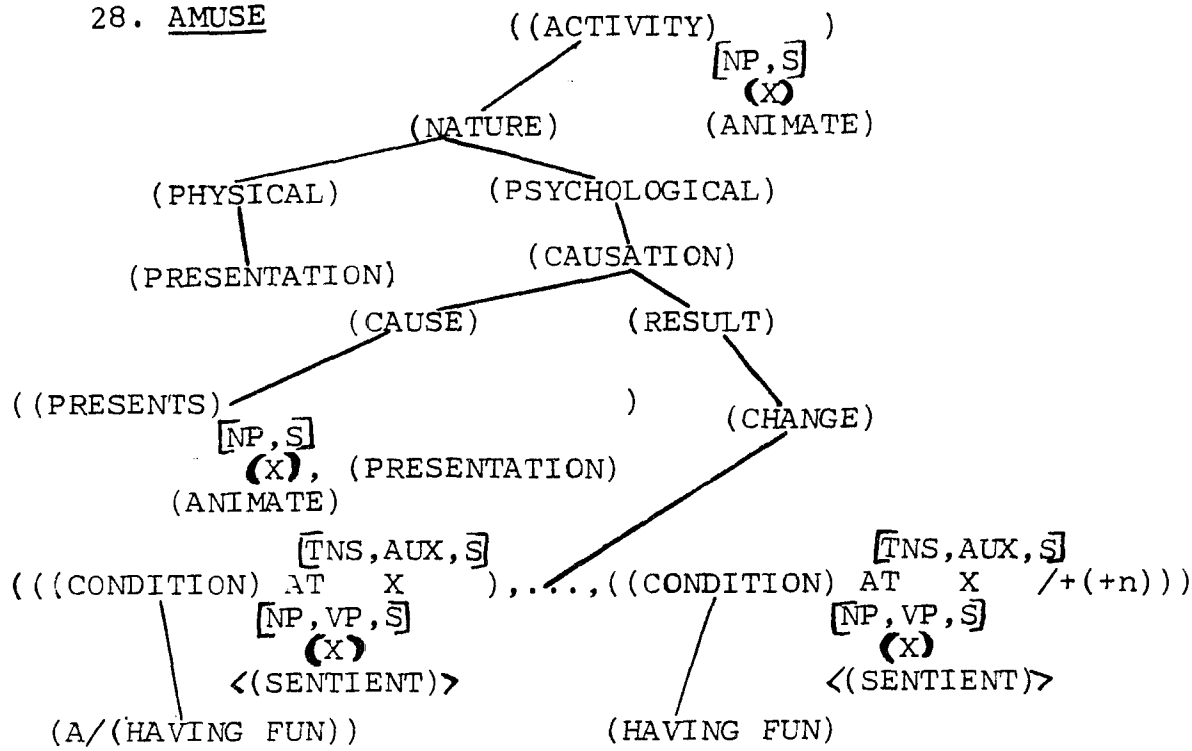
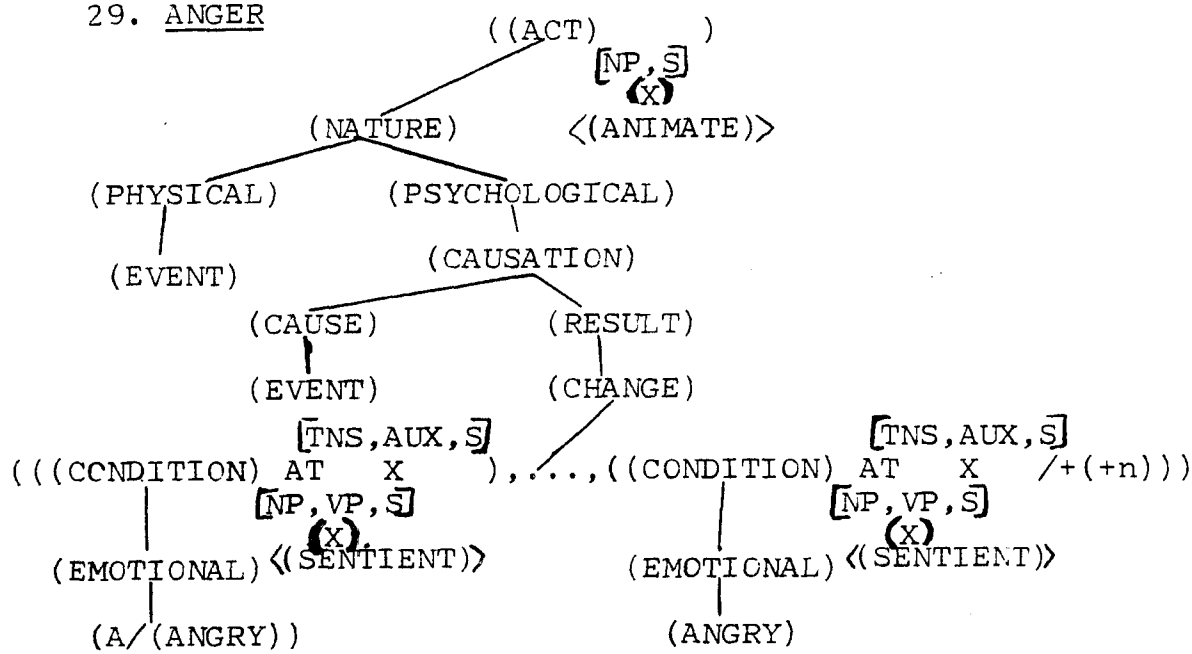
[10] Katz, "The Advantages of Semantic Theory . . .," pp. 390-91.

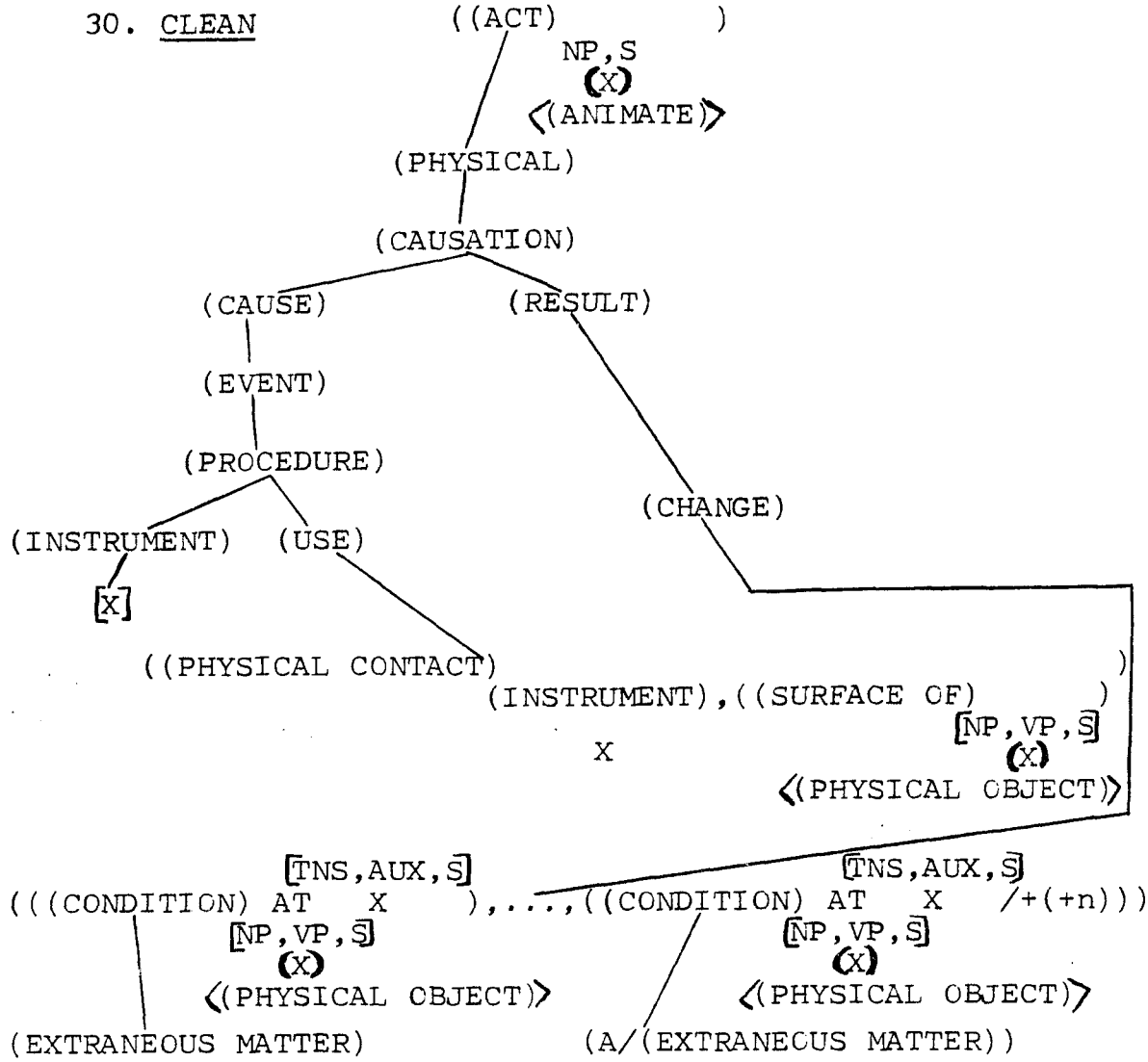
28. AMUSE



29. ANGER



30. CLEAN



Similarly, relative to the theory's definition of analyticity,[11] it seems evident that a semantic marker theory can predict and explain the following analyticities, given the appropriate nominalized versions of the above semantic markers:

31. Amusing someone is causing him to have fun.
32. Angering someone is causing him to become angry.
33. Breaking something is causing it to become broken.
34. Cleaning something is causing it to be free of extraneous matter.

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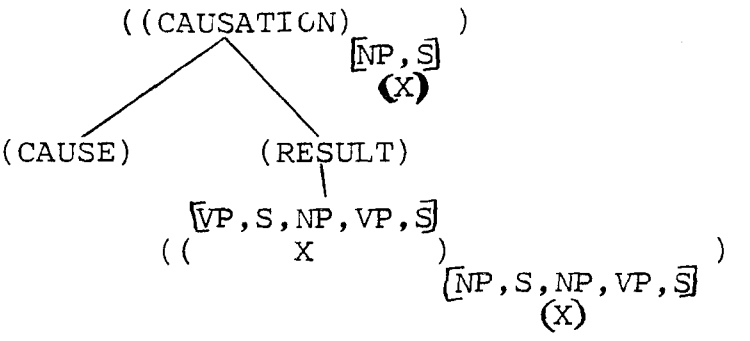
[11] Katz, Semantic Theory, pp. 171-78.

To account for such analyticities we sould also need something like the following semantic marker for the verb "to cause":

35. CAUSE



This semantic marker definition of the verb "to cause" also helps to explain certain aspects of the difference in meaning between pairs of sentences such as "John killed Mary" and "John caused Mary to die." For example, the semantic markers given here indicate that whereas the former entails a physical act on John's part, the latter does not entail any act by John at all. Moreover, since some such distinction is a general characteristic of the difference, with these semantic markers, between the readings of lexical causatives and their phrasal counterparts with "cause," we can on this basis explain the difference in the degree of directness between lexical causatives and their phrasal counterparts. That is, the fact that our markers for lexical causatives show them as involving an act of causation suggests that they would involve a higher degree of directness than phrasal causatives that do not involve an act. The structure of the markers for lexical causatives can be seen as restricting the circumstances that satisfy

the truth conditions of propositions containing them to just those circumstances that constitute a fairly direct type of causation in which the referent of the subject term participates crucially in the causation by his acts or act. On the other hand, the phrasal causatives with the verb "to cause" do not provide such restriction. For example, these facts about the semantic structures of lexical versus phrasal causatives explain the following: John places an expensive vase at the edge of a shaky table so that when I walk by the vase falls to the floor and breaks; it seems to me that under such circumstances we can truly say that John caused the vase to break but not that John broke the vase. That is, John's act is not as crucially related to the process of causation as the semantic structure of the verb "break" requires.

Relative to the definition of contradictoriness,[12] our semantic marker account of causative verbs would predict and explain the contradictoriness of such propositions as follows:

36. Amusing someone is causing him to be sad.
37. Angering someone is causing him to be pleased.
38. Breaking something is causing it to be whole.
39. Cleaning something is causing it to be dirty.

To explicitly capture this property of contradictoriness, our semantic marker theory must specify that, in so far as the above examples are concerned, the pairs being sad and having fun, being dirty and being free of extraneous

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[12] Ibid., pp. 178-80.

matter, and being pleased and being angry, each belong to an antonymy n-tuple of semantic markers.[13]

So far we have been able to show in a fairly straight forward fashion that ST is capable of predicting and explaining entailment, analyticity, and contradictoriness. For one thing, the intuitions regarding the semantic properties and relations of analyticity, contradictoriness, and entailment are reasonably clear. But in dealing with the semantic relation of synonymy our intuitions have to be more precise and we must also provide rules of modification to derive equivalent readings.

For example, we might be tempted to take the following pair as synonymous:

40. a. John killed Harry.
    b. John did something that caused Harry to die.

(40b) specifies an act as well as causation, and, given the syntactic structure of the sentence, seems to subordinate the causation to the act, which is just what we need according to (40a'), the derived reading for (40a) on the following page. However, if we return to our hypothetical case of John putting the vase at the edge of the table, we note that we could truly assert the following:

41. a. John did something that caused the vase to break.

But we still cannot truly assert the following:

41. b. John broke the vase.

The case of (40a) and (40b) is parallel to the case of

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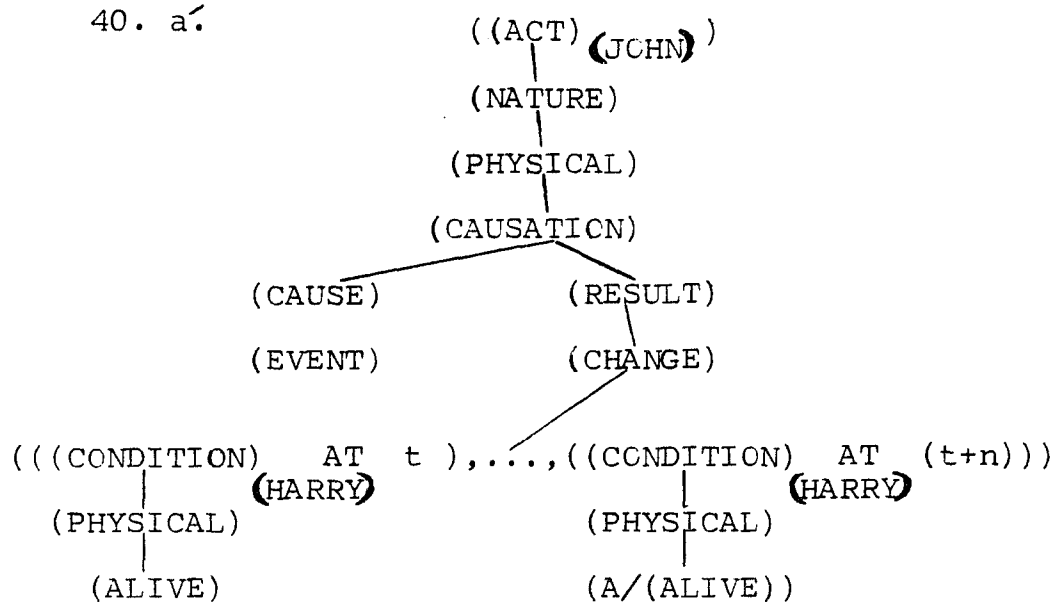
[13]Ibid., pp. 51-53.

40. a.



(41a) and (41b), which suggests that we were correct in hesitating about accepting (40a) and (40b) as synonymous. In fact, we can easily imagine a case for (40a) and (40b) that is similar to the vase example. Thus, imagine that John leaves the scene of an accident of which Harry is a victim and that John could have saved Harry's life had he tried. We might then truly assert (40b) but not (40a).

Now the difference between (40a) and (40b), and between (41a) and (41b) seems to rest on whether or not the act in question was the exclusive cause of the change of condition. Furthermore, this question of being the exclusive cause is clarified when we consider that what we need for synonymy with "John killed Harry" is not (40b), but rather the following:

40. c. John did something which was the cause of Harry's death.

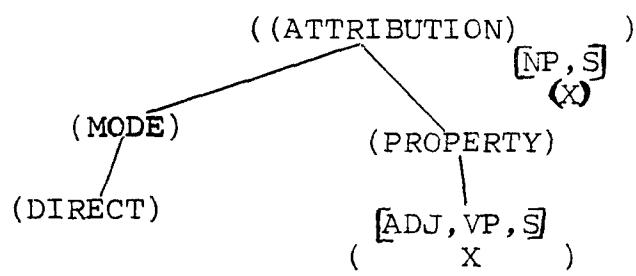
It is because (40c) provides this concept of exclusive

cause in the meaning of the definite article that it is synonymous with (40a). Notice that we cannot truly assert (40c) of our hypothetical accident victim case just as and for the same reason that we cannot truly assert (40a) in this case.

But in order for our theory to capture the synonymy relation between (40a) and (40c) it must provide a way of showing how the concepts of the various constituents of (40c) are combined to form a structure equivalent to that which we have provided for "John killed Harry."

Now since the key relational verb relating the two major constituents of (40c) is the verb "to be," we need a marker for the meaning of the verb "to be." We might propose the following marker for the sense of the verb "to be" in sentences like "the ball is red" and "the ball is colored":

42.



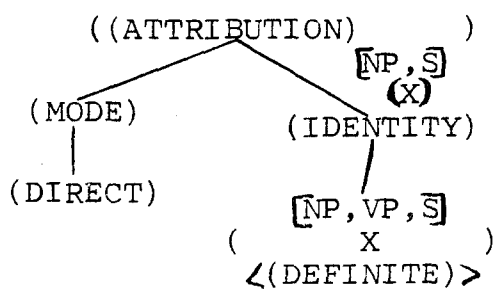
But the sentence we are considering here seems to make use of a different sense of the verb "to be." We might see this more clearly by considering the following three examples:

43. a. John is human.
    b. John is a doctor.
    c. John is the doctor.

It seems to be the case that in both (43a) and (43b) we are

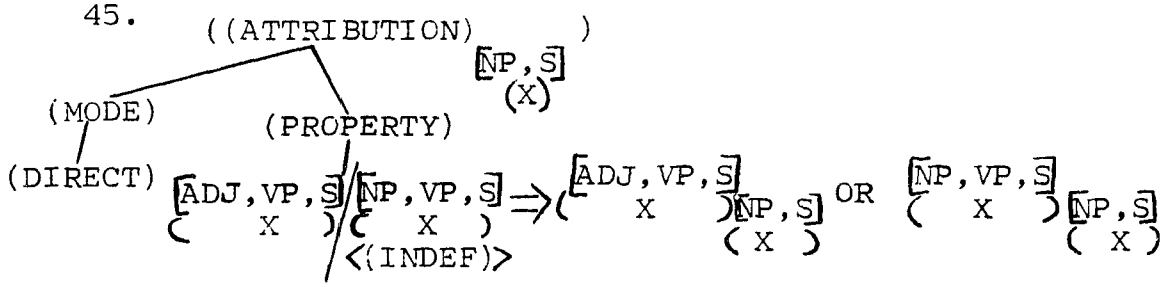
attributing a property to John. But in (43c) it seems that what we are attributing to John is not a property but an identity, and that this difference is due to the exclusiveness meaning of the definite article. It seems, therefore, that in the case of the NP complement with the definite article we need the following structure for the verb "to be":

44.

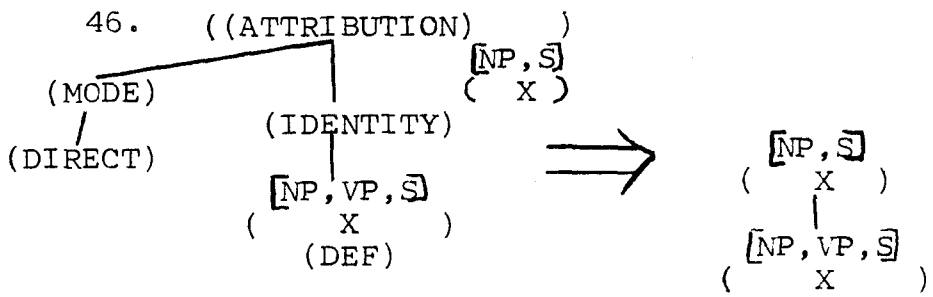


Now we may think of the marker (ATTRIBUTION) as performing the function of an operator that modifies the marker of the subject term in accordance with the following two rules, one for each of the two senses of the verb "to be" that I have presented:

45.



46.



That is, we would derive something like the following structures for the sentences (43a), (43b), and (43c) respectively:

47. a. ((HUMAN) (JOHN))

b. ((DOCTOR) (JOHN))

c. ((JOHN))
|
(DOCTOR)

We also need the following marker for the verb "to do":

48. ((ACT))
/ [NP, S]
(X)
<(ANIMATE)>
(NATURE)
\
[NP, VP, S]
(X)

What this marker says, among other things, is that the nature of the act is to be determined by the thing that is done. Thus, we may informally suggest the rule that the marker immediately dominated by the marker (NATURE) will be (PHYSICAL) if the marker (PHYSICAL) occurs in the marker for the object term, or (PSYCHOLCGICAL) if the marker (PSYCHOLOGICAL) occurs in the marker for the object term, or both if both occur.

If we look again at sentence (40c) we see that the first part of the sentence--"John did something"--sets up a structure like that in (48) with the catergorized variable for the subject replaced by a marker for John.

The remainder of the sentence--"which was the cause of Harry's death"--brings into use rule (46), our informal rule for determining the marker dominated by (NATURE), our marker for the verb "cause," and an analysis of the concept of death into a change of condition from one of being alive to one of not being alive to derive the structure (40a′). This derivation establishes the synonymy of (40a) and (40c). It seems to me that the manner in which this derivation was arrived at is natural and independently motivated. This shows that ST is capable of predicting and explaining the semantic relation of synonymy in a revealing manner. In particular, the theory helps to clarify the notion of exclusive cause as being essential to the structure of causative-act verbs.

Finally, it is pretty clear that our theory can handle anomalies like the following:

49. a. The juggler amused the chairs.
    b. John angered the flowers.
    c. John broke the afterimage.
    d. The waiter cleaned his ideas.

These anomalies can be predicted on the basis of the appropriate selection restrictions beneath the catergorized variables for the objects of the verbs. Hence, these selection restrictions specify that the objects of "amuse" and "anger" must be marked (SENTIENT), while the objects for "break" and "clean" must be marked (PHYSICAL OBJECT). Our markers then do account for these anomalies.

We have seen that our semantic markers for causative

verbs, on the basis of structural definitions in semantic theory, can account for a wide variety of semantic facts. And, in addition to the semantic facts discussed so far in this chapter, it is clear that our theory can account for various semantic roles. Katz has suggested definitions for the semantic roles of agent and recipient,[14] and in Chapter III I suggested a definition for the patient reading. The following related definitions for the semantic roles of causer and causee seem fairly straight forward:

B. R is the "causer reading" in the sentence R_S =df
   a. the part of R_S that represents the propositional content of the sentence S contains a semantic marker of the form

   $$((\text{CAUSATION})\left((M_{i_1}),\ldots,(M_{i_s})\right)—(M_{j_1})—(M_{j_2})—\ldots\ (M_{j_n}))$$

   b. or of the form

   $$(\left(\left\{\begin{matrix}(\text{ACT})\\(\text{ACTIVITY})\end{matrix}\right\}\left((M_{i_1})\ldots,(M_{i_s})\right)\right)—(M_{j_1})—(M_{j_2})—\ldots—(M_{j_n})$$

   Where there is some M_{j_i}, $2 < i < n$, such that (M_{j_i}) = CAUSATION

   c. R is the reading $(M_{i_1}),\ldots,(M_{i_s})$

C. R is the "causee reading" in the sentence reading R_S =df. The part of Rs that represents the propositional content of the sentence S contains a semantic semantic marker of the form:

   $$(\text{CAUSATION})—(\text{RESULT})—((\text{CHANGE OF CONDITION})\left((M_{i_1}),\ldots,(M_{i_s})\right)—(M_{j_1})—(M_{j_n})$$

b. R is the reading $(M_{i_1}),\ldots,(M_{i_s})$

Definition (B) takes it to be the case that the agent of a causative act also plays the role of causer, and expresses

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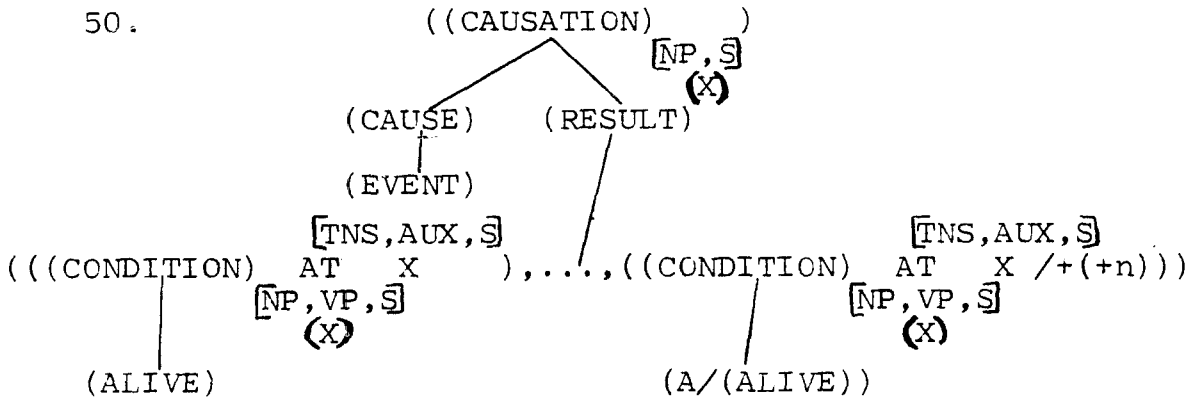
[14] Katz, Propositional Structure and Illocutionary Force, pp. 84-85.

the notion that in such cases the role of causer is included in the role of agent. Definition (C) defines the role of the person or thing affected by causation. Given the previous definitions of recipient and patient readings it also seems that the notion of a causee is included in the notion of a patient, which in turn is included in the notion of recipient with respect to causative propositions. According to our definitions, however, in general a reading can be a patient reading without being a causee reading, though not vice versa. And a reading can be a patient reading without being a recipient reading and vice versa. And a reading can be a causee reading without being a recipient reading and vice versa.

Another aspect of our account of causative verbs is that it indicates a possible ambiguity in some of these verbs. It thus seems that in addition to the sense for which we gave the semantic marker implicit in the derived reading (40a'), the verb "kill" also has another sense defined by the following semantic marker:

50.



This is the sense that we might use in saying "cancer killed John Wayne," or "the electrical shock killed him." Here no act is predicated and the subject of the verb need not be sentient. We might think that this latter sense of the verb is not really a separate sense but just as in (40a') with the subject of the verb personified. However, that such personification effects are rather a consequence of the real ambiguity is suggested by the fact that "cancer killed John Wayne" and "cancer caused John Wayne to die" are synonymous, whereas "John killed Harry" and "John caused Harry to die" are not. It seems, moreover, that this type of ambiguity exists for many causative verbs, and that the sense of the causative-act verbs is more natural when the subject is animate or sentient, while the simple causative sense, when it exists, is the only sense available when the subject is inanimate. The fact that this well attested ambiguity falls out directly from a semantic marker account of causative verbs is another instance of the explanatory adequacy of the semantic marker definitional approach.

Another example of such an ambiguity between a causative-act sense and an exclusively causative sense will further help to indicate the explanatory adequacy of this account of causative verbs since the ambiguity helps to explain what at first might seem to be a counter-example. Thus, it might be claimed that the sense of the verb "keep" in the following sentence is a counter-example to the analysis given here:

51. John kept the cat in the room by closing all the doors and windows.

The claim then would be that "kept" here means something like "caused to stay," in which case the (RESULT) branch of the marker for this sense of keep would not indicate a change of state, contrary to the analysis here. However, our analysis could be justified if we could find evidence to show that the sense of "kept" here is not "caused to stay" but is instead "caused not to leave." The problem with finding such evidence is just that the causative-act sense of "keep" will have the negative element occuring at a point relatively close to the bottom of the semantic marker, immediately dominated by the marker (RESULT). Consequently, in accordance with our distance hypothesis of Chapter I, introspections about the meaning of this sense of "keep" would not be likely to reveal the presence of a negative element.

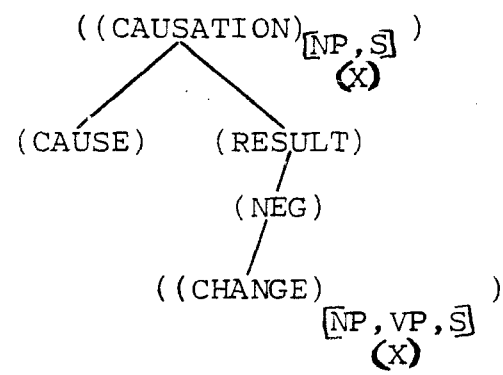
But this is where the hypothesized ambiguity between the causative-act sense and the exclusively causative sense might help us in accordance with our analysis. That is, the exclusively causative sense of the verb "keep," since it would not have the upper part of the structure of the marker of the causative-act sense of this verb--that is, the structure dominating the marker (CAUSATION)--would have a structure in which the negative element is closer to the surface. In this case, then, the negative element should be more available to introspections. Is there such a sense

of the verb "keep" which is judged more clearly to involve the presence of a negative element? Examples like the following indicate that the answer to this question is "Yes."

52. a. Chewing gum keeps him from wanting to smoke.
    b. Chewing gum prevents him from wanting to smoke.
    c. The noise kept him from doing his work.
    d. The noise prevented him from doing his work.

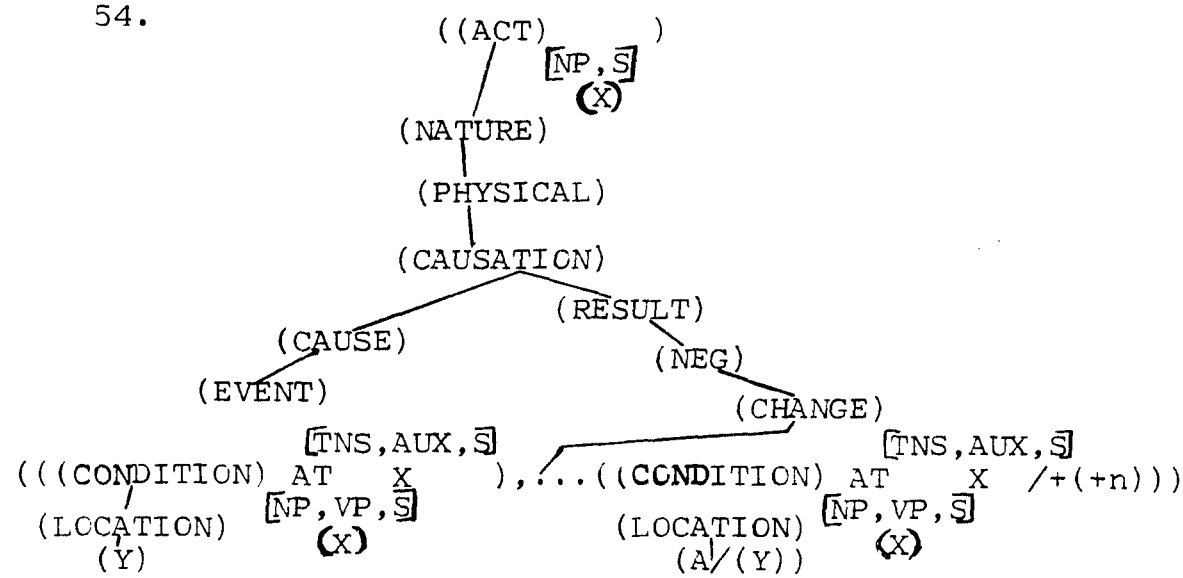
Such examples, it seems to me, are evidence for the following semantic marker for this exclusively causative sense of the verb "keep":

53.



To account for the causative-act sense of "keep" I suggest the following semantic marker:

54.



It seems, therefore, that the ability of the theory presented here to account for this putative counter-example in such an elegant and coherent way is strong indication of the explanatory adequacy of this theory.

The Psychological Evidence

In this section I will examine two aspects of the psychological evidence relating to causative verbs, one of which we have already discussed, namely Gergely and Bever's relatedness evidence. While Gergely and Bever's evidence relates to adult introspections concerning language, the second aspect of psychological evidence relevant to causative verbs that I will discuss, namely Bowerman's evidence,[15] relates to language development. I will first briefly review what I have said about the relevance of Gergely and Bever's evidence concerning causatives, and then go on to discuss Bowerman's evidence. I shall not discuss the FGWP evidence because, as we have seen, it fails the decidability requirement and is therefore not helpful in supporting either a decompositional or a non-decompositional view.

As suggested in Chapter IV, the psychological constraints on a competence theory are just that it should

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[15]Melissa Bowerman, "Learning the structure of causative verbs: A study in the relationship of cognitive, semantic, and syntactic development," in Papers and Reports on Child Language Development, No. 8, ed. E. Clark (Stanford: Linguistics Committee, 1974), pp. 142-78.

display the structures that a performance theory needs in order to make its predictions, which predictions are subject to be tested by the psychological evidence. In the case of Gergely and Bever's relatedness evidence I argued that whereas the definitional semantic marker account of word meanings provides their performance theory with the structures it needs to make its predictions about relatedness judgements, a monolithic, non-decompositional theory like the meaning postulate theory of FFG, FGWP, and Fodor, lacking the necessary type of internal structure, could not. In particular, whereas a semantic marker theory could provide the performance theory with a structure like the one in (13) of Chapter IV (page 89) in order to predict the effect on relatedness judgements of verbs like "persuade," a meaning postulate theory, lacking the strong hierarchical structure needed, could not provide the performance theory with the necessary distinctions in a non-arbitrary way. In this respect I claimed that Gergely and Bever's evidence weighs in favor of a definitional semantic marker theory as opposed to a non-decompositional theory. Hence, the only psychological evidence on adult linguistic introspections regarding sentences with causative verbs that satisfies our aboutness and decidability requirements can be accounted for with a decompositional theory of word meanings while it cannot be accounted for with a non-decompositional theory.

With respect to lexical development and causative

verbs the story seems to be the same. That is, here again in the other large general area of psycholinguistic evidence, there is support for a decompositional theory. This is noted by Gergely and Bever, who cite Bowerman's evidence in child language development concerning lexical causatives. And when we look at the semantic markers that I have presented here for the causative-act verbs as well as for the periphrastic sentences with the verb "to cause," we see that these structures are supported by the evidence set forth in Bowerman's paper.

Bowerman's paper begins by presenting some errors in word use of her "almost-four-year-old" daughter, Christie. These errors involve using a non-causative verb, an adjective, or a locative particle in a causative sense, and occurred between the ages of two years and one month and three years and eight months. Some examples of these errors are as follows:[16]

55. a. Daddy go me around. (=Daddy make me go around)
    b. Come her. (=make her come)
    c. But I can't eat her. (=make her eat; feed her)
    d. Full it up. (=make it full; fill it up)
    e. I am gonna sharp this pencil.
    f. Up your legs. (=make your legs go up; put your legs up)

In her initial discussion of these errors Bowerman points out that we could not explain this evidence on the basis of the child's confusion of transitive and intransitive uses of a verb since, for one thing, some of the

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[16]Ibid., pp. 143-45.

errors--for example, (55c) above--involve a mistaken transitive use of a transitive verb.[17] That is, the error is not in using an intransitive verb transitively, but rather in using a no-causative word causatively. In addition, a case grammar explanation of such errors as coming about as a result of mistaken case assignment to verbs that do not have a causative sense, on analogy to such verbs as "open," "break," and "warm" which do is also not borne out by the evidence. The case grammar explanation here would be that it is the presence of an agent in a sentence with one of these verbs that picks out the causative sense, and the absence of the agent which picks out the state or process sense. By analogy, according to the case grammar explanation, the child mistakenly uses an agent with a verb that has only the state or process sense in order to render a causative sense. Bowerman points out, however, that this explanation fails because it does not account for the directionality of the errors (non-causatives used as causatives and not the other way around), and it does not explain non-causative transitive verbs that do have agents--for example, "read," "listen," "look at." That is, if the child's hypothesis is supposed to be that the presence of an agent confers a causative sense, how are we to explain the child's correct use of transitive verbs with an agent that do not have a causative sense? Bowerman concludes that

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[17] Ibid., p. 147.

the best explanation is given by theories that say either that the meanings of the transitive forms of "open," "break," and so on are derived from the meanings of their intransitive forms, or that the meanings of the intransitive forms are included in the meanings of their transitive counterparts.[18] That is, a decompositional view can give an adequate account of the facts here. Bowerman specifically cites a generative semantics structure of causative verbs as being the sort of structure that might explain the evidence, notion first that CAUSE and BECOME are concepts characteristically included in the senses of causative verbs, and later that McCawley posited a structure including the concepts "DO CAUSE BECOME NOT ALIVE" for the verb "kill." In any event, Bowerman clearly finds a decompositional analysis to offer the best explanation of the developmental evidence.

Some of the more decisive aspects of the evidence supporting this view are as follows. First, there is a stage at which children "create novel causative verbs by analogy with existing ones."[19] Moreover, this stage is clearly marked and comes several months after children have started using causative verbs in an apparently correct manner. Bowerman argues that just as children use irregular verbs like "know" and "grow" first with their correct past forms and then later with the incorrect regular past tense ending,

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[18]Ibid., p. 149. [19]Ibid., p. 174.

they use causative verbs in an apparently correct manner first and then later suddenly begin making errors in using non-causatives causatively, In the case of errors with the past tense, emergence of "growed" and "knowed" is taken as evidence of learning the underlying morphological rule for forming the past tense of regular verbs. Similarly, errors in the mistaken use of non-causative verbs in a causative sense should be taken as learning the underlying semantic structure of causative verbs.

Bowerman notes that children learn to use causative transitive verbs long before they begin to make errors of the sort noted above with the use of non-causatives causatively, that over the course of this period there is no change in children's correct use of non-causative transitive verbs, and that during the initial period of correct use of causative verbs there are no multi-word sentences which explicitly mention "an act, a patient, and a stative or locative effect."[20] Bowerman interprets this evidence, particularly the latter, as supporting the view that children's initial correct use of causative verbs emerges from an unanalyzed, referential conception of their meanings, and that it is only when they master the underlying conceptual structure of causative verbs that they are able to make the analogies that result in the creative but incorrect use of non-causatives causatively. In addition, multi-

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[20]Ibid..

word, or so-called phrasal causatives emerge at the same time as the errors of non-causatives used causatively. This latter evidence strongly supports the view that errors like those in (55a)-(55f) above come about as a result of learning the internal decompositional structure of causative verbs.

Although Bowerman refers to a generative semantics type structure of the sort mentioned as possibly the underlying structure of causative verbs, the semantic marker structures for causative verbs presented in this chapter offer a better explanation of her evidence along similar lines. If we confine ourselves to the verbs mentioned in the evidence we see that, from the point of view of the semantic marker analysis of this chapter, there are really three structures that the child eventually learns to master: verbs whose meanings involve an act or activity but not the concept of causation, verbs whose meanings involve causation without the concept of an act, and verbs whose meanings involve both the concept of an act and the concept of causation. Thus we have the following three semantic marker structures:

56. a. (ACT)

b. (CAUSATION)

(CAUSE) (RESULT)

(CHANGE)

c. (ACT)

(CAUSATION)

(CAUSE) (RESULT)

(CHANGE)

Bowerman observes that she cannot answer the question as to what the child's initial concept of the meaning of causative verbs might be when they are first used apparently correctly, but, according to Bowerman, in an unanalyzed form.[21] However, our semantic marker analysis allows us to give at least part of the answer to this question. The initial unanalyzed forms of these early uses of causative verbs must be just the structure of (56a) above without any specification of causation. In other words, the child's first conception of the meaning of causative verbs is as some type of act, where the type is specified only contextually with respect to specific uses of specific causative verbs. Later, when the child learns the concept of causation, perhaps via periphrastic causatives, he or she is ready to reanalyze the meanings of causative verbs. But at this point the child does not know which of his (ACT) verbs are causatives and which are not--they all have the same basic semantic structure for him or her so far. The only natural way to find out which ones are causative and which ones are not is to use them all causatively. Hence the errors noted. As the child recognizes the mismatch between his or her more or less completely causativized vocabulary and the only selectively causativized adult vocabulary, he or she begins to give up the incorrect causatives and to keep the correct ones. In this

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[21] Ibid..

period of searching out the vocabulary to come up with the correct set of causatives, the child may rely on his or her linguistic concept of the causer-causee relation to pick out the correct uses. That is, if a verb is never used with reference to a causer or causee, then it is not causative--but if it is so used, then it is causative.

It may seem that one could work out a generative semantics explanation along the same lines as the one just offered from a semantic marker point of view. But one major and perhaps crucial difference between the ST analysis and the generative semantics analysis is that in the latter the concepts are each independent units within a syntactic structure, whereas in the former there is a hierarchical relation of superordination and subordination between the component concepts of the structure. Thus, the structures presented here for for the concept CAUSATION make the claim that CHANGE is part of the concept of CAUSATION. On the other hand, in the generative semantics framework it makes no sense to say that BECOME is part of the concept CAUSE. What this means is that the semantic marker structure makes stronger claims about the structure of the concept of CAUSATION, claims which the generative semantics framework cannot easily express. Moreover, as I have tried to illustrate, the claim that CHANGE is part of the concept of CAUSATION is borne out by a closer look at putative counter-examples like "keep."

Conclusion

It seems, then, that the best psychological evidence with respect both to child concept development as well as adult semantic performance supports a definitionally based psychological theory. At the same time, the ability of ST to predict and explain native speaker intuitions about the semantic properties and relations of sentences and expressions shows that a definitional theory of meaning is explanatorily adequate as part of a theory of grammar. Further, that a non-decompositional meaning postulate theory is essentially non-explanatory and incapable of distinguishing inferences based on meaning from inferences based on logical laws shows that a definitional account is preferable for a theory of grammar to a non-decompositional theory. Now this theory of grammar may be interpreted either conceptualistically as part of a theory of mind, or platonistically as a theory of abstract objects. In either case the definitional view is valid an preferable since both ontological positions are committed to accounting for native speaker linguistic intuitions and since the definitional view gives a better account.

In sum, then, the definitional view is valid and preferable from all three points of view outlined in Chapter I: from that of a psychological theory, from that of conceptualism, and, finally, from that of platonism.

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