

ON THE NOTION MODEL IN GENERATIVE GRAMMAR

by

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B. A., Kansas State University, 1973

A MASTER'S THESIS

submitted in partial fulfillment of the

requirements for the degree

MASTER OF ARTS


Department of Modern Languages

KANSAS STATE UNIVERSITY

Manhattan, Kansas

1974

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INTRODUCTION

The general aim of this paper is to make clear what one linguist and one philosopher mean by references to models in the context of transformational generative grammar, and involved with this is a concern with the substantial role that the notion model plays in current linguistic theory. The concept of model has become so fundamental to linguistics under the continuing influence of Chomsky that perhaps it is taken for granted, considered to be a given. This is one explanation which could account for the relatively little attention given this subject in the literature. Something which is held to be an assumption to be acted upon does not usually become at the same time an object of extended critical discussion. And, if this is the case, then anyone for whom linguistics is a newly acquired interest, and therefore who knows very little of the great changes in linguistics which took place in the late fifties and early sixties, is confronted with no small problem. The revolutionary advances which took place within linguistics, resulting mainly from the impetus of Chomsky, were so much so, simply because they were so very basic. Though I am certain that the concept 'model' had a great deal to do with these achievements, I am not at all sure that the material role which the notion 'model' had in this has ever been articulated clearly enough, especially for those who are approaching linguistics for

the first time. This is the main consideration which guides this effort. It is an attempt to make clear, or more accessible, something which is somewhat obscure.

Obviously, concern with models is a very technical and difficult area of philosophy and methodology of science. But, since I have situated the topic in this fashion, and defined the emphasis as making the subject more understandable to the uninitiated, the problems involved may be addressed in a less technical fashion. This perspective necessitates a simplification of some aspects of the general issue of models in science, but the intent of this study will be served. Having thus shown the orientation which I will take in regard to this issue, I turn now to the specific areas that will be covered in the body of the text which follows.

Section I is devoted to reaching a fairly simple answer to the question, "What is a model?" This question may be divided into two related subquestions: (1) What is the ordinary conception of a model? and (2) What is the scientific conception of a model? It is essential to consider the first question insofar as it is very instructive when juxtaposed with the much more complicated second question. Various senses and levels of what constitutes a model can be distinguished in question (2), and though these notions of model as used in science tend to be confusing, they do complement each other.

With this much accomplished, and with these senses of model in mind, Section II will consist first of a consideration of Chomsky's early theoretical innovations concerning grammars

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in linguistics, and secondly, of a look at the idea that a grammar should be considered as a model of a speaker's linguistic competence. The first part is important, because it is here that one can find the first clear example of how a particular model was used in the development of transformational generative grammar. I will discuss what was involved when Chomsky considered certain mathematical models as a basis for the construction of a grammar. The second part is relevant because here too a certain concept of model is at work. Of course, I believe that the senses of model which are distinguished in Section I will provide some insight when they are compared to both examples in this section. I will conclude this section and introduce the topic of the next by focusing on the second example of Chomsky's use of 'model,' bringing out some of the special problems which are involved in this particular application of a model, a grammar as a model of competence.

Jerrold J. Katz has written extensively on generative grammar and semantics, emphasizing always the significance which contemporary linguistics can hold for modern philosophy of language and for philosophy in general. In Section III, I will concentrate on one article he has written, "Mentalism in Linguistics." This article is relevant here, because Katz formulates the proper task and methodology of the linguist as he sees it, and in doing so, must at the same time speak with some authority on the notion of model in generative grammar. His conception of 'model' is interesting, but there appears to be a

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problem involved with this use of 'model.' I will attempt to show just what this problem is, discuss how it comes about, and, finally, suggest how it might be avoided.

The fourth and final Section of the paper will consist of a summary and a conclusion, and will be concerned with placing the various issues raised concerning these uses of model into an appropriate perspective.

I. Ordinary and Scientific Conceptions of Model

As I mentioned in the Introduction, the concept of model plays a prominent role in current linguistic theory, but what I failed to mention is that this important concept seems to be used almost indiscriminately by some linguists. So much is this the case, that often it is difficult to understand what they could mean when they talk of models.¹ Though I have partially eliminated this problem by choosing to deal with only two prominent conceptions of models in linguistics, this confusion about models has to be mentioned. I shall not attempt to account for this phenomenon here, and mention only that some do try to explain it by pointing to the fact that linguistics is a relatively young discipline still in the theoretical throes common to any developing science. This seems plausible, but I shall argue neither for nor against it.

If there does exist such an intensive use or even misuse

of the concept of model in linguistics, I must establish some basis or point of reference to which I can refer when examining Chomsky's and Katz' conceptions of model in Sections II and III. That this basis will be simplistic has been mentioned, but it should serve its purpose well enough. The question which should be answered in this section is "What is a model?", and this question can be approached on two levels. First, what is our ordinary notion of a model? And secondly, what is the scientific conception of a model? Or correspondingly, what do we commonly mean by a model, as opposed to what a scientist means by a model? That there will be large differences and large similarities in the answers to these two questions should be evident at the outset, but I do expect these contrasts to be helpful in arriving at which important features about models need to be distinguished.

This, then, is the first question to be considered: What is the commonsense conception of a model? What do we generally mean when we speak of a model, and what are the relevant features which characterize this sense of model? I think that there are at least four different uses or senses of model which we all commonly employ. They are as follow:

- (i) Model trains, boats, cars, airplanes, etc.
- (ii) Model citizens, cities, children, of decency, etc.
- (iii) Fashion models and artists' models.
- (iv) Experimental models, or new models of anything.

Each of these uses displays some features which distinguish it from the other three, but which can serve at the same time to relate it to them in a special way. These become apparent when

each of the senses of model is considered separately.

- (i) These sorts of models are replicas of the object modeled. Though different in scale, they are accurate representations of the real object which they are models of. They are the same as the object modeled, up to some point, and cannot be identical in every feature to it.
- (ii) This sense of a model is an ideal, or some perfect thing, which may be held up as an example, or for emulation.
- (iii) Models such as these have a definite function, in that they serve as an object for the artist or fashion buying public.
- (iv) These models consist of a certain type or design of some object. Experimental models, or prototypes, are built for design testing purposes, and when modified, will be copied when building the product they are models of. To speak of a new model of a car, for example, means that there is a new design, of which this particular car is an instance.

These examples are mundane, of course, but the important points which characterize the ordinary conception of a model do become apparent when attention is paid to the features distinguished in the examples above. The ordinary notion of model, as I see it, can be said to have these main characteristics: Analogy, Identity, and Utility.

1. Analogy--In the case of (i) and (iv) above, models are the same as, or similar to the objects they are models of. They are analogous to the objects they are modeling, and this analogy is never accidental; it is one of the salient features of a model.

2. Identity--A model is always a model of another object. It has to resemble, or be identical to the object modeled to a certain degree, but it can never be completely identical to the object modeled. If it were to be so, it could never be called

a model of that object. It would be absurd to speak of an analogy existing between two completely dissimilar objects, just as it would be to say that an analogy exists between two identical objects.

3. Utility--We can do things with models that one could never do with the real thing modeled. This can be extremely useful, as for example, in the case of experimental models (iv). A model will usually have some useful functions, owing of course to the fact that it is a model of something. The analogy which exists between a model and object modeled is the key to this.

Senses (ii) and (iii) above play virtually no part in the determination of the main characteristics of this notion of model. I also suspect that these particular senses will prove to be unrelated to a more scientific sense of a model. Of the senses I have distinguished, (i) and (iv) should bear most upon a scientific conception of a model. At this point it should be recognized that there is some connection between the ordinary and scientific conceptions.

Max Black has written an excellent article, "Models and Archetypes,"² which deals in some respects with this idea of an ordinary notion of a model. I turn to it now, and will mention several points he makes which apply to the discussion.

Black's stated purpose in writing this article is to bring out the presuppositions which underlie the use of models by scientists, and the implications which are involved with their use in science. Although the article is ultimately concerned with the use of models in science, he begins by giving a "clear

and uncontroversial example of a model" (p.219). But one wouldn't give for such an example some model used in science, and it is because of this that he comes to consider what I have called the ordinary conception of a model, or as he puts it, "examples of models in the literal sense of the word" (p.219). For him, "standard cases are three-dimensional miniatures, more or less 'true to scale,' of some existing or imagined material object" (p.219). He feels that other senses of model, such as 'a type of design' and 'an exemplar' (a model husband, a model solution of an equation, etc.), can be ignored, and I agree with him on this point. He believes the relevant sense of model which stands in need of explication is what he calls 'scale models,' which ". . . will cover all likenesses of material objects, systems, or processes, whether real or imaginary, that preserve relative proportions . . ." (p.220).

Black next lists what he considers to be uncontroversial points about scale models. A paraphrase of these would be inadequate, so I will quote him on this in full. He writes:

1. A scale model is always a model of something. The notion of scale model is relational and, indeed, asymmetrically so: If A is a scale model of B, B is not a scale model of A.
2. A scale model is designed to serve a purpose, to be a means to some end. It is to show how the ship looks, or how the machine will work, or what law governs the interplay of parts in the original; the model is intended to be enjoyed for its own sake only in the limiting case where the hobbyist indulges a harmless fetishism.
3. A scale model is a representation of the real or imaginary thing for which it stands: its use is for "reading off" the properties of the original from the directly presented properties of the model.
4. It follows that some features of the model are irrelevant or unimportant, while others are pertinent and essential to the representation in question. There is no

such thing as a perfectly faithful model; only by being unfaithful in some respect can a model represent its original.

5. As with all representations, there are underlying conventions of interpretation---correct ways for "reading" the model.

6. The conventions of interpretation rest upon partial identity of properties coupled with invariance of proportionality. In making a scale model, we try on the one hand to make it resemble the original by reproduction of some features (the color of a ship's hull, the shape and rigidity of the airfoil) and on the other hand to preserve the relative proportions between relevant magnitudes(p.22

As he says, the reason why we make models is that in reproducing in the model certain features of the thing modeled, we can better observe or manipulate these features. In terms of the model, they are made more accessible for us. Or as he states, "we try to bring the remote and unknown to our level of middle-sized existence" (p.221).

Black's next move in the article is to consider what he calls 'analogue models,' but this moves a step up the scale away from the ordinary notion of model that I am trying to establish at this point. What concerns me now is how to situate the points that have been raised thus far concerning the ordinary notion of a model. As can be seen, Black's analysis of scale models is somewhat sophisticated, but I am certain that one can draw conclusions such as he does only by a careful consideration of the more mundane senses of model. Though he does not reveal this process in the article, even the trivial senses of model play a part in determining the general concept of model at which he finally arrives. He is starting at the bottom, so to speak, in everyday existence and its concepts, and is working his way up through the various, more

complicated, and ever increasingly abstract notions of model, until he reaches the idea of a model as used in science.

Actually, one is hard pressed to make fine distinctions between the different senses of model, but doing this seems necessary for purposes of exposition. Even though particular distinctions may be somewhat artificially imposed on the total area of the characteristics of models, it is difficult not to accept the view that various senses of model should be described separately. If one objects to this "lower to upper level" description of models on the grounds that it tends to distort the real situation, the obvious reply is that matters are not very clear to begin with.

I make these points only to emphasize the relevance or importance that an ordinary notion of model can have when it is related or compared to a more scientific conception of a model. Surely there is a relationship between the mundane senses of a model and the higher level scientific conception of a model, but I shall not attempt to explain it here. Even after I have explored the various scientific senses of a model, this relationship would still prove to be very difficult to describe with accuracy. In any case, such a description would not contribute to the expressed intention of this section.

I mentioned Black's discussion of 'analogue models,' but before I take up this part of his discussion, I should situate it within the spectrum of various senses of model. I suppose that one could refer to this sense of model as "intermediate," insofar as it lies between the very mundane senses of model,

and the more abstract scientific senses. But, as we shall soon see, this type of model (as well as his 'scale model') shares several features or characteristics with a scientific conception of a model.

For Black, what distinguishes scale models from analogue models is that whereas scale models resemble, literally, the object modeled, and reproduce its features of interest, an analogue model is "some material object, system, or process designed to reproduce as faithfully as possible in some new medium the structure or web of relationships in an original" (p.222). Many of his comments above on scale models apply equally as well to analogue models, but again, an important difference exists between them. This difference has to do with the different methods in each case by means of which the model is interpreted. Scale models imitate the original, and this reliance upon identity is part of the technique of their interpretation. In constructing analogue models we aim at reproducing the structure of the original. This is a more abstract operation than goes on in the construction of replica models, and accordingly, this is reflected in the technique of interpretation. Black writes:

An adequate analogue model will manifest a point-by-point correspondence between the relations it embodies and those embodied in the original: every incidence of a relation in the original must be echoed by a corresponding incidence of a correlated relation in the analogue model. To put the matter in another way: there must be rules for translating the terminology applicable to the model in such a way as to conserve truth value. Thus, the dominating principle of the analogue model is what mathematicians call "isomorphism". . . . The analogue model shares with its original not a set of features or an identical proportionality of magnitudes, but, more abstractly, the same structure or pattern of relationships (p. 222).

Up to this point in the discussion, one characteristic which is central to models can be singled out. This is the notion of analogy. In her article, "Models and Analogies in Science,"³ Mary B. Hesse categorizes different kinds of models in terms of the notion of analogy. It would be instructive to take into account her treatment of analogy in science since she, too, views it as being central to models both in the ordinary sense and in the scientific sense.

First of all, Hesse distinguishes two kinds of analogical relations between model and object modeled: formal analogy and material analogy. Formal analogy has essentially the same characteristics as does Black's analogue model, in that there is "analogy of structure or isomorphism between model and system, deriving from the fact that the same formal axiomatic and deductive relations connect individuals and predicates of both the system and its model" (p.355). What she calls replica models exhibit not only formal analogy, but material analogy, which consists of the material similarities which obtain between the model and object modeled. It is possible to have formal but not material analogy between two systems, but "it does not seem possible to conceive of a material analogy without some formal analogy; if there is material analogy, there is presumably some consequent structural similarity that could--at least in principle--be formalized" (p.355). She contends further that a relation of either formal or material analogy must imply differences as well as similarities: "In analogous systems let us denote the set of similarities by the term 'positive analogy'

and the set of differences by 'negative analogy'" (p.355).

Both Hesse and Black devote a section of their articles to mathematical models, though neither seems to regard this type of model as being important as an example of a model as used in science. Mathematical models are indeed used extensively in science, and are popular among social scientists. As Hesse writes, "It has become common to speak of 'probabilistic models' of, for, example, psychological learning theory or population dynamics. In this context, 'model' refers to a mathematical theory containing the axioms of probability together with an interpretation of all or some of the nonlogical constants and variables of the theory into empirical observables"(p.355).

Black is quite critical of mathematical models, and writes, "When used unemphatically, 'model' in such contexts is often no more than a pretentious substitute for 'theory' or 'mathematical treatment'" (p.223). He admits that there are advantages to be gained from using such models, but his strongest criticism is aimed at the "serious risk of confusing accuracy of the mathematics with strength of empirical verification in the original field" (p.223). He writes further:

Especially important is it to remember that the mathematical treatment furnishes no explanations. Mathematics can be expected to do no more than draw consequences from the original empirical assumptions. If the functions and equations have a familiar form, there may be a background of pure mathematical research readily applicable to the illustration at hand. We may say, if we like, that the pure mathematics provides the form of an explanation, by showing what kinds of function would approximately fit the known data. But causal explanations must be sought elsewhere. In the inability to suggest explanations, 'mathematical models' differ markedly from the theoretical models now to be discussed (p.223).

The next part of his article deals with theoretical models, and the point has been reached in this paper where these should be considered. Black takes this sense of model to be the prominent one to be extracted from science, and Hesse concurs on this. I will rely on Hesse's comments in the first part of the discussion, and will return to Black's consideration of theoretical models after that.

First of all, Hesse distinguishes 'simplifying models,' which are the kind of model sometimes associated with scientific theories, but which should not be considered as theoretical models proper. Simplifying models are "systems that deliberately simplify and even falsify the empirical situation under investigation for purposes of convenience in research or application" (p.355). Also included in this category are 'archaic models,' which have been developed in now falsified theories but which still have some use as convenient approximations in applied rather than pure science" (p.355). A simplifying model can be very useful, then, but only because it possesses a degree of positive analogy sufficient to lead to correct conclusions despite the large presence of negative analogy.

Theoretical models are "much more intimately associated with the structure of theories than simplifying models. Roughly speaking, these are models that appear--at least on first sight--to be identical with the relevant theory. . ." (p.355). Examples of this seeming identification are numerous. We speak indifferently of both the corpuscular model and the corpuscular theory of light. Bohr's theory, which accounted for certain

quantum phenomenon, is referred to as Bohr's model of the atom. And, similarly, cosmologists use the term "world model" interchangeably with theories of the structure of the universe. The difference in these cases between the use of 'model,' as opposed to 'theory,' can be seen as dependent upon "the degree of acceptability of the theory. Thus, Bohr's theory, which is a rather radical departure from previous physics, or a theory of light that is not fully established and to which there are viable alternatives, may be called a model. It would however, be odd to speak today of a wave model of sound, since a theory of sound in terms of wave motion is fully established and is even regarded as factual rather than theoretical" (p.355).

There are other, more important considerations which lead many philosophers to distinguish between theoretical models and the theories which they are models of. To illustrate these considerations, Hesse outlines the outstanding features of theoretical models. "First, these frequently are models in something like the logical sense of being interpretations of a formal or semiformal theoretical system from which the phenomena are deducible" (p.356). For example, if some archaic model of X had turned out to be acceptable in a modern, more advanced theory of X, then "it would have also been an interpretation of the formal structure of" X (p.356). This interpretation could be analyzed in terms of formal analogy, as it is outlined above.

Another salient feature of theoretical models is that they "provide explanation in terms of something already familiar

and intelligible" (p.356). The model is dependent on, and understandable in terms of some familiar system which is "epistemologically prior to and independent of the particular phenomenon that the model is invoked to explain" (p.356). This feature will be implicit in any attempt to explain unknown or obscure phenomena in terms already known and understood. Hesse's comments on this are very clear. She writes:

. . . the less well established system under investigation . . . may be called the explanandum. What chiefly distinguishes theoretical models from other kinds is a feature that follows from their associating another system with the explanandum. This is that the theoretical model carries with it what has been called "open texture," or "surplus meaning," derived from the familiar system. The theoretical model conveys associations and implications that are not completely specifiable and that may be transferred by analogy to the explanandum; further developments and modifications of the explanatory theory may therefore be suggested by the theoretical model. Because the theoretical model is richer than the explanandum, it imports concepts and conceptual relations not present in the empirical data alone (p.356).

But it is true that most types of models carry surplus meaning, in terms of positive or negative analogy. However, we can distinguish theoretical models from other types by virtue of the fact that a theoretical model can be "used in a way that exploits this surplus meaning in prediction and explanation" (p.356). Too, any model which is derived from a familiar mechanism or system will contain a certain amount of negative analogy. For example, Hesse points out that in the "billiard ball" model of gases, we tacitly ignore certain features of the billiard balls such as size, the number inscribed on them, or color. We exclude their negative analogy when using the

model. Since this is the case, we can make the distinction "between the model as exhibited by the familiar system and the model as it is used in connection with the theory. The latter is a conceptual entity arrived at by stripping away the negative analogy, and it is only this that can plausibly be identified with the theory" (p.356).

With these points in mind, the relationship between a theory and a theoretical model can be stated generally in the following fashion. Initially, a model is advanced because there exists in it some straightforward positive analogy with the explanandum. The negative analogy which exists in the model is ignored, resulting in a theoretical model, a conceptual entity; but this theoretical model does not consist just of the remaining positive analogy with the explanandum, for the theoretical model must be richer than the explanandum. This is open texturedness, or "neutral analogy" and "exploitation of the model consists in investigating this neutral analogy and in allowing the neutral analogy to suggest modifications and developments of the theory that can be confirmed or refuted by subsequent empirical tests" (p.356).

These remarks essentially cover the features of theoretical models which Hesse has found to be most relevant. I will turn now to Black's treatment of this type of model, focusing on the points he makes regarding theoretical models which differ substantially from Hesse's, although his treatment of theoretical models generally augments her discussion. Black first of all makes a useful distinction which reflects opposing views of the

proper use of theoretical models. Some scientists have regarded theoretical models as heuristic fictions, while others have seen them in an "existential" manner. The former can be described as the "as if" interpretation of theoretical models, and the latter as the "as being" interpretation. In an "as if" interpretation, the scientist views the explanandum as if it has the features displayed by a model, or as Black puts it, with "a willing suspension of ontological disbelief" (p.228). An "as being" interpretation, on the other hand, commits the scientist to viewing the explanandum as being identical with a model's features.

As can be imagined, whether one views models as very helpful devices which aid understanding of an obscure matter, or as being statements of empirical reality, involves one in much controversy and debate. There are, however, points concerning these opposing viewpoints which can be safely made. As Black understands it, there is a price to be paid with each interpretation. "As if" thinking can result in a real lack of explanatory power. "In risking existential statements, however, we reap the advantages of an explanation but are exposed to the dangers of self-deception by myths . . ." (p.228). Nonetheless, the 'existential' use of models seems to have been a characteristic mark of the great theorists in science; models have been viewed as more than expository devices.

But, whether or not one opts for the 'fictitious' or 'existential' interpretation of theoretical models, one feature that a theoretical model does not share with Black's 'scale models'

and 'analogue models' clearly emerges. Whereas scale models and analogue models must actually be constructed, a theoretical model is essentially a conceptual system or entity. It is not literally constructed, and as Black says, ". . . the heart of the method consists in talking a certain way" (p.229). Some writers claim further that using a theoretical model consists in extending the language connected with a familiar theory to a new domain of application. Black makes the point here that even this new language application describes some object or system, which is the model itself, and that, therefore, one cannot justifiably speak only of language in this connection.

We could properly say then that a theoretical model is described, not constructed; but, there are difficulties which go along with this apparent freedom from the limitations of construction. One set of limitations and disadvantages can well be traded for another. Scale and analogue models usually contain irrelevant features which the creation of a model by abstract means can avoid, but, according to Black, there exist certain desirable controls in an actual attempt at construction of a model. Without these guiding controls, a danger exists that the creator of such a model may fall into a situation with the model where one cannot tell clearly what the reality of the model is meant to be.

Finally, Black lists several conditions for use of a theoretical model. Since he is so succinct, I once more shall not give an exposition of his remarks, but will allow him to speak for himself. These comments show clearly what is involved

when a theoretical model is used in science. He writes:

1. We have an original field of investigation in which some facts and regularities have been established (in any form, ranging from disconnected items and crude generalizations to precise laws, possibly organized by a relatively well-articulated theory.)
2. A need is felt, either for explaining the given facts and regularities, or for understanding the basic terms applying to the original domain, or for extending the original corpus of knowledge and conjecture, or for connecting it with hitherto disparate bodies of knowledge--in short, a need is felt for further scientific mastery of the original domain.
3. We describe some entities (objects, materials, mechanism, systems, structures) belonging to a relatively unproblematic, more familiar, or better-organized secondary domain. The postulated properties of these entities are described in whatever detail seems likely to prove profitable.
4. Explicit or implicit rules of correlation are available for translating statements about the secondary field into corresponding statements about the original field.
5. Inferences from the assumptions made in the secondary field are translated by means of rules of correlation and then independently checked against known or predicted data in the primary domain (p.230).

As can be seen, a theoretical model and its domain exhibit the same sort of relations as those between an analogue model and its original. Identity of structure, or isomorphism, is the essential relation which allows for assertions made about the secondary domain to apply equally as well to the primary domain under investigation.

Black's discussion of theoretical models turns at this point away from their distinguishing features to questions involved in the controversy regarding their use in science. Certainly, whether or not these models should be used in science is a moot point, and those who argue that models are "props for feeble minds," would do well to note the number of minds so supported in the history of science. Black takes the

position that models "play a distinctive and irreplaceable part in scientific investigation . . ." (p.236). Actually, no one could deny that models have a definite heuristic function in science, but this is not the point of contention. As Hesse writes, "The main philosophical debate about models concerns the question of whether or not there is any essential and objective dependence between an explanatory theory and its model that goes beyond a dispensible and possibly subjective method of discovery" (p.356). That models do play an important role in scientific investigation is acknowledged by most writers, and whether or not this role is a proper one is open to debate. However, this question lies outside the main considerations of this section, and I mention it only because it is somewhat related to the features which characterize models in general.

It would be superfluous to conclude this section with some sort of summary or recapitulation of the entire discussion, since it is so brief. The questions raised at the beginning of the section concerning the nature of models have been answered well enough, I think, to serve as the point of reference I intended this section to be for what follows. The various senses of what a model is are so interconnected that perhaps all of the points covered in this section should be kept in mind when Chomsky's and Katz' notions of 'model' are discussed. If we limit ourselves to one sense of model for reference in the following discussions, it should be the theoretical model, since this is the model most closely associated with scientific investigation.

II. Chomsky and Models.

This section consists of two parts, the first concerned with Chomsky's early work in generative grammar, where he considered certain mathematical models as being the possible basis for the construction of a grammar of a language, and the second concerned with his notion that a grammar is a model or a theory of linguistic competence. Accordingly, I will consider these two areas under separate headings.

1. Mathematical Models for the Description of Language.

There is a background or rationale which supports Chomsky's early work with mathematical models which needs to be made clear at the outset. Or to put it another way, we should examine first what considerations led him to be concerned with mathematical models, before we look at what he did in this connection. Actually, the rationale behind this work is at least as important as the work itself since this is what reveals the purpose behind his concern with these models, and the context of this paper demands this sort of explanation rather than a straight exposition. This is the important question to be dealt with here, and raises what in my mind is one of the problems with comprehending generative grammar. Too often it seems that a clear background is lacking in the work of Chomsky and others. One is presented always with completed works, and never gets

to see the steps which led to the final product. I must emphasize seems here, because Chomsky has made repeated attempts to make it absolutely clear what he is up to, because of the fact that many of the criticisms leveled against his work miss the very points that he takes for granted. I suppose that it could be argued that the sort of explanation that his critics are really in need of has yet to be offered by him. This section could be regarded as a beginning of the kind of explanation that is needed.

In light of the comments above, an answer to the question of why Chomsky was concerned with mathematical models will involve first of all an explanation of why he views a grammar as a generative device. Chomsky claims that generative grammar finds its historical basis in traditional or classical grammar and with 17th century rationalist grammarians and their views on language. It is therein that the notion that a grammar should be a generative grammar lies.

There are several concepts involved with this which must be explored. One important aspect of traditional grammar that generative grammar has adopted is the distinction between linguistic competence and performance. As Chomsky writes, "A distinction must be made between what the speaker of a language knows implicitly (what we may call his competence) and what he does (his performance)."⁴ Or as he writes elsewhere, competence is "the speaker-hearer's knowledge of his language," and performance is "the actual use of language in concrete situations."⁵

Under the traditional view a grammar is an account of competence insofar as it "describes and attempts to account for the ability of a speaker to understand an arbitrary sentence of his language and to produce an appropriate sentence on a given occasion."⁶ A grammar can be pedagogic, in that it attempts to convey to a language learner competence in a language, or it can be linguistic insofar as "it aims to discover and exhibit the mechanisms that make this achievement (competence in a language) possible."⁷ A linguistic grammar can be expressed as a system of rules which correlate sounds (or signals) with meaning (or semantic interpretations). It has been the traditional problem in grammar and linguistics to discover and formulate these rules, even though this concern may have been sometimes disguised and even ignored in much of the first half of this century.

If we accept this distinction, between competence and performance, and agree that a speaker's competence is what stands in need of investigation, resulting in a grammar which accounts for it, then how should this investigation proceed, since it seems clear that we cannot directly observe anyone's linguistic competence? Performance provides the data from which we generalize to a system of rules in the grammar which accounts for competence. So far as this is so, we can say that competence underlies performance, even though performance can never directly reflect competence. Chomsky has emphasized this point a great deal, as he must, and it does seem obvious that one must view performance in such a fashion. Actually,

linguists have always idealized from the data of performance in much this manner no matter what type of grammar they were constructing.

We might ask at this point what is wrong with the traditional and more recent grammars so constructed by such an idealization, since the same data of performance has been used as a basis for the generalizations made in each case. Again, Chomsky is clear on this point, and answering this question involves a further consideration of competence.

When one considers competence an important aspect emerges. This Chomsky calls creativity, which is the ability of the speaker of a language to produce and understand an infinite number of completely new sentences. In Wilhelm von Humboldt's words, language (or the speaker of a language) "makes an infinite use of finite means." Given a finite number of linguistic elements, a speaker is able to utter and to understand an indefinite number of original sentences. This notion of creativity is of fundamental importance for generative grammar and it has succeeded in accounting for this aspect of language where traditional and recent linguistics have failed to do so.

Returning to the question raised above, what is wrong with the grammars produced by both traditional and more modern approaches in linguistics is that they have not dealt sufficiently with the notion of creativity in language. There are several reasons for this failure.

Traditional grammarians, even though they recognized the importance of creativity, produced grammars that appealed

basically to the intelligence of the reader of the grammar. Someone who used such a grammar had to depend upon his own linguistic knowledge or ability more than on the grammar. Even though these grammars compiled a wealth of information about the structure of sentences, the various uses of words, etc., this information was never set down in such a way as to allow the reader of the grammar to produce utterances in the language without appealing to his own linguistic abilities. In other words, a grammar should be formulated in terms of rules so explicit and so organized as to allow the grammar itself to "produce" correct sentences of the language. Only some sort of explicit, sentence generating grammar can possibly account for creativity in language. An "infinite use of finite means" implies that a grammar must somehow incorporate recursive devices or generative mechanisms into its structure. Traditional grammarians were unable to do anything like this, for recursive devices or generative mechanisms were simply unheard of at that point in history.

Traditional grammars were inadequate, then, for a technical reason. As Chomsky writes, "Although it was well understood that linguistic processes are in some sense 'creative,' the technical devices for expressing a system of recursive processes were simply not available until much more recently. In fact, a real understanding of how a language can (in Humboldt's words) 'make infinite use of finite means' has developed only within the last thirty years, in the course of studies in the foundations of mathematics."⁸ Traditional grammar raised, but did not

solve, certain problems which dictated that a grammar of a language must be generative. When such generative devices became available, there was no longer any technical barrier to a study of generative grammars, and the problems raised could be attacked in an explicit fashion. Thus, Chomsky's early contribution in linguistics was to investigate certain mathematical systems, generative systems, which could serve as the basis for the construction of a generative grammar. I will return to this after considering the second part of the question raised above. Why are the grammars which modern structuralist linguists constructed inadequate, insofar as performance is the data upon which they are based, as is the case in all grammars? What is wrong with them is that they too did not treat with any degree of explicitness the creative aspect of language and suffer from the same sort of limitations as do traditional grammars. But more than this, contemporary structuralist grammars did not even recognize such concepts as competence or creativity. These notions have come down to us only in the work of traditional grammarians. This being so, they suffer from a defect in principle, as Chomsky says, and no amount of empirical investigation could ever lead to the construction of a generative grammar. The basis of a generative grammar is implicit in the notion of creativity, and recent linguists more or less ignored this notion completely.

Recent work which did deal informally in some respects with the creativity of language tended to deal with this by referring to language use as a matter of 'habit,' and to the

formation of new sentences as a matter of 'generalization.' Chomsky argues that "there is no sense of 'habit' known to psychology in which this characterization of language use is true (just as there is no notion of 'generalization' known to psychology or philosophy that entitles us to characterize the new sentences of ordinary linguistic usage as generalizations of previous performance.)"⁹ If these easy characterizations are used in a literal sense they are untrue, so they must be considered as metaphors, and as misleading ones at that, because they "tend to lull the linguist into the entirely erroneous belief that the problem of accounting for the creative aspect of normal language use is not after all a very serious one."¹⁰ Having thus briefly looked at the background of considerations which led Chomsky to evaluate mathematical models in relation to grammar, I turn now to this work itself, and will sketch what was involved and what relevance, if any, the discussion of models in Section I can have to it.

As has been pointed out, the recursive mechanisms of syntax have been studied recently only owing to the development of the understanding and techniques of formal processes and the nature of algorithms in mathematics. The study of these mechanisms in relation to language is referred to as "generative grammar," and the generative grammar of a language is understood "as the system of rules and processes that characterize the potentially infinite class of sentences of a natural language and that assign to each of these objects a structural description representing its significant phonetic, syntactic, and semantic properties."¹¹

The earliest seminal work which Chomsky did was to investigate three finite mathematical models which exhibited the required generative capacity in order to ascertain which of them could serve as the basis for the construction of an adequate formal grammar of a language. I will briefly consider this work, relying on two sources: "Three Models for the Description of Language,"¹² and Syntactic Structures.¹³ Before considering each of the models, a passage clarifying exactly what Chomsky conceives the grammar of a language to be will be helpful. He writes:

The grammar of a language can be viewed as a theory of the structure of this language. Any scientific theory is based upon a certain finite set of observations and, by establishing general laws stated in terms of certain hypothetical constructs, it attempts to account for these observations, to show how they are interrelated, and to predict an indefinite number of new phenomena. A mathematical theory has the additional property that predictions follow rigorously from the body of the theory. Similarly, a grammar is based on a finite number of observed sentences (the linguist's corpus) and it "projects" this set to an infinite set of grammatical sentences by establishing general "laws" (grammatical rules) framed in terms of such hypothetical constructs as the particular phonemes, words, phrases, and so on, of the language under analysis. A properly formulated grammar should determine unambiguously the set of grammatical sentences.¹⁴

As he says elsewhere, a language (either natural or artificial) can be defined as a finite or infinite set of sentences, each of finite length, each constructed from a finite set or alphabet of symbols or elements, and the grammar of a language is "a device that generates all of the grammatical sequences of a language and none of the ungrammatical ones."¹⁵

The best way to look at these mathematical models is to consider each separately, but I will give only a very brief

characterization of each model and the reason for Chomsky's rejection or acceptance of it, because there are more important questions concerning the entire procedure which should be dealt with. This is in keeping with the intention of the paper, which again, is to consider the use and role of 'model' in linguistic theory. Also, these questions require a comparison or relation of the various senses of a model distinguished in Section I with Chomsky's use of the term.

1. Finite State Model--"The most elementary grammars which, with a finite amount of apparatus, will generate an infinite number of sentences, are those based on a familiar conception of language as a particularly simple type of information source, namely, a finite-state Markov process."¹⁶ This theoretic communication model of language is extremely powerful and general, but it turns out to be impossible to construct a finite state grammar of English for the simple reason that English is not a finite state language. Any attempt to account for the structure of English based on this conception of linguistic structure will have to fail. This conception of grammar which has been rejected "represents in a way the minimal linguistic theory that merits serious consideration. . . . A limited linguistic theory is not adequate; we are forced to search for some more powerful type of grammar and some more 'abstract' form of linguistic theory."¹⁷

2. Phrase Structure Model--There had been much syntactic description prior to Chomsky's work, which was formulated in terms of immediate constituent analysis, or parsing of sentences.

The formalized grammar which is based on this conception of syntactic structure is called a phrase structure grammar. This type of grammar is more powerful than a finite state grammar, because it can handle sentence structures that a finite state grammar cannot. But, phrase structure grammars are successful only when applied to a limited number of sentences with a simple structure, and is therefore inadequate in regard to all sentences of English and their structural interrelationships.

3. Transformational Model--An even more powerful model is needed to remedy the inadequacies of phrase structure grammar, and this model consists of a combination of phrase structure and certain rules called grammatical transformations. Obligatory transformations apply to the output (terminal strings) of the phrase structure rules, yielding kernel sentences; and, these kernel sentences are operated on by optional transformations to yield a set of derived sentences. A grammar of this sort is adequate in respect to all levels and complexities of sentence structure, and is therefore seen as the most promising grammar for further investigation of the structure of language.

My account of these three models above makes no pretense to technical adequacy and may even be misleading in ignoring the great complexities involved with Chomsky's analysis. For the purposes of this paper, though, just a mention of the actual technicalities is sufficient because the discussion which follows deals in a very general manner with these mathematical models. A general question can be asked regarding Chomsky's work with

these models: In what sense is his investigation of these three models consistent with the use of models in science as outlined in Section I? This question can further be broken down into three interrelated questions: (i) In what sense are the three models above models, as outlined in Section I? (ii) How can these mathematical systems be said to serve as models for the construction of a grammar? (iii) Can a finite state, phrase structure, or transformational model be properly referred to as a model of linguistic structure? Before considering each of these questions in turn, some general remarks concerning the relation of mathematics to linguistics, and models and language in general, will help establish a setting for their discussion.

There is a branch of linguistics known as mathematical linguistics which, naturally enough, uses mathematical concepts and techniques in investigating language, and thus far, certain kinds of mathematical structures have been found adequate for the description of language. Zellig Harris has done much work in this area and his book, Mathematical Structures of Language,¹⁸ while not a survey of this field, does serve as a cogent example of the sort of things mathematical linguistics is concerned with. He writes in the preface:

This book attempts to show how one can arrive at an abstract system which characterizes precisely natural language. This is done by taking the data of language and finding within the data such relations as can be organized into a suitable model. The problem here was not to find a broad mathematical system in which the structure of language could be included, but to find what relations, or rather relations among relations, were necessary and sufficient for language structure. Then anything which is an interpretation of the model can do the work of natural language.

Harris claims that language as a whole can be analyzed from the body of data consisting of the recognition that "certain sound sequences, out of some large sample, are utterances of the language (with normal acceptance, or less) while others are not, and that certain ones are repetitions of each other"(p.1). Structural linguistics has already done much work toward formally characterizing these utterances.

The concern here is not to define a mathematical system which has some relation to language, but to formulate "as a mathematical system all the properties and relations necessary and sufficient for the whole of natural language"(p.1). As regards the progress of this approach generally, he says that so far, the main result has been in "defining an abstract system that fits language, and in constructing a few further systems as extensions of this, rather than in proving theorems about the abstract systems. It may be expected, however, that theorems which are proved about these systems would be interpretable as true properties of language, since the systems are built to describe neither more nor less than natural language"(p.1).

These remarks could well be considered with a clear conception of 'model' at hand, since there is much implied here as to the function of a model. As pointed out in Section I, both Black and Hesse play down the importance of mathematical models in science, insofar as these type of models do not suggest causal explanations, but provide rather the form of an explanation. In other words, initial empirical investigation of the explanandum suggests an explanation which can advantageously

be couched in terms of a mathematical system, the system thereby proving a form for the explanation which resulted from the initial empirical investigation. That the mathematical system plays an important role here cannot be denied, but if it is to be referred to as a model, it must also be recognized that it may not play the standard role that a theoretical model in science does; i.e., aiding in the initial investigation, and suggesting the substance of an explanation.

It seems that this characterization of a mathematical model can apply to Harris' explanation of mathematical linguistics. As he says, the 'data' of language exhibits certain relations which can be organized into a suitable 'model.' The data of a language is understood as the characterization of acceptable and unacceptable utterance of a language, and this data comes from empirical investigation. The data does contain certain relations or a structure which can be formalized by using an appropriate mathematical system for display. These relations are, in effect, the explanation of language and the mathematical system provides the form of the explanation. The point to be made is that the mathematical system or 'model' does not aid in the discovery of these relations. What is criticized here is not what mathematical linguistics actually does, but how the procedures involved are referred to, the use of the term "model" being the case in point.

A few remarks concerning models and language are appropriate at this point, and should help situate the problem to be taken up next. I.I. Revzin, in Models of Language,¹⁹ writes on the

status of scientific methodology in linguistics. As he sees it, linguistics is both an inductive and a deductive science in that it employs both methods in arriving at its abstractions. First of all, it must be inductive; generalizations must come from the data of actual speech or language. But it must also rely on deductive methodology, owing to the fact that an infinite number of different speech acts precludes the possibility "of formulating the basic concepts of the Science of Language by generalizations from induction" (p.2). He writes further:

In its deductive part, Linguistics, it seems, can be constructed just as Logic or Mathematics are constructed; a certain minimal quantity of primary indefinable terms is established, and all the rest of the terms are defined by means of the primary ones. At the same time certain primary statements as to the connections between these terms (axioms) should be clearly formulated and all other statements should be proved, i.e., reduced to certain other statements (p.3).

Revzin claims that much of the recent dispute in Mathematical Linguistics has been about whether or not Linguistics can successfully be constructed along these lines, and that it is not yet clear "how all the great variety of the rather contradictory facts of speech reality, as they are described in the many grammars of actual languages, may be placed in the framework of a strict deductive theory" (p.3). We do have tools which can aid us in this; we can construct models, and these models can help in discovering the "complicated phenomena of actual reality" (p.3). He is quite specific as to how such models are to be constructed. First of all, certain primary linguistic concepts are selected, and the relations between them are adopted as postulates. All of the remaining statements

follow on a deductive basis in terms defined by means of the primary concepts.

A model so constructed will be a hypothetical construct, an abstract object not dependent on the nature of its elements. The model must be distinguished from its 'linguistics interpretation,' which is arrived at by substituting linguistic items for corresponding mathematical terms in the model. The linguistic interpretation of the model must be investigated, or else the model remains a "pure fiction." "Indeed, proof obtained by the deductive route, however irreproachable from the logical point of view it may be, still tells us nothing about the actual language described by the model. Only if definite correspondences between the original and the model obtain, can we speak of the convincingness of the model" (p.4). As Revzin describes it here, this process of constructing models evokes many of the senses of a model as outlined in Section I.

The three questions raised earlier concerning Chomsky's investigation of three mathematical models as a basis for the description of language can now be considered.

(i) In what sense are the three models models, as outlined in Section I? Finite state, phrase structure, and transformational models provide a certain mathematical framework which is appropriate for describing language, given the creative aspect of language use. Language makes an infinite use of finite means, and these mathematical systems exhibit just such a generative capacity. They are models insofar as primary linguistic concepts can be "plugged into" their framework. It is only

because initial empirical investigation yielded such primary concepts, and exposed the basic structure of language, that these mathematical systems came to be seen as appropriate 'models' for the description of language. It seems that much of what Black says about mathematical models is relevant here; these 'models' can provide the form of an explanation, but do not lead to any explanation themselves. As such, these 'models' are not models in the scientific sense of a theoretical model, and to use the term "model" here is misleading.

But there is another sort of response that can be made to this question, one in which these models should be considered models in just the classic scientific sense. One might say that though it is true that empirical investigations resulted in the articulation of primary linguistic concepts and revealed the basic structure of language, these investigations were basically rudimentary, and left much unaccounted for, in spite of their insights. The use of appropriate mathematical systems as models serves to enhance or facilitate the further investigation of linguistic structure, and does not provide simply the form of an explanation already arrived at by other methods; in fact, there have been no clear explanations, in any strong sense of the word, up to this point in linguistics. Chomsky would claim further that the distinction between linguistic competence and performance dictates an initial investigation of competence, but since the data of performance underdetermines competence, we are forced to use models as part of the methodology of investigation. These particular models

are the only kind which are candidates for consideration, and the transformational model is the only one which is adequate, given what we know about competence and linguistic structure. This is a use of a model in the classic sense, and does not consist simply of dressing up results of linguistic investigation in the cloth of a mathematical system.

(ii) How can these mathematical systems be said to serve as a model for the construction of a grammar? The answer to this question is fairly clear. There is the obvious sense of model where the mathematical systems serve as the "blueprint" for the construction of a grammar. Too, these mathematical systems can serve as a model for the construction of a grammar insofar as they provide the framework in which the linguistic terms of a grammar are formulated. These uses of these mathematical systems would not fulfill the conditions of use of a theoretical model.

A grammar could be considered to be a 'linguistic interpretation' of the mathematical model, as Revzin puts it, which means that the model is a hypothetical construct arrived at by generalization from linguistic facts, and the grammar is arrived at by substitution of linguistic terms for the mathematical ones. In the case of Chomsky's use of these mathematical 'models' however, this analysis does not apply. Revzin considers a model to be a construct based on linguistic facts, and these mathematical systems were not constructed on the basis of linguistic investigation. They were developed in the field of mathematics, completely apart from any linguistic considerations.

Since this is so, what Revzin says about models cannot apply to Chomsky's investigation of these three mathematical systems or 'models.' Apart from attempting to apply Revzin's analysis, these mathematical systems are clearly not models in the sense of a theoretical model, and the only way in which they can be considered as 'models' for the construction of a grammar is in the sense that they are "blueprints," or something to that effect. I must emphasize that this question as to the use of the term "model" is quite distinct from the others raised.

(iii) Can a finite state, phrase structure, or transformational model be properly referred to as a model of linguistic structure? Revzin's program for the construction of models of language cannot be followed in this case, because these mathematical systems are not constructed because of the articulation of primary linguistic concepts; they exist apart from linguistic investigation. Perhaps these systems are interpretable as models, and this would consist of substituting linguistic terms for mathematical terms in the system. In any case, to call this process "using a model" would be applying the term "model" in a very weak sense. This would not be a case of a theoretical model being constructed.

These mathematical systems can become models however, and this occurs when they are used in the further investigation of linguistic structure. These systems do not have to be constructed by means of initial linguistic investigation, in the sense that Revzin has in mind, but simply exploited in subsequent investigation.

In what sense, then, is Chomsky's investigation of these mathematical systems, or 'models,' consistent with the use of models in science as distinguished in Section I? Each of the three questions above has touched on this question, though in an equivocal manner. It is difficult to answer this question in a completely unambiguous fashion, because Chomsky's work with these three mathematical models is subject to much interpretation. I am inclined, however, to respond to this question that Chomsky's use of the term "model" in connection with these three mathematical systems is generally consistent with the notion of a theoretical model as distinguished in Section I, but not so much so with other sense of model outlined there. There are several reasons for this.

It appears that most of the characteristics or conditions for use of a theoretical model are shared by Chomsky's use of model. This correspondence can be outlined briefly as follows:

(a) There is an original field of investigation (linguistic competence) in which some facts and regularities have been established (primary linguistic concepts, structural syntactic relationships, etc.).

(b) There is obviously a need for further scientific investigation of this original field.

(c) A well known and understood system (finite state, phrase structure, or transformational mathematical system) is exploited in order to better understand the less well established system under investigation (competence).

(d) Statements about the secondary system (mathematical

systems) are translatable into statements about the original field (linguistic competence).

(e) The mathematical system exploited in studying linguistic competence has 'open texture,' or 'neutral analogy,' and is richer in concepts and relations than the explanandum. This surplus meaning suggests further developments and routes of investigation, and associations and implications present in the mathematical system or 'model' are transferred by analogy to the explanandum. Concepts and conceptual relations not present in the empirical data of language are brought to bear by the more familiar mathematical system, and when it is thus exploited it becomes a model.

If Black is right in asserting that the heart of a theoretical model consists in 'talking a certain way,' then a transformational model in linguistics is very much a theoretical model. The only difficult problem I see concerns the concept of isomorphism, which guarantees that statements made in the secondary system can apply equally well to the primary system under investigation. It is simply not very clear how isomorphism is involved with these mathematical systems, though it must be part of the process. I will return to this problem below.

2. A Grammar as a Model of Linguistic Competence.

This is the second area of concern raised at the beginning of this section, and there are some problems connected with it which should be discussed. Before I do this however, I will

give a clear statement of how Chomsky views the relationship between a grammar and competence.

First of all, what is linguistic competence? As stated earlier, what a speaker knows implicitly is competence, while what he actually does with language is performance. Linguistic competence then, is a speaker-hearer's knowledge of his language, and performance is the speaker-hearer's use of this knowledge in actual situations. The investigation of competence must take place prior to the study of performance, since it is the most important factor underlying performance; the primary data, however, for this investigation can come only from performance. Chomsky says that a grammar in the traditional view is an account of competence; he writes in this connection that "a generative grammar (that is, an explicit grammar that makes no appeal to the reader's 'faculté de langage' but rather attempts to incorporate the mechanisms of this faculty) is a system of rules that relate signals to semantic interpretations of these signals. It is descriptively adequate to the extent that this pairing corresponds to . . . competence . . ."²⁰

The first section of Aspects is entitled, "Generative Grammars as Theories of Linguistic Competence," and therein Chomsky does not refer to a grammar as a model of competence. In "Explanatory Models in Linguistics," he does refer to a grammar as "an explanatory model, or a theory of the linguistic intuition of the native speaker."²¹ I would have to say that Chomsky is careful, then, when using the terms "model," and "theory," and does not use one for the other. I was under the

impression that he regularly uses the term "model" in referring to a grammar, i.e., a grammar as a model of competence, but I find rather that he usually refers to a grammar as being an 'account,' 'description,' or a 'theory' of competence. For purposes of discussion however, I will assume that he would not object to using the term "model" in place of the terms that he regularly uses. Any problems or criticisms raised regarding this usage will therefore provide reason for not using the notion 'model' in this context. This discussion will be aimed at other authors who do use 'model' indiscriminately, and speak of a grammar as a model of competence.

How does this use of the notion 'model' compare with the senses of model distinguished in Section I? If competence is understood to be something that a person has, rather than something he does, this implies that competence is something "internal," something not accessible to direct observation; this is the way Chomsky describes the situation. Since this is the case, performance provides the only access to competence, and though it directly reflects competence only in an idealized situation, we must investigate competence and construct a grammar which accounts for it solely on the basis of the imperfect data of performance. Viewing competence in this way, as only indirectly observable and therefore only "indirectly" understandable, it seems that the conditions for calling a grammar a model of competence could perhaps be met. For we do know some things about competence and, because of this, some more familiar system (a grammatical system in this case) is

suggested which we can exploit in continuing the investigation of competence. This more accessible and familiar secondary system is a model of competence providing that statements about it are translatable into statements about competence.

That which guarantees this translatability or conservation of truth value is analogy of structure or isomorphism between the model(grammar) and the explanandum (competence). The conditions of isomorphism are met if the structure or relationships exhibited in the explanandum exist correspondingly in the model. Do a grammar and competence share such an identity and analogy of structure? This is the crucial question that must be answered in considering whether or not a grammar can be called a model of competence.

To claim that isomorphism of structure exists between a grammar and linguistic competence, enough must be known about the structure and relations in competence to be able to say that the grammar's structure and relations are the "same as" those in competence. Of course, I am speaking here of the model's positive analogy with respect to the explanandum; in any case, to speak of analogy between two systems implies that the analogous characteristics of each are known. Can it be said that these characteristics which obtain in linguistic competence are known? I think not, at least not in a formal degree sufficient to claim that a grammar's structure is isomorphic with the structure of competence. Though linguists know much about ambiguity, syntactic structure, synonymy, etc., and claim that these things are part of a person's competence,

such knowledge is not, I think, the kind of knowledge of structure that is required to talk of isomorphism. This "intuitive" linguistic knowledge is formalized in a grammar; in fact, a grammar is constructed just to achieve this. In order for a grammar to be a theoretical model for competence, it must exist apart from knowledge of competence, and must be exploited in the investigation of competence, etc. It seems that a grammar is constructed because of the linguist's investigation of competence, and since this is so, along with the fact that isomorphism cannot be met, a grammar cannot be properly referred to as a model of competence. To do so is to misuse the concept of a model, and invites confusion. I suggest that Chomsky is more careful than others in his use of the term 'model' in this connection because of this problem.

III. Katz' Use of Model

This section will be concerned with Jerrold J. Katz' article, "Mentalism in Linguistics."²² In this article, Katz uses the term "model" in several ways, but there is a problem in his discussion of a grammar as a model of a brain mechanism. I will briefly outline the intention of Katz' article in order to give a background to his use of 'model' regarding the brain. Then I will describe this use of 'model,' and argue that a problem is involved in using the term "model" in this fashion.

The main purpose of this article is to argue that a mentalistic theory of language is better than a taxonomic linguistic theory because the former's conceptual machinery contains references to mental states and that this enables it to handle a greater range and kind of linguistic phenomena than the latter. A taxonomic theory suffers from limitations imposed on it by behavioristic and empiricist assumptions and viewpoints, and thus fails where theories that contain mental concepts succeed. To argue the superiority of a mentalistic theory, Katz must first characterize the mentalist's conception of linguistic theories and make clear "what a mentalist means when he says that reference to mental states is a necessary aspect of any adequate linguistic theory, and just what status he intends mentalistic concepts to have" (p.127).

First of all, how can these mental events referred to in the theory be part of a causal chain which also contains physical items--vocalizations, sound waves, etc.? He says that the mentalist linguist hypothesizes that there exists 'a highly complex mechanism' which underlies a speaker's ability to communicate, and that this mechanism is essentially the same from speaker to speaker. The mentalist "thus views the process of linguistic communication as one in which such mechanisms operate to encode and decode verbal messages. The aim of theory construction in linguistics is taken to be the formulation of a theory that reveals the structure of this mechanism and explains the facts of linguistic communication by showing them to be behavioral consequences of the operation of a mechanism

with just the structure that the formulated theory attributes to it" (p.128).

What kind of a move is the positing of such a mechanism, and what kind of status does this mechanism have? Katz answers this when he says that the "step of hypothesizing such a mechanism in the process of theory construction in linguistics is no different from hypothetical postulation in theory construction in any other branch of science where some component of the system about which we wish to gain understanding is inaccessible to observation" (p.128). A linguist cannot look into a fluent speaker's head to observe the workings there any more than a physicist, for example, can directly observe the innermost happenings in an atom. The physicist, as well as the linguist, Katz contends, can gain scientific understanding of the unobservable system only by constructing a model of the system which "contains a hypothesis about the structure of the components of the system which are not observable" (p.128). He writes further:

If the logical consequences of the model match the observable behavior of the system and would not do so without the hypothesis, the scientist may say that this hypothesis accounts for the behavior of the unobservable but causally efficient component. If the model is the simplest one which enables the scientist to derive all of the known facts and predict previously unknown ones as effects of the hypothesized component, he can assert that his model correctly pictures the structure of the system and its unobservable components. In this way, a linguist can assert that his theory correctly represents the structure of the mechanism underlying the speaker's ability to communicate . . . (p.128).

The earlier question about how mental events can be links in a causal chain containing physical events can now be answered. The mechanism to which these mental concepts refer is actually

a brain mechanism, a component of the neural system.²³ This mechanism is "inaccessible to observation in the sense that, even if the linguist could look inside a speaker's head, he would be unable to figure out the structure of the mechanism from the electrochemical events going on there" (p.128). In spite of this limitation, the linguist can proceed as Katz argues in the passages above, and assume that his linguistic constructions have an underlying "psychological reality."

The steps a linguist follows in constructing a theory of linguistic communication are clear. He must first 'hypothetically infer' the characteristics of the mechanism which underlies linguistic communication. This inference begins by positing such a mechanism which causes the observable events in linguistic communication. He next "invents a theory about the structure of this mechanism and the causal chain connecting the mechanism to observable events, to explain how these internal causes produce linguistic communication . . ." (p.129). Even though a linguist claims that this theory describes a neurological mechanism, he cannot "immediately translate the theory into neurological terms, i.e. into talk about synapses, nerve fibers, and such" (p.129). This simply means, however, that the linguist cannot yet specify "what kind of physical realization of his theoretical description is inside the speaker's head" (p.128). He is not committed then to psychophysical dualism in this regard, since he believes that there does exist such a brain mechanism as he is describing theoretically, and that this has just not yet been described in purely physical

terms. It is the task of neurophysiologists to undertake this sort of physical investigation of such a mechanism, not that of linguists. Linguistics and neurophysiology are independent fields of investigation, and it does not matter what sort of physical realization of the linguist's theory actually does exist. Katz writes further on this that

for the purpose of linguistic investigation, it is immaterial whether the mechanism inside the speaker's head is in reality a network of electronic relays, a mechanical system of cardboard flip-flops and rubber bands, or, for that matter, a group of homunculi industriously at work in a tiny office. All of these possibilities, and others, are on a par for the linguist as physical realizations of this mechanism, so long as each is isomorphic to the representation of linguistic structure given by the theory of the language. The critical distinction is, then, between an abstract, formal characterization of linguistic structure--the theory itself--and a physical system of some kind which instantiates this structure. Discovering what kind of a physical system in the human brain instantiates the representation of structure given by a linguistic theory is the task of the neurophysiologist. The linguist's task is to provide a theory which represents the structure that any physical system must possess if it is to be capable of linguistic communication . . . (p.129)

After Katz shows how mentalistic concepts can be understood as links in a causal chain which includes physical events, he discusses the three fundamental questions which a synchronic linguistic description must deal with. They are: (1) What knowledge underlies linguistic ability? (2) How is this knowledge put to use in actual communication situations? (3) How do speakers come to acquire this ability? In other words, these three questions deal with, respectively, competence, performance, and language acquisition. The first question is logically prior to the others, and must be investigated before the other two can be answered, as they depend

on competence. As Katz says, "We must know what linguistic facts a speaker knows before we can say how those facts enable him to communicate and before we can say how he acquired them . . ." (p.131). The answer to this first question will be called a linguistic description and will take the form of a generative grammar.

Katz next explicates that most significant aspect of competence, creativity, which is the ability a speaker has to make an infinite use of finite means, as described in Section II. To account for the creative aspect of competence, the grammar must take the form of a system of explicit generative rules. Taxonomic grammars have rules, too, of course, but not rules which are formulated on this basis; these rules are said to have no 'psychological reality' (i.e., they do not refer to mental states and ultimately to brain mechanisms in a speaker), as do the rules in a generative, mentalistic grammar, and therefore cannot account for linguistic competence in any fashion whatsoever.

At this point an overall model is needed, says Katz, which will demonstrate how the mechanism of linguistic communication operates in actual communication situations. Commenting on such a model, he says that this model "represents the most rudimentary form of the theory of linguistic communication which mentalists seek to construct, and is thus a first approximation toward an exact formulation of that theory in its fully sophisticated form. It should be stressed, however, that even as a first approximation such a model shares with a fully

elaborated and precise theory the character and status of a hypothetically inferred theoretical construction" (p.132).

Put very briefly, this model of linguistic communication has a speaker encoding his thoughts into acoustic messages which a hearer decodes in order to have the same thoughts, or "understand" the speaker. All of the actions involved are described in terms of physical mechanisms in the speaker's and hearer's brains and bodies, with the syntactic, phonological, and semantic components of a generative grammar being used to model what must go on in the brain of a speaker-hearer when all this takes place.

Within the framework of the above model of linguistic communication, every aspect of the mentalistic theory involves psychological reality. The linguistic description and the procedures of sentence production and recognition must correspond to independent mechanisms in the brain. Componential distinctions between the syntactic, phonological, and semantic components must rest on relevant differences between three neural sub-mechanisms of the mechanism which stores the linguistic description. The rules of each component must have their psychological reality in the input-output operations of the computing machinery of this mechanism (p.133).

Neurophysiological mechanisms perform the 'encoding' and 'articulation' of an utterance on the side of the speaker, and the 'perception' and 'decoding' of the utterance on the side of the hearer. The mentalist's description of this process must be understood to refer to real links in a causal chain and cannot be considered as containing heuristic fictions, as it were. The hypothesized mechanism which underlies linguistic communication has the kind of structure attributed to it by the linguistic description (grammar) and, "can explain how linguistic communication takes place only if the mechanism

and all its features have the same ontological status as the utterance itself" (p.136). This commits the mentalist linguist not to any particular physical realization for the linguistic description, or grammar, "except that whatever is inside the speaker's head must be capable of causal connection with the physical sounds that serve as the vehicle of linguistic communication" (p.136).

As I understand it, the linguistic description, or grammar, must be regarded as a model of the neural mechanism which underlies linguistic communication. In other words, there is some mechanism in the speaker's brain which enables him to communicate with language, and we construct a grammar whose structure pictures the unobserved structure of this mechanism. We might want to ask here how it can be said that a grammar pictures this mechanism, since we cannot observe the mechanism to check and see. Katz has stated above in this connection that "If the model is the simplest one which enables the scientist to derive all of the known facts and predict previously unknown ones as effects of the hypothesized component, he can assert that his model correctly pictures the structure of the system and its unobservable components" (p.128). I believe that this overlooks one of the features that distinguishes the proper use of a theoretical model, and the Katz cannot correctly say that a grammar represents or pictures the structure of a brain mechanism. I do not believe that isomorphism of structure between the grammar as a model and a brain mechanism can be demonstrated, and that thus Katz' use of 'model' is improper.

First of all, I have to assume that Katz is using 'picture' here in the sense of 'reproduces,' or 'mirrors,' or 'repeats the structure of,' etc. To object as I have above, that we cannot speak of a secondary object or system picturing an original object or system unless we are in a position to check or verify the truth of this by 'looking' at the original, misses the point in a certain way. Of course the original system in this case is unobservable, since it is a brain mechanism. We are not able to look inside people's heads, and even if we were to do so, we would not understand the structure of the electrochemical events taking place in the brain. We use a model to investigate this system just because of this inaccessibility; we have to speak of a model picturing the system under investigation if we are to investigate it at all. To object to this would be to object to a common scientific activity in regard to unobservable systems, and therefore cannot be seriously made.

This all may be true, but I think we have to admit that the concept of 'picturing' is essential to the use of models. I assume, then, that 'picturing' corresponds to the technical term "isomorphism," which was discussed in Section I as the most important feature of theoretical models. The positive analogy or identity of structure which exists between a model and the system it models is the only feature which makes translatability of statements between model and system possible. Without this conservation of truth value via rules of correspondence, a model is worthless for purposes of investigation.

With this in mind, the question now becomes: "Is there isomorphism between a grammar and a brain mechanism it is a model of?" If it can be shown that the answer to this is no, then it follows that it cannot be correct to speak of a grammar as a model of a brain mechanism, given the dependence which a theoretical model has on the concept of isomorphism.

It was shown in Section I that a model (as a more familiar system) suggests itself for use because there exists some rather straightforward analogy of structure between it and the system under investigation. This, of course, implies that at least some things are known about the system under investigation. At least enough must be known about its structure to be able to see the analogy to it which exists in a more familiar system. In the case of the brain mechanism which underlies linguistic communication however, practically nothing at all is known about its structure--it is unobservable, and if observed, inscrutable. How, then, can a linguistic description (grammar) suggest itself as a model of this mechanism, since not enough is known about the structure of the mechanism to say that any analogy to it exists in the grammar? I suggest that a grammar doesn't enter into the investigation of this brain mechanism as it would were it a true theoretical model. Without the positive analogy which exists in a familiar system exploited as a model of the system under investigation, isomorphism between them cannot be guaranteed. I believe that there can be no positive analogy between a grammar and a brain mechanism, and thus no isomorphism.

One might respond to these arguments that 'theory' could be the correct term to use in this case, since 'model' and 'theory' are often used interchangeably. And, once this move is made, it appears that the objections I have raised to Katz' use of 'model' can be met. To say that a grammar is a theory of a brain mechanism, rather than a model, seems more appropriate, since empirical knowledge of the brain is unavailable at this point. The 'theory' will be confirmed or disconfirmed at such time as this knowledge is available. Until then, a grammar is "just" a theory of a brain mechanism. With this move then, my problem becomes one of making sense of the new claim.

There is a crucial problem involved with viewing a grammar as a 'theory' of a brain mechanism, which has to do with the interpretation of theoretical terms. This process of interpretation is referred to by some writers as "semantic ascent."²⁴ Briefly outlined, semantic ascent is the way in which the uppermost terms in a theory are interpretable by means of the lowest level terms forming the empirical base of the theory. There are terms at the highest levels of a scientific theory (for example, neutrinos in quantum physics) which have no direct interpretation by means of the terms in the theory's observational base; but these upper level terms do have a firm place so long as they occur only at the "top" of a theory, and can be interpreted in some way up from the theory's bottom terms.

However, a linguistic theory offered as a theory of a brain mechanism cannot meet the conditions of semantic ascent regarding its theoretical terms. The observational base of

the theory consists of direct observation of what people say, statements of linguistic "intuition," etc. Given the state of present neurological investigations, no item of structure in the brain can be identified, and therefore the theory cannot contain lower level empirical terms about the brain. If this is so, then the upper level terms in a linguistic theory can have no relation to any terms regarding the brain, and thus can never have an interpretation based on any brain terms. Such a lack of empirical terms about the brain makes semantic ascent impossible, and it won't do to simply claim that there is some sort of structure in the brain which the linguistic theory is a theory of. If the theory is to be considered a theory of the brain, or a brain mechanism, then some items of structure must be identifiable in the brain, but at the present time, no item of structure in the brain can be said to correspond to any item, at any level, in a linguistic theory of a brain mechanism.

It seems difficult to conceive how a grammar as a theory of a mechanism in the brain can ever be said to explain such a mechanism in the standard sense of how theories explain what they are theories of. A linguistic theory of the brain can neither describe nor simulate point by point the activity in such a brain mechanism, since no such points can be distinguished. If this is so, then it is incorrect to use the term "theory" here, and if a grammar must be considered a theory, then perhaps it should be regarded as a theory of something other than a brain mechanism. I see no objections to claiming that a

grammar is a theory of linguistic competence, so long as competence is not understood as brain activity.

Given the arguments above, I do not believe that Katz can claim that the actual physical realization of this brain mechanism is irrelevant or immaterial for purposes of a linguistic investigation. He says on the one hand that this mechanism is unobservable and on the other hand that any physical realization of this mechanism will do, just as long as each is isomorphic to the representation of linguistic structure given by the theory of the language. I have argued that to speak of 'isomorphism' in this context is impossible, given our present state of knowledge about the structure of the brain, and I regard this argument as essentially correct.

It is not a proper use of the terms "model" or "theory" to say that a grammar is a 'model' or a 'theory' of a brain mechanism, since an element necessary for their proper application is lacking in each case. If linguists insist on using "model" or "theory" in connection with a grammar, then I feel that they should do so with extreme caution, and only with the standard senses of these scientific terms in mind.

IV. Summary and Conclusion

This section is comprised of a short summary of the discussion in the previous sections of the paper, plus some general conclusions. While the summary is intended simply to

reiterate the main points raised in the body of the paper, the conclusion will be concerned with the role which the notion 'model' has in generative grammar and linguistics in general.

Both ordinary and scientific senses of a model were discussed in Section I. The important features of an ordinary conception of a model were found to consist of (1) representation or material analogy between the model and object modeled, and (2) the model making relevant features of the object modeled more accessible. An intermediate sense of model, an analogue model, can be distinguished in the area between ordinary models and scientific, theoretical models. An analogue model reproduces the structure of the object modeled, rather than literally resembling it; this analogy is called formal analogy, and the dominating principle of this type of analogy is isomorphism. This identity of structure between a model and original enables statements formulated in the model to apply as well to the original system.

Mathematical models were found to be prevalent in science, but are not regarded as scientific models in the 'classic' sense, for they do not aid empirical investigation or provide causal explanations; rather, they provide only the form of an explanation. The type of models which are most intimately associated with scientific theories are theoretical models. Initially, this type of model is advanced because there exists some straightforward positive analogy between it and the system under investigation. Any negative analogy in the model is

ignored, resulting in a conceptual entity, a theoretical model. This abstract construction is usually richer in concepts and relations than is the explanandum, and exploitation of it consists in investigation of this neutral analogy or open texturedness, leading to further developments in the x theory. Again, isomorphism of structure between model and system modeled is essential for the translatability of statements from one domain to the other. Theoretical models are described, not actually constructed, as are replica and analogue models, and it can be said that the key to this sort of model consists of talking a certain way.

The discussion in Section II was limited to two uses of 'model' by Chomsky, the first, his investigation of three models for the description of language, and the second, the idea of a grammar being a model of competence. Chomsky was led to the investigation of these three mathematical systems (finite state, phrase structure, and transformational) because of his view that a grammar must be a generative device. His conception of linguistics has a great affinity with the views of traditional, rationalist grammarians, and he has accepted several of their distinctions. Among those is the distinction between a speaker's competence and performance, between his linguistic knowledge and his use of this knowledge. Traditionally, a grammar has been viewed as an account of competence, but recent grammars are inadequate because they fail to take into account the creative aspect of competence, i.e., the making of an infinite use of finite means. This creativity of

language can only be reflected in a grammar whose structure incorporates recursive or generative devices, and the study of such mechanisms has come about only recently in mathematics.

Chomsky found the finite state model to be inadequate for the description of language because English is not a finite state language. A phrase structure model was found to be adequate only for sentences with a simple structure, and a transformational model which incorporates both phrase structure and transformational rules was found to be adequate for all sentences of English. Several questions can be asked concerning these three models. (i) In what sense are these models models, as outlined in Section I? (ii) How can these mathematical systems be said to serve as models for the construction of a grammar? (iii) Can these models be properly referred to as models of linguistic structure?

Even with the addition of Zellig Harris' comments on mathematical linguistics and I.I. Revzin's analysis of models of language to the foundation of the discussion, the answers to these question were still somewhat ambiguous as to the real nature of this use of 'model.' I concluded, however, that a strong argument can be made for claiming that Chomsky's use of the term 'model' in this connection is generally consistent with the notion of a theoretical model as distinguished in Section I. The conditions for the use of a theoretical model and its salient features seem to be found in Chomsky's use of 'model.'

Concerning the second use of 'model' by Chomsky, I found that he does not refer to a grammar as a model of competence, but

rather as a 'theory,' or an 'account' of competence. He is careful then, in not using 'model' loosely in this context, but others are not, and the discussion of the problem involved with this use of 'model' is directed at them. If one says that a grammar is a model of competence, then it must be shown that isomorphism between the model (grammar) and competence exists. I argued that not enough is known initially about competence to justifiably speak of analogy or identity of structure between it and a grammar; and, thus it is not proper to speak of a grammar being a model in this case, since an essential aspect of the use of 'model' in the scientific sense is absent.

Jerrold J. Katz has spoken of a grammar as a model of a brain mechanism in "Mentalism in Linguistics." He argues that a mentalistic linguistic theory is better than a behavioristic, empiricist oriented taxonomic theory because it can handle a greater range and kind of linguistic fact than can the latter. This is so because the mentalist theory's conceptual machinery contains references to mental events, or rather, to a certain brain mechanism. This mechanism is hypothesized as underlying the process of linguistic communication in which a physical encoding and decoding of thoughts and acoustic messages takes place. The brain mechanism accomplishes the "internal" part of this process. The linguist constructs a theory about the structure of this mechanism and a linguistic description, or a grammar, is understood to be a model of it; the components of a grammar correspond to "neural submechanisms."

The problem with this use of 'model' is that one cannot claim that this model represents the structure of a brain mechanism unless much more is known than presently about the actual structure of such a mechanism in the brain. If isomorphism cannot be shown to exist between a model and this mechanism, then this use of model is a misuse of the term. One can also argue that to speak of a theory of the brain in linguistics is a mistake because an element essential for its correct use is lacking, given the present state of knowledge of the brain. This element has to do with the 'semantic ascent' of theoretical terms; the uppermost terms in a scientific theory must be interpretable in terms of the theory's empirical, observational base. In the case of a linguistic theory of a brain mechanism, however, this condition is not met. No item of structure in the brain can be said to correspond to any item, at any level, in the theory. These brain items are as yet merely unobserved, rather than unobservable in principle, as are, for example, 'neutrinos' in the theory of quantum physics. Even so, a linguistic theory of the brain can neither describe nor simulate point by point the activity in the brain, since no such points of activity can be distinguished in the brain.

Given these arguments, it is not a proper use of 'model' to say that a grammar is a 'model' or a 'theory' of a brain mechanism, and if the terms must be used here, then they should be used with caution, and with a clear sense of these terms as used in science in mind.

From all of the discussion above, I believe that some conclusions can be drawn concerning the use and the role which 'model' has in generative grammar and in linguistics in general. First of all, I think that it has been clearly established what Chomsky and Katz mean by their references to models in their work. The arguments that I have put forth concerning their uses of 'model' have been aimed only at whether or not these uses can be considered proper, given what the standard senses of model are in science.

In the case of Chomsky I am persuaded that his early work concerning three mathematical models for the description of language was not a misuse of the notion of 'model,' though at times the crucial points are obscure. He does not refer to a grammar as a model of competence, but the points I raised in this regard do apply to those linguists who are not as careful as he in their use of the term "model."

Katz refers to a grammar as a model of a brain mechanism, and I think that it has been shown that this use of 'model' is a misuse insofar as it fails to meet the test of isomorphism. If one claims rather that a grammar is a theory of a brain mechanism, then the problem of 'semantic ascent' enters in. Since our knowledge of the brain is so scant, the upper level terms in a linguistic theory of the brain have no real connection with brain terms, since brain terms in the theory's observational base are completely lacking. Since this is so, to use the term "theory" in this context is improper. I remain unconvinced that 'model' can ever be used as Katz does here.

Regarding the total issue of the use of 'model' in generative grammar, I feel that I have demonstrated beyond any doubt that one must be extremely careful in using the term "model," and should use it only with knowledge of the correct scientific uses of 'model,' so that the issues I have raised may be recognized and avoided.

Speaking briefly to the role of 'model' in linguistics generally, I would say that the use of models is unavoidable, given the assumptions of modern linguistics. If it is true that the heart of a theoretical model consists of talking a certain way, then Chomsky's contribution to linguistics has been in abetting the change from talking one way to talking another way. One would not call American linguistics before the advent of generative grammar unscientific, I think, but perhaps narrowminded. It was only after the point of view that produced generative grammar superseded structuralism that linguistics came to be more properly scientific in its methodology. Scientific activity consists in part of constructing models, and linguistics is no different in this than any other discipline. Naturally enough, there exist questions concerning the use of models in linguistics, as is the case in every other instance where models are part of the methodology. In this paper, I have raised some of the problems concerning their use which should be considered.

FOOTNOTES

1. Cf., Y.R. Chao, "Models in Linguistics and Models in General," in E. Nagel, P. Suppes, and A. Tarski (eds.), Proceedings of the 1960 International Congress on Logic, Methodology and Philosophy of Science, (Stanford, California: Stanford University Press, 1962), p.55.

2. Max Black, "Models and Archetypes," in Models and Metaphors, Studies in Language and Philosophy, (Ithaca, New York: Cornell University Press, 1962). Further references to this work are indicated by page number.

3. Mary B. Hesse, "Models and Analogies in Science," in Encyclopedia of Philosophy, Vol. 5, (New York: Macmillan, 1967). Further references to this work are indicated by page number.

4. Noam Chomsky, Topics in the Theory of Generative Grammar, (The Hague: Mouton & Co., 1966), pp.9-10.

5. Noam Chomsky, Aspects of the Theory of Syntax, (Cambridge, Mass.: M.I.T. Press, 1965), p.4.

6. Chomsky, Topics, p. 10.

7. Ibid.

8. Chomsky, Aspects, p. 8.

9. Chomsky, Topics, p. 12.

10. Ibid.

11. Noam Chomsky, Preface to M. Gross and A. Lentin, Introduction to Formal Grammars, trans. M. Salkoff, (Heidelberg: Springer-Verlag, 1970), p.8.

12. Noam Chomsky, "Three Models for the Description of Language," in R.D. Luce, R. Bush, and E. Galanter (eds.), Readings in Mathematical Psychology, Vol. II, (New York: Wiley, 1965), p. 105.

13. Noam Chomsky, Syntactic Structures, (The Hague: Mouton & Co., 1957).

14. Chomsky, "Three Models . . ." p. 105.

15. Chomsky, Syntactic Structures, p. 13.

16. Chomsky, "Three Models . . ." p. 106.

17. Chomsky, Syntactic Structures, p. 24.

18. Zellig Harris, Mathematical Models of Language, (Philadelphia, Pa.: University of Pennsylvania Press, 1968). Further references to this work are indicated by page number.

19. I.I. Revzin, Models of Language, trans. N.F.C. Owen and A.S.C. Ross, (London: Methuen & Co., Ltd., 1966). Further references to this work are indicated by page number.

20. Chomsky, Topics, p. 12.

21. Noam Chomsky, "Explanatory Models in Linguistics," in E. Nagel, P. Suppes, and A. Tarski (eds.), Proceedings of the 1960 International Congress on Logic, Methodology, and Philosophy of Science, (Stanford, California: Stanford University Press, 1962).

22. Jerrold J. Katz, "Mentalism in Linguistics," in Language, Vol. 40, No. 2, 1964, p. 125. Further references to this work are indicated by page number.

23. This move of Katz', along with his rejection of psycho-physical dualism, indicates that he is committed to some version of the identity thesis. It is not clear however, exactly what position he would take on this.

24. See Yorick Wilks, "One Small Head--Models and Theories in Linguistics," in Foundations of Language, Nov. 1974, pp.77-95.

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ON THE NOTION MODEL IN GENERATIVE GRAMMAR

by

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B. A., Kansas State University, 1973

AN ABSTRACT OF A MASTER'S THESIS

submitted in partial fulfillment of the

requirements for the degree

MASTER OF ARTS

Department of Modern Languages

KANSAS STATE UNIVERSITY

Manhattan, Kansas

1974

ABSTRACT

The paper is concerned with establishing what N. Chomsky and J.J. Katz mean by their references to 'model' in the context of generative grammar. Ordinary and scientific conceptions of a model are established to serve as a point of reference in the discussion. Chomsky's investigation of three mathematical models for the description of language is considered, along with the notion that a grammar is a model of linguistic competence. The question here is: Do these uses of 'model' correspond to the scientific senses of a model distinguished earlier? Katz' conception of a grammar as a model of a brain mechanism is critically discussed, and problems are raised which deal with the concept of isomorphism. Is this a correct use of the term "model," given our present knowledge of the brain? A summary and a conclusion regarding the role of 'model' in generative grammar and linguistics in general close the discussion.