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PREFACE

The marriage of linguistics and the psychology of language, while more than a century old, is one of doubtful stability. From time to time the partners get involved in a serious struggle for power, with the outcome that either the psychology of language becomes dependent on linguistics (Steinthal), or linguistics becomes dependent on the psychology of language (Wundt). There are also long periods of coldness in which the two parties tend to ignore each other, as was the case in the first quarter of this century.

Fortunately, however, from time to time one can witness a refreshing and intense cooperation between linguists and psychologists. Such was the case in the 1960's when the new transformational linguistics gained great influence on the psychology of language, in particular through the work of George Miller and his collaborators. During that period various formal models were enthusiastically studied and examined for their "psychological reality". The studies were based on Chomsky's distinction between competence and performance, with which linguists had joyfully thrown themselves into the arms of psychologists ("linguistics is a chapter of psychology").

In the long run psycholinguists, however, could not live up to it, and there followed a period of reflection — but certainly not of cooling-off — during which the relations between the linguistic and psychological models were examined with more objectivity and a greater sense of reality.

This volume will be devoted to a discussion of the connection between formal grammars and psycholinguistic models. The connection has been worked out in various ways over the three main fields of psycholinguistics: (1) the study of the psychological basis of linguistic intuition; (2) the study of the "primary" linguistic behavior of the speaker-hearer; and (3) the study of language acquisition.

Our aim in this volume will be expressly limited. We do not attempt to present an introduction to the modern psychology of language, but rather only to show how the theory of formal languages and its applications to linguistics have penetrated psycholinguistics. That influence can be seen above all in the syntactic applications, but we shall show that formal language theory is of increasing importance to the semantic and conceptual aspects of the psychology of language, and that those aspects will draw growing attention.

Although this volume is addressed primarily to psychologists, it treats a number of topics which are also of interest to linguists, such as the problem of competence and performance, the structure of linguistic intuitions, and the language of the small child.

The presentation supposes that the reader is familiar with the material given in Volumes I and II, to which reference is often made.

W. J. M. Levelt

August, 1973 *Nijmegen*

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GRAMMARS IN THE PSYCHOLOGY OF LANGUAGE: THREE PROBLEMS

1.1. LANGUAGE USE, LINGUISTIC INTUITIONS, AND THE ACQUISITION OF LANGUAGE

The empirical psychology of language came into being in the second half of the nineteenth century, and developed rapidly to a tentative culmination in the classical work of Wilhelm Wundt (1900). The discipline has always been occupied with the psychological investigation of the acquisition and use of language. Language acquisition involves, in the first place, the process by which the growing child learns to use his native language, but it also includes the learning of a foreign language in later development, and the learning of artificial languages, such as the sign language used by the deaf, and more abstract systems of symbols. It corresponds to the developmental aspect of the psychology of language. The use of language corresponds in turn to the functional aspect. It has to do with such matters as the way man uses his language in communication situations, the way he formulates what he means while speaking, and the way be deciphers what another means while listening. It also takes in the derived processes of reading and writing, the way in which man memorizes verbal material over a longer or shorter period of time, and the way he later reconstructs it, and finally, the relations between speech and other psychological functions such as perception, thinking, decision making, and so forth.

We have stated, for the sake of caution, that the aim of the psychology of language (psycholinguistics) is the *psychological* investigation of language acquisition and speech. But in this respect, it is difficult to establish the frontiers between linguistic and psychological research. In Volume II, Chapter 1, we stated that the formulation of psychological theory is essentially part of linguistic interpretation. The present volume is almost exclusively concerned with these psychological aspects of linguistic interpretation, and it will be quite evident that it is often impossible to draw a sharp line of demarcation between the two disciplines. In Volume II, Chapter 1, we showed that certain phenomena are considered to fall into the domain of linguistics merely because of more or less arbitrary traditions. Some intuitions of the native speaker are considered to be linguistic, while others are not. The inacceptability of very long sentences, for example, is seen as an intuition irrelevant to linguistics. The history of the psychology of language shows clearly that such traditions are indeed arbitrary. The line drawn between psychology and linguistics, or more precisely, the relationship established between the two sciences, will largely be dependent on some a priori philosophy concerning the relationship between language and the human mind. In recent history opinions have diverged considerably on this point. In the remainder of this section we shall mention, without attempting an exhaustive discussion, two prominent points of view on the subject while making some critical remarks on a number of current trends.

Some authors, such as Steinthal in the nineteenth century and Whorf in the twentieth, claim that the structure of the most important human cognitive functions, such as perception and thought, is determined by language, and that psychology should therefore be based on linguistics. Others reverse the argument, and state that the structure of human language can be understood only on the basis of the structure of the human mind. This latter point of view was already to be found in the nineteenth century among the *Junggrammatiker* (Paul and others), and explicitly in the work of Wundt and many of his disciples. They tried to find psychological explanations for linguistic phenomena, above all for diachronic regularities which were the *pièce de résistance* of nineteenth century linguistic research. But even since de Saussure revived interest in synchronic relations, many linguists have maintained that their science should, in the final analysis, be drawn back to psychology. Sometimes only lip service was paid to this point of view, without any practical consequence (de Saussure, Bloomfield), but in Chomsky's work the conception was given a new and detailed formulation. For Chomsky, "linguistics is a chapter of human psychology" (Chomsky 1968), and he attempts carefully to delimit that chapter. He calls the psychological object of linguistics LINGUISTICS COMPETENCE, the creative faculty which allows the language user to understand and form an unlimited number of sentences. According to Chomsky, it is a whole of more or less unconscious knowledge which is applied in every act of linguistic behavior, such as speaking and listening. Chomsky calls this actual usage of language PERFORMANCE, and relates it to competence in a way which may be seen in the following quotation:

> To study actual linguistic performance, we must consider the interaction of a variety of factors, of which the underlying competence of the speaker-hearer is only one (Chomsky 1965).

Other psychological variables are such matters as attention, memory span, etc. But this is also an a priori delimitation of the domain of linguistic research, and Chomsky is aware of its tentativeness:

> It must, incidentally, be borne in mind that the specific competence-performance delimitation provided by a grammar represents a hypothesis that might prove to be in error... When a theory of performance ultimately emerges, we may find that some of the facts we are attempting to explain do not really belong to grammar but instead fall under the theory of performance, and that certain facts that we neglect, believing them to be features of performance, should really have been incorporated in the system of grammatical rules (Chomsky and Halle 1968).

It is interesting to notice in this quotation that although Chomsky is aware of the arbitrariness of the delimitation of linguistics, he nevertheless considers it an empirical question. When a complete theory of human linguistic behavior is prepared, it will be evident which part of the theory is concerned with competence and is therefore the linguistic part. It is clearly presupposed here that empirical evidence will not have to do with the distinction between competence and performance itself, but only with the precise delimitation of competence. This position obscures the empirical character of the very starting point, which can be summarized in the following two points:

(1) The language user disposes of a system of rules called linguistic competence, which is the basis of actual linguistic behavior. Actual linguistic behavior (performance) is the result of the interaction of competence and other psychological factors.

The empirical character of this first point is comparable to that of the psychological notion of intelligence. In psychology, intelligence was originally no more than a theoretical construct with little empirical basis, but a promising program of research was defined with it. For the first time careful distinctions were made between such matters as "dumb" (competence) and "lazy" (performance), and external factors which stimulate or hinder intelligent behavior were discerned. At the same time it gradually became more plausible that a common factor, called "general intelligence", lay at the base of all forms of what could be called "intelligent behavior". Empirical research showed that when that common factor is subtracted, a whole gamut of specific capacities, which play a role in some forms of intelligent behavior and not in others, comes to light. Linguistic competence is an empirical notion in quite the same way. It is originally only a theoretical construct by which a program of research is defined. Only growing empirical evidence can prove that linguistic competence, like intelligence, can be clearly distinguished from other psychological factors, and we might mention in passing that three quarters of a century of research on intelligence has not been sufficient to be really decisive in this regard (cf., for example, Layzer 1973). Also, it is an empirical issue whether linguistic competence is indeed a general factor in human linguistic behavior, or to what extent various specific competences play a role in particular forms of verbal behavior. These latter can nevertheless be specifically verbal, that is, relatively autonomous with respect to external factors such as attention and perception. The distinction between competence and performance is not a platonic truth, but an empirical psychological question.

(2) A grammar is a description of linguistic competence.

Some explanation will be needed to show that this is also an empirical issue. To do so we shall suppose that proposition (1) is indeed valid, i.e. that linguistic competence, a relatively autonomous factor at the basis of all linguistic behavior, does indeed exist. The question at this point is whether a grammar is a description of that linguistic competence. According to Chomsky, this should in fact be the case:

> We use the term "grammar" with a systematic ambiguity. On the one hand the term refers to the explicit theory constructed by the linguist and proposed as a description of the speaker's competence. On the other hand, we use the term to refer to this competence itself (Chomsky and Halle 1968).

The first part of this quotation states that a grammar should be a theory of linguistic competence, but according to the second part we may as well call the linguistic competence of the language user his grammar. Linguistic competence and linguistic theory would thus completely coincide. The empirical question is whether a (complete) theory of linguistic intuitions is identical with a (complete) theory of human linguistic competence. In Volume II, Chapter 1, we saw that the empirical domain of a Chomsky grammar consists of linguistic intuitions. Chomsky, then, has no doubt as to this identity:

> the grammar is justified to the extent that it correctly describes its object, namely the linguistic intuitions — the tacit competence — of the native speaker (Chomsky 1965).

This, however, is even less a self-evident truth than point (1), for as opposed to the first proposition, the second is unlikely to be confirmed empirically. The theory of one kind of linguistic behavior, namely, metalinguistic judgment on such things as grammaticality and paraphrase, would then as a whole be built into theories on other forms of linguistic behavior such as speaking and understanding. The theory of linguistic intuitions is competence theory, according to (2); this in turn, according to (1), is part of every performance theory. The priority, given in this way to the theory of linguistic intuitions, has no empirical basis whatsoever. On the contrary, if we wish to think in terms of primary and derived forms of verbal behavior, the speaking and the understanding of language fall precisely into the category of primary forms, while metalinguistic judgments will be considered highly derived, artificial forms of linguistic behavior, which, moreover, are acquired late in development. We mentioned in Volume II, Chapter 1, that nothing is known of the origin of linguistic intuitions; we do not know what role factors such as imagery and verbal and nonverbal context might play, nor do we know to what extent intuitions are learnable, or how they originate and develop in the child.[1] We simply do not know the psychological factors which determine the formation of such intuitions. It would be foolish to make linguistic virtue of psychological necessity by concluding that these factors are unimportant simply because they are unknown, but this is precisely what is done when linguistic intuitions are made the key to linguistic competence. Some introspection, moreover, will make it plausible that the imagination of primary linguistic behavior — speaking and listening in a given situation of communication — plays an important role in the formation of linguistic judgments. Judgments on paraphrase are often based, at least from a phenomenological point of view, on a search of an imagery situation in which both sentences may be said with the same intention; if such a situation can be imagined, the two sentences are considered paraphrases of each other. In a similar way, judgments on grammaticality are perhaps dependent on the possibility of imagining a situation in which a sentence may be said. It would be an illusion to suppose that in such a case external factors, such as memory, motivation, etc., suddenly no longer play a role in the formation of

[1] During the translation of this volume Gleitman, Gleitman, and Shipley (1973) published a highly interesting article on this subject.

the judgment. We unfortunately do not know precisely what that role is, but there is at least as much reason to take a theory of primary verbal behavior as the basis of a theory of linguistic intuition as the inverse. It seems safer, however, to avoid connecting such theories a priori in series.

These remarks on the relation between psychology and grammar allow us at this point further to define the problems which will be discussed in the present volume. As we have already mentioned, the psychology of language is traditionally concerned with the usage of language and with language acquisition. From the point of view of grammars, the empirical problem in the psychology of language is in turn divided in two, the investigation of psychological factors in PRIMARY LANGUAGE USAGE, and the psychological investigation of LINGUISTIC INTUITIONS. The investigation of LANGUAGE ACQUISITION adds still a third problem to this — genetic aspects of grammars. Volume III is subdivided according to these three subjects. As was the case in Volume II, the principal accent will be laid on the formal aspects of the problems concerned, and questions of substance will not be treated systematically.

It should be noted that this division into three empirical fields is above all pragmatic. In principle the various subjects overlap in important and interesting ways. The remainder of the present chapter will evoke a few examples of such relations.

1.2. PRIMARY USAGE OF LANGUAGE AND LINGUISTIC INTUITIONS

In the preceding paragraph linguistic judgments were opposed to primary usage of language in speech and understanding. In the present paragraph we would emphasize that there is an area of rather fluent transition between the two, a field of research to which neither psychologists nor linguists have given much attention.

We can call this area of overlap that of METALINGUISTIC USE OF LANGUAGE. It is obvious that intuitive linguistic judgments are metalinguistic judgments. The object of a judgment of grammaticality such as *the sentence "John lives in town" is good English* is a linguistic object (*the sentence S*), and a judgment is made on it as a linguistic object. But the judgment itself is also a sentence, and as such it is subject to paraphrase relations with other sentences. The above example is thus a paraphrase of *"John lives in town" is good English.* An interesting question is whether grammatical relations between metalinguistic judgments are determined by the same rules as relations between "ordinary" sentences. The question is difficult to answer in this general form, but a step in the direction of analysis might involve the following, more elementary question: do metalinguistic forms exist in ordinary speech, and if so, how can they be described?

It is in fact easy to find various forms of "ordinary" metalinguistic speech. A few examples are the following:

(1) *John strikes Mary and inversely*
(2) *John strikes Mary and the latter strikes the former*
(3) *John and Mary entered in that order*

In these sentences *inversely*, *latter*, *former*, and *in that order* refer to the order of constituents in the sentence; in that respect they are metalinguistic.

There are other metalinguistic sentences in which the vocabulary of the language is extended, as it were, to include a linguistic auxiliary vocabulary, that is, to include words like *sentence*, *word*, *manner*, *rhyme*.

(4) *the sentence "John lives in town" has four words*
(5) *the word "biscuits" comes from French*
(6) *they sang a song called "By the Old Mill Stream"*
(7) *he looked at her in a "what do I care" manner*
(8) *"house" rhymes with "mouse"*

The use of these and other metalinguistic constructions is bound by specific rules. Restrictions of the use of *latter*, *former*, and *in that order* may be seen in the following constructions which are rather difficult to accept.

(9) **after they had entered the house in that order, John struck Mary*

(10) **after the latter had struck the former, John and Mary entered*

Latter and *former* do not simply function as pronouns, for we can say the following instead of (10):

(11) *after he had struck her, John and Mary entered*

and we obviously cannot paraphrase (2) with:

(12) *Mary was struck by John and the latter struck the former*

This shows that sentence (2) must have a complicated syntactic description. To derive sentence (2) it is evidently first necessary to generate the first half in such a way that the surface word order has *John* and *Mary*, and the second half of the sentence must in some way be generated so as to make reference to this surface order. In the syntactic description of metalinguistic sentences, coordination will often necessitate the introduction of the surface form of one subsentence into the underlying structure of the other subsentence. None of the transformational grammars in Volume II offers the technical means for accomplishing this. Nevertheless we are dealing here with ordinary speech, and it is not audacious to assert that linguistic judgments are an extension of it. Moreover, such usage can be consciously learned. This is precisely what happens in secondary and higher education: the language student becomes familiar with a whole vocabulary whose function is to refer to the language; the vocabulary includes such words as *vowel, morpheme, coordination, constituent, transformation,* and so forth.

Metalinguistic speech is not limited to one level. Just as one can speak of the language, he can also speak of metalinguistic sentences, and there again certain restrictions will apply. It is not to be expected that a system of rules which is adequate for one level should also be adequate for the next higher level. In this way linguists create a language to which their grammars are not applicable.

Not only is there a linguistic vacuum around metalinguistic usage, but there is also little known of this reflective behavior from a psychological point of view. Experiments have never been carried out on the way the hearer assimilates such metalinguistic sentences as examples (1) to (8). Likewise, psycholinguistics has yet to begin the investigation of the closely related field of the psychological background of metaphore. (Gleitman and Gleitman [1970] have, however, developed an ingenious experiment on the origin of the paraphrase judgment.)

1.3. LINGUISTIC INTUITIONS AND LANGUAGE ACQUISITION

We know as little about the acquisition of linguistic intuitions among children as we do of their nature among adults (see, however, footnote on page 6). But it is generally accepted in the investigation of children's languages that the small child is hardly capable of making judgments on his own language. As we have seen in Volume II, Chapter 1, this leads to an essentially different linguistic analysis; the child's grammar is not tested on the basis of intuitions, but rather on a corpus of utterances, complemented by understanding games, imitation, and careful notation of the situational circumstances in which certain utterances occur. The relation between such a speaker/hearer grammar and an intuition grammar is unknown. We mentioned the analysis of children's languages as a characteristic example of a linguistic problem of interpretation, and stated that psychological theory is indispensable to its solution.

A modest step toward research on the growth of linguistic intuitions in the child would be the investigation of metalinguistic usage. When does the child produce and understand metasentences such as examples (1) to (8)? Here, too, little is known, but some aspects of the problem are striking. The child, for example, is quite rapid in acquiring the notion of what a word is; if a three year old is asked what a word is, he will respond with a series of nouns and proper names. At the same age he will understand what rhyme is. The child, therefore, apparently disposes of metalinguistic notions of word unity and sound associations quite early. But it is only later that he comes to the idea of sentence; two and three year olds notice ungrammaticality in word order only by exception (de Villiers and de Villiers 1972). It is quite normal for a child of six or seven not to know what a sentence is, or to reserve the notion for declarative sentences while excluding interrogative and imperative sentences. An early form of metalinguistic behavior was described by Weir (1962). She showed how the child practices syntactic forms by successively fitting various words into a given sentence frame: *what color — what color blanket — what color mop — what color glass* — etc.; this was done by the child immediately before falling asleep. The activity was spontaneously performed without training. The influence which metalinguistic behavior has on language acquisition itself is unknown, but it is probably considerable. In Chapter 4 we shall discuss a model of language acquisition in which metalinguistic feedback plays an essential role. In its explicit form, that feedback consists of an announcement on the part of the educator that a given utterance is or is not good English; there are, of course, other more subtle forms such as the giving of an inadequate reaction to an utterance which has not been understood.

There is also an interesting inverse relation between language acquisition and linguistic intuition. McNeill (1966) shows that adults have rather accurate intuitions on the level of development at which different sentences are formed. In an experiment, he asked adults to rank several sentences according to degree of grammaticality. The sentences were spontaneous utterances of an English-speaking child at the ages of twenty-six, twenty-eight and a half, and thirty-one months. They were selected for the experiment in such a way that age and sentence length were not correlated. The result was that the ranking of grammaticality as judged by the adults corresponded well with the stages of development at which the sentences were produced. Nearly the same was true of the judgment of adults who had acquired English only as a second language. McNeill relates this to his theory on the differentiation of word classes. For him, all children, regardless of linguistic environment, begin with the same fundamental word classes, and the first differentiations which are introduced into those word classes are also universal. The differentiation of classes is also reflected in the syntactic structure of sentences produced at various levels of development. The last stage of differentiation is the adult's system of categories. The system of syntactic categories used by the adult, however, is not only a refinement of the various systems used by the child: the actual sequence of the phases of development is reflected in the adult's hierarchical subcategorization. Thus, for example, not only does the primitive distinction between verb and noun remain, but verbs are further differentiated at a later stage of development into subcategories of transitive and intransitive verbs, nouns are further distinguished as concrete or abstract, or divided into other subcategories. On this point, McNeill relates his model to Chomsky's grammaticality model (Chomsky 1964), according to which sentences become ungrammatical through the violation of category and subcategory features. Ungrammaticality increases when a more fundamental category is violated, that is, a category which is localized higher in the hierarchy. The the string *the students elephant the car* (violation of the distinction between noun and verb) is more seriously ungrammatical than *the students laugh a car* (violation of the distinction between transitive and intransitive). Sentences produced at a developmental stage where a certain category or subcategory has not yet been acquired, therefore, will, on the average, be more seriously ungrammatical than sentences produced at a later stage, where the differentiation in question has already been introduced.

One can rightly pose objections to Chomsky's classification into strictly hierarchical subcategories. Cross-classification of subcategories also occurs, as Chomsky shows in *Aspects*. That cross-classification model is in fact adopted by McNeill (1971) in his theory of the development of word class differentiation. But the fact that judgments on grammaticality and the stages of language development show a strong relationship calls for further research. There is, moreover, a set of related psycholinguistic phenomena, such as the apparent ease with which adults speak children's languages and the ease with which small children imitate the languages of even smaller children. We know of no research already done on the development of this "feeling for language".

1.4. LANGUAGE ACQUISITION AND PRIMARY USAGE OF LANGUAGE

Nearly all research on language acquisition has been based on the child's usage of language. Research on the interaction of these two psycholinguistic aspects is therefore not easily distinguishable from research on language acquisition as such. We refer, therefore, to Chapter 4 of the present volume, in which this subject will be further discussed.

2

GRAMMARS AND LINGUISTIC INTUITIONS

2.1. THE UNRELIABILITY OF LINGUISTIC INTUITIONS

The empirical touchstone in the tradition of transformational linguistics is the linguistic intuition, either of the linguist himself or of an informant. This is also the case in other linguistic traditions, but not in all. Some linguists write grammars for a given corpus, at times on principle, and at times because they are forced to do so for lack of informants. Without taking position on the problem of whether or not intuitions constitute a sufficient basis for a complete language theory, we can in any case propose that their importance in linguistics is essentially limited by the degree to which they are unreliable. It is a dangerous practice in linguistics to conclude from the lack of psychological information on the process of linguistic judgment that intuitions are indeed reliable. Although incidental words of caution may be found in linguistic literature, their effect is negligible. Chomsky warns his readers that he does not mean "that the speaker's statements about his intuitive knowledge are necessarily accurate" (Chomsky 1965), and further states that

> in short, we must be careful not to overlook the fact that surface similarities may hide underlying distinctions of a fundamental nature, and that it may be necessary to guide and draw out the speaker's intuition in perhaps fairly subtle ways before we can determine what is the actual character of his knowledge of his language or of anything else.

As we pointed out in Volume II, Chapter 1, Chomsky (1957) emphasizes that, as far as possible, grammars should be constructed on the basis of clear cases with regard to grammaticality. If the grammar is adequate for those cases, the status of less clear cases can be deduced from the grammar itself, and the intuitive judgment is no longer necessary.

After the first phase of the development of transformational generative linguistics, little seems to remain of these two directives in linguistic practice. Instead of an increasing number of cases in which the theory decides on the grammatical status of half-acceptable sentences, we find an enormous increase of examples in which sentences of doubtful grammaticality are applied as tests of syntactic rules.

In order to show how serious this development is, we offer an elaborate example of it. Fourteen sentences taken from a reader on transformational linguistics (Jacobs and Rosenbaum 1970) follow. In that book, each of the sentences is marked by the author concerned[1] as grammatical or ungrammatical. We shall allow the reader himself, however, to decide which of the sentences were marked ungrammatical in the original text. The original judgments of the respective authors may be found in a note at the end of this chapter. In making his judgment, the reader should imagine that the sentence is presented to him in spoken form.

(1) *Your making of reference to the book displeased the author* (Fraser)
(2) *No American, who was wise, remained in the country* (Postal)
(3) *They never insulted the men, who were democrats* (Postal)
(4) *They never agreed with us planners* (Postal)
(5) *The talking about the problem saved her* (Fraser)
(6) *The machine's crushing of the rock was noisy* (Fraser)
(7) *The giving of the lecture by the man who arrived yesterday assisted us* (Fraser)
(8) *Your making of a reference to the book displeased the author* (Fraser)
(9) *Her slicing up of the cake was clever* (Fraser)

[1] The name of the author is given in parentheses after each sentence.

(10) *John's cutting up of four cords of wood yesterday and his doing so again today was a welcome gesture* (Fraser)
(11) *John's tendency to sleep along with Mary's tendency not to do so ruined the party* (Fraser)
(12) *I didn't believe it, although Sid asserted that Max left* (Lakoff)
(13) *I did't believe that John would leave until tomorrow* (Lakoff)
(14) *His criticism of the book before he read it* (given as a noun phrase) (Chomsky)

We used these fourteen sentences as a demonstration example for a group of twenty-four trained linguists, and asked them to judge which sentences were marked as ungrammatical in the original text. The results of this little experiment, also given in the note at the end of this chapter, showed that the sentences marked ungrammatical by the authors had half as much chance of being judged ungrammatical by the linguists as those marked grammatical by the authors. This is precisely the opposite of what might have been expected. Though this experiment (reported in further detail in Levelt 1972) was not watertight because none of the judges was a native English speaker (but all had had higher education in English and many were specialists in the study of the English language); however, the results are alarming enough to incite us to caution in the use of linguistic intuitions. These fourteen sentences can also act as the basis of a discussion of a number of factors which contribute to the unreliability of linguistic judgment but are systematically underestimated and often denied by linguists.

The context of linguistic presentation. The grammatical status of many examples among sentences (1) to (14) is well indicated in the original articles, but outside of that context, the same sentences become problematic. The development of the argument in a linguistic article influences the grammaticality judgment in a way which has not yet been investigated.

Comparison with other sentences. A sentence which appears to be grammatical in isolation can nevertheless become ungrammatical when it is compared with other sentences. Sentence (1), for example, loses much of its grammaticality if sentence (8) is presented first. Another example is the doubtful grammaticality of the following sentence:

(15) *Tom was not present, and many of the girls believed that the paper had been written by Ann and him himself.*

Ross marks the sentence grammatical (in Jacobs and Rosenbaum 1970) in contrast to the following sentence:

(16) *Tom was not present, and many of the girls believed that the paper had been written by Ann and himself*

which, in his opinion, is ungrammatical.

Judging isolated sentences differs very much from judging contrasting sentences. Which of the two methods is to be preferred? It is the exception rather than the rule that a stable criterion can be maintained by a judge in an actual judgment situation. Psychology makes it quite clear that such a criterion will be sensitive to pay off, that is, independently of the possibility of distinguishing grammaticality from ungrammaticality, the percentage of judgments "grammatical" will increase when the judge feels that such a reaction is desired of him. This is anything but an imaginary factor in present linguistic practice. As we have already pointed out, a linguistic article in itself can often induce a certain expectation in the first place, and that expectation can influence the criterion in one direction or in the other. But when the linguist is his own informant, reward or pay off and the criterion can no longer be distinguished from each other. The linguist's theoretical expectation on a given sentence is also determinant for the position of the criterion of grammaticality, but its influence will not necessarily be in a direction advantageous to the theory. The critical (or hypercritical) linguist can also show the reverse tendency. The point is that it is an illusion to think that an objective absolute judgment of grammaticality is possible. Abstracting from the effects of pay off, absolute judgments usually also show the effects of a central tendency. If a sequence of certainly grammatical sentences is given, followed by a somewhat less grammatical sentence, this latter has a good chance of being judged as ungrammatical. Beside the reward effect, there is a tendency toward a fifty-fifty criterion. It would be best advice for the linguist who wishes to show a sentence of doubtful grammaticality to be grammatical to place that sentence at the end of a series of strongly ungrammatical examples.

There is, therefore, good reason to mistrust absolute judgments of grammaticality. Judgment of contrasting examples, in which the position of the criterion no longer plays a role, seems to be a considerably safer procedure. This does, however, lead to a type of linguistic data which is related to the linguistic theory in a different way. We shall return to this problem of interpretation later.

The use of unnatural and misleading examples. This is a common practice everywhere, as we see in the following examples, taken from the same reader;

(17) *The number of dollars that a dozen eggs cost in China is greater than the number of degrees centigrade the temperature was in Chicago* (Hale)

(18) *That Tom's told everyone that he's staying proves that he's thinking that it would be a good idea for him to show that he likes it here* (Langendoen)

(19) *I dreamed that I was a proton and fell in love with a shapely green-and-orange striped electron* (McCawley)

(20) *Tom thinks that I tried to get Mary to make you say that the paper had been written by Ann and him himself* (Ross)

In all of these cases, misleading factors are expressly introduced, rather than being eliminated. This can only increase the unreliability of the judgments. The reader may give his judgment on sentences (17) to (20); the judgments of the original authors are given at the end of this chapter.

The linguist as his own informant. The transformational linguist usually bases his arguments on his own intuitive judgments.

We have already pointed out that certain theoretical expectations on his part can influence the position of the criterion in the judgment situation. But the combination of linguist and informant can also be the source of problems in other judgment situations, such as the judgment of grammaticality on the basis of contrasting sentences and paraphrase judgments. The question is how the nature of the criterion is related to the linguistic training of the investigator. This is a particular instance of an old psychological problem — the use of trained subjects. At the beginning of this century, the Würzburg studies on thinking (Ach, Bühler) were among the first in which trained subjects were used for systematic introspection. The practice was a source of much vexation to Wundt, who on more than one occasion (1907; 1908) rejected it as unscientific. Van de Geer (1957) gives a survey of the discussions on the matter, and wonders in which field the subjects were actually trained. He writes that in Wundt's day

> training was assumed to be an unlearning of bad perceiving-habits, not the learning of a specific technique of perceiving. Nowadays we are inclined to say that the subjects were trained in a specific technique, and we recognize that different training systems may lead to different results

and further,

> one serious objection can be maintained: the special training of the subjects and the impossibility to see in how far the Würzburg results are a consequence of this training. This objection is the more cogent as other studies produced results which were at variance with those of the Würzburg school.

These considerations are almost literally applicable to the present situation in linguistic practice. Chomsky's warning, quoted at the beginning of this paragraph, is based on the supposition that a careful elimination of external factors (such as surface similarities) in the judgment situation will lead to the discovery of the "real" underlying knowledge. Linguistic training is useful in that it makes the judge aware of such factors: thanks to his training, the linguist unlearns his bad perceiving habits. But, in linguistic literature, we seldom find instances of awareness of the fact that linguistic training also determines the form of the criterion itself, which expresses itself in a certain judgment technique. To illustrate this, we need not even compare the different schools of linguistics. A linguist trained in the transformational grammar of the type presented in *Syntactic Structures* will judge the string *colorless green ideas sleep furiously* as grammatical (in the restricted syntactic sense of the word), although it is semantically abnormal. A linguist trained in the *Aspects* theory will find the same string ungrammatical, because (syntactic) lexical insertion rules have not been respected in its derivation. The linguist trained in generative semantics, on the other hand, will in turn judge the string as grammatical, because the selection restrictions which have been violated are purely semantic in nature. We see here that the same phenomenon is alternately called semantic and syntactic, independently of the form of the theory, and this in turn determines the nature of the criterion of judgment. In this regard, judgments can only confirm the theory. If we hold the convention that selection restrictions are semantic, it is the theory which decides that *colorless green ideas sleep furiously* is syntactically correct. The judgment of the linguist adds nothing to this. We do not wish to say by this that it is pointless to train informants. One can make the judge aware of a particular characteristic of sentences, of theoretical importance at that moment. The theory can be tested on such judgments. One would, however, prefer that linguists were completely explicit on the nature of the criterion which they use in their judgments. With sufficient precautions, the use of trained subjects can indeed be useful, as is apparent in the history of psychology. The entire field of human psychophysics is based on experiments in which trained subjects were used, and it does not appear that any great problem occurred.

Written language versus spoken language. Linguistic judgment is often clearly based on the written form of the sentence, and at times even on the punctuation. Sentence (2) would be a good example of this. It remains an open question as to whether or not there is an acoustic equivalent to the commas when the sentence is spoken. If not, it is impossible to hear whether the relative clause is restrictive or expansive. It is also unclear whether this distinction is indeed of syntactic nature, or whether we are dealing with a semantic or even pragmatic characteristic which has come to be expressed in writing in our culture in the form of punctuation.

2.2 FROM DATA TO MODEL

If we suppose that all the problems of reliability mentioned in the preceding paragraph have been solved, we must still ask what the linguist can do with his reliable data. Data would offer the linguist the opportunity to test his theory, but this does not work only in one direction. The theory (grammar) determines which data are relevant, or, in other words, which linguistic intuitions must be investigated in order to justify certain conclusions. In Volume II, Chapter 1, we stated that the formal relations between data and theory are elaborated in the theory of linguistic interpretation. This theory may be said to indicate how the data (intuitions) are represented in the model (the grammar). In this respect the theory of interpretation fills the same function in linguistics as measurement theory in the social sciences (cf. Krantz, et al. 1971). But unlike the measurement theory, the theory of interpretation is only at the first stage of its development. In Volume II we discussed two cases in which the absence of a theory of interpretation had serious consequences for the testing of a linguistic model. The first case was Chomsky's rejection of the regular model for natural languages (Volume II, Chapter 2, section 2.2.). We showed that the data used by him were insufficient to justify his conclusion. On the ground of a bit of interpretation theory developed there ad hoc, however, it was possible to refer to, and (to a certain extent) to find, data which could tentatively justify the conclusion. The second case was Postal's rejection of the context-free model for natural languages (Volume II, Chapter 2, section 2.3.). His "proof" did not relate his data to the model he tested.

Postal's argument nevertheless appeared quite clear on first examination, and many references are made to it in linguistic theory. Without interpretation theory, such snares remain for the linguist. The choice between absolute judgment of grammaticality and judgment by contrast, for example, has repercussions for the possiblity of testing the grammar. In the following paragraph we shall illustrate this to a certain degree, but without a theory of interpretation, such considerations remain ad hoc remarks.

The remainder of this chapter will deal with the formal aspects of interpretation of two types of linguistic intuitions, namely, those concerning grammaticality and those concerning syntactic cohesion. On the first point, hardly any experimental work has been done on the testing of formal linguistic theories. Our remarks will therefore be limited to a few fundamental aspects of the relationship between data and theory, in particular concerning judgments of grammaticality by contrast. The second topic is an exercise in linguistic interpretation. Without arriving at definitive conclusions, we will show how a formal interpretation theory can be constructed and tested experimentally.

2.3. THE JUDGMENT OF GRAMMATICALITY: ABSOLUTE JUDGMENT VERSUS JUDGMENT BY CONTRAST

It is only since the generative point of view became common in linguistics that the absolute judgment of grammaticality came to be of great importance to theory. In *Syntactic Structures* Chomsky wrote:

> The fundamental aim in the linguistic analysis of a language *L* is to separate the *grammatical* sequences which are the sentences of *L* from the *ungrammatical* sequences which are not sentences of *L* and to study the structure of the grammatical sequences. The grammar of *L* will thus be a device that generates all of the grammatical sequences of *L* and none of the ungrammatical ones.

Only absolute grammaticality defines the language, and the language is what the linguist describes. Relative grammaticality or gradation of grammaticality is an interesting but secondary problem in this point of view. The principal distinction is that between grammatical and ungrammatical, and an order of grammaticality can be determined only for the ungrammatical sentences. Chomsky (1964) and others (Katz 1964; Ziff 1964; Lakoff 1971) have developed theories on degrees of ungrammaticality. They are all based on the consideration that given a grammar, by the systematic violation of certain rules, ungrammaticality can be varied as a function of the seriousness and the number of those violations. None of these theories has ever been the object of direct experimental tests.[1] Experimental work on grammaticality, such as that of Maclay and Sleator (1960) and of Quirk and Svartvik (1965), has been concerned principally with the relations between that which subjects understand by "ungrammaticality" and other experimental variables, such as judgment of "meaningfulness" of a sentence, and behavioral tests concerning the sentence (accuracy in making a semi-grammatical sentence passive or interrogative, etc.). Systematic predictions which can be made on the basis of the formal theory have never been tested. Given the secondary importance of the phenomenon of ungrammaticality, such tests would moreover have been rather indirect for such formal theories. According to the point of view of *Syntactic Structures*, it is in the grammatical sentences, and therefore in absolute grammaticality, that the real interest lies.

It should be pointed out, however, that, in other schools of linguistics, *relative* grammaticality is at least as important a notion. In Harris' transformation theory (both his earlier theory and his present operator grammar), paraphrastic transformation is defined by an equivalence relation between two classes of sentences in the language (for example, active and passive), with the property that the order of acceptability within the two classes is equal. The supposition behind this argument is that sentences in the language do indeed vary in acceptability. It should be noticed that acceptability is not identical to grammaticality, but Harris does not make

[1] During the translation of this book an article by Moore (1972) appeared in which Chomsky's theory was investigated experimentally with a completely negative result.

the distinction, and the context makes it clear that for him acceptability is a theoretical linguistic concept. It may therefore be said that RELATIVE GRAMMATICALITY is the central notion here; it is a condition on paraphrastic transformations. It is in fact nothing other than the supposition that transformations are independent of each other in their effect on the acceptability of the sentence. Suppose that sentence x is more acceptable than sentence y; we may represent this as $x \succ y$. Harris' condition states that if x' and y' are transformations of x and y, respectively, by means of the application of the optional (paraphrastic) transformation t_1, then it must hold that $x' \succ y'$. Suppose that x'' and y'' are, in turn, transformations of x' and y' by application of transformation t_2; it must then hold that $x'' \succ y''$. In other words, the order of acceptability resulting from the application of t_1 cannot be reversed by the application of t_2. But notice that this condition of independence says nothing of the effect of the transformation itself on acceptability. The effect of t_1 can as easily be $x \succ x'$ as $x \prec x'$. We shall return to this subject later.

There is nothing in *Aspects* to render this assumption of independence of optional transformations improbable. However, such an assumption is quite irrelevant to the framework of the *Aspects* theory in the first place. All the deep structures generated by the base are grammatical; on that level there is no gradation in grammaticality, and consequently, the assumption of independence is trivial. But in the practice of later developments, the very definition of deep structure becomes problematic. In the framework of generative semantics it is no longer clear how the underlying structures are generated, and one cannot be certain in advance that a given underlying structure can be accounted for on the basis of the (unknown) base grammar. This need not be a great hindrance to the investigation of transformational relations among sentences, provided that attention is paid to the fact that sentences of doubtful grammaticality can again play a role in the testing of transformations, and, as we have seen in the preceding section, this is a real problem. Under these circumstances the principle of independence becomes interesting once again.

One of the tests for the correct formulation of a transformation will be to see if it has any influence on the order of acceptability of the sentences. The investigation of grammaticality by contrasting sentences will be sufficent to test this prediction. For a given transformation a number of characteristic sentences x_1, x_2, ..., is determined where it holds for every pair x_i, x_j, that if $x_i \succ x_j$, then $x'_i \succ x'_j$ (when x and x' differ only in that x' has had the transformation in its derivation and x has not). It is therefore of secondary interest to know whether the sentences x satisfy the highest norms of grammaticality. Uncertainty in that regard does not effect the validity of the test to the extent that the transformation must in any case be rejected if the order of grammaticality is reversed. It is not the case, however, that the test is also *sufficient* for the demonstration of the transformation. Imagine, for example, the following transformation: t permutates every article with the rest of the noun phrase. Thus, if x = *the child eats an ice cream*, then $t(x) = x'$ = *child the eats ice cream an*. Suppose we have y = *the ice cream eats a child*, and $x \succ y$, then it decidedly also holds for $t(y) = y'$ that $x' \succ y'$: *child the eats ice cream an* is more grammatical than *ice cream the eats child an*. But t is clearly not a transformation in English. A further requisite which must apparently be stated is that an optional transformation should not decrease grammaticality; that is, it may never happen that $x \succ x'$, where x' is the transformed equivalent of x. Indifference, indicated by $x \sim x'$, seems to be an acceptable situation, however. A second condition on optional transformations is therefore $x \precsim x'$: the transformation may not lead to a decrease in grammaticality. It is also the case for this second condition that it is of little importance that there be some doubt as to the grammaticality of x, thanks to the condition of independence. Judgment of grammaticality by contrast is also sufficient for testing $x \precsim x'$.

As long as no theoretical objections can be brought against the assumption of the independence of optional transformations, judgment of grammaticality by contrasting sentences can play an important role in the testing of such transformations.

What is the situation for obligatory transformations? The consequence of omitting the application of an obligatory transformation is the derivation of an ungrammatical string. If x is such a string and x' is the corresponding grammatical sentence, then $x \prec x'$. If there is uncertainty as to the grammaticality of x', as occurs at times in present practice, this inequality can nevertheless be investigated by means of judgment by contrast. We must, however, be sure that subsequent transformations cannot reverse the order of grammaticality. For this reason we must once again call upon the assumption of independence. As long as there are no theoretical objections, obligatory transformations can also be investigated to a large extent by means of judgment by contrast. In that case the absolute grammaticality of the example sentences is of minor importance. To sum up, the following tests by contrast may be performed for transformations:

for optional transformations: $x \precsim x'$
for obligatory transformations: $x \prec x'$, for all pairs x, x'
for both: if $x \succ y$, then $x' \succ y'$, for all pairs x, y and x', y'

Finally, we would point out that ranking judgments can give the linguist some insight into the futility of his problem. In the opinion of some authors (e.g. Suppes 1970), linguists are fond of all sorts of marginal transformational phenomena, while the attention paid to the more essential linguistic rules is considerably less. Obligatory linguistic rules could be ranked according to *importance in the grammar*. Violation of the more important rules leads to serious failure of communication, while violation of marginal rules leads only to rather mild forms of ungrammaticality. Let c be a central rule and m a marginal one. Let $x(c)$ be the sentence in which only rule p, and not rule c, is violated; let $x(m)$ be the inverse situation. In that case, $x(c, m) \succ x(c) \succ x(m)$. These inequalities can in turn also be investigated by means of judgment by contrast, and, in principle, rules of the grammar can be ranked according to importance. Thus one could retain minor problems for freshman students, and all dissertations could, on the other hand, be relevant.

2.4. THE JUDGMENT OF SYNTACTIC RELATEDNESS: A FEW MODELS OF INTERPRETATION

The intuition of grammaticality is the very basis on which generative linguistics defines a natural language. By definition, the correct prediction of that intuition by a grammar signifies observational adequacy. But if a grammar G is observationally adequate for language L, all grammars equivalent to G are also observationally adequate for L. Judgment of absolute grammaticality is therefore neutral in regard to the descriptive adequacy of a grammar. Thus we have seen, for example, that a context-free grammar is weakly equivalent to a dependency grammar. Judgments of absolute grammaticality can never differentiate between these two types of grammars. Judgments of relative grammaticality can say something on descriptive adequacy, but only in an indirect way. Two weakly equivalent grammars will, in general, define two different rank orders of ungrammaticality over the complement of the language. The testing of such gradations can therefore give indirect evidence for the descriptive adequacy of the grammar. But this is an unsatisfactory method — much as if psychology could only work with rats, or theology alone with fellow-men.

For the direct investigation of the descriptive adequacy of a grammar, that is, for the investigation of the correctness of the structural descriptions, intuitive judgments of another nature are needed; we call them STRUCTURAL INTUITIONS. One of the most often used structural intuitions is expressed in the paraphrase judgment. Two sentences with the same underlying structure are, according to the *Aspects* model, paraphrases of each other. The paraphrase judgment offers the possibility of concluding that two sentences differ in deep structure. Without underestimating the linguistic importance of such judgments, we would point out that the psychological problems which occur in this connection are even more treacherous — if that be possible — than those which occur in the judgment of grammaticality. The principal problem is that two sentences are never complete paraphrases of each other. The manner and degree in which they are paraphrases is dependent on various syntactic, semantic, and pragmatic factors. Compare, for example, the following sentences, all of which are paraphrases:

(1) *I disapprove of the fact that John catches butterflies.*
(2) *I disapprove of the fact that butterflies are caught by John*
(3) *I disapprove of John's catching butterflies*
(4) *I do not like the idea of John's catching butterflies*
(5) *I object to John's catching butterflies*
(6) *I do not consent to John's catching butterflies*

We shall not discuss this question here. For the only psychological investigations on paraphrase judgments known to us, we refer to Gleitman and Gleitman (1970) and Honeck (1971).

In this section we shall discuss a type of structural intuition which is sometimes used in linguistic practice and which can offer direct insight into the structure of the sentence: intuitions on syntactic cohesion. Cohesion intuitions are expressed in judgments on whether or not words or phrases belong together in a sentence. Chomsky (1965) uses cohesion intuitions for the study of relations between the main verb and prepositional phrases:

> It is well known that in Verb—Prepositional Phrase constructions one can distinguish various degrees of "cohesion" between the verb and the accompanying Prepositional Phrase.

He illustrates this with the sentence *He decided on the boat* which can be read in two ways. *On the boat* refers either to the place or to the object of the decision. This is clear when we compare it with the following nonambiguous sentence: *He decided on the boat on the train.* Chomsky writes that in the latter sentence "the first prepositional phrase ... is in close construction to the verb", and he modifies the base grammar to agree with this insight. Cohesion is a direct and potentially valuable structural intuition, but its use in linguistics demands a theory of interpretation which establishes the relation between syntactic structure and cohesion judgment. The absence of such a theory easily leads to quasi-arguments and to confusion. Thus Uhlenbeck (1964) correctly pointed out that a parsing such as *(the man saw) (the* *boy)* would not be in conflict with cohesion intuitions, quite in agreement with experimental results of the type to be discussed presently. The fact that a parsing such as *(the man put) (it into the box)* does conflict with intuition (Chomsky 1965) calls for explanation, but it is not an argument against the first analysis without further theory on cohesion intuitions.

2.4.1. *Methods for the Measurement of Syntactic Relatedness*

Let us use the simple sentence *John breaks in* as an example. There is a gamut of methods for having subjects judge how strong the syntactic relations are among the three words of this sentence. The following three, however, are the most common in the literature (in all three the judge is exposed to the complete sentence and is instructed constantly to relate his judgments to it).

(i) *Rank ordering of Word Pairs*

The example sentence contains three word pairs — *(John, breaks)*, *(breaks, in)*, and *(John, in)*. The subject is asked to rank these word pairs according to relatedness. The most probable result is (from strong to weak): *(breaks, in)*, *(John, breaks)*, *(John, in)*. For longer sentences, where the number of pairs becomes quite large, the task can be facilitated in several ways. One of these is TRIADIC COMPARISONS, in which the subject must indicate for every triad of words from the sentence which pair has the strongest relation in the sentence, and which has the weakest. The triads may be presented, for example, as shown in Figure 2.1. The subject marks his judgment in every triangle by placing a plus sign (+) at the side of the triangle showing the strongest relation, and a minus sign (—) at the side showing the weakest relation. When every triad for the sentence has been judged, each word pair can be assigned a number which represents the relatedness judgment. This can also be done in various ways. One of these consists of counting the number of times a word pair is judged as stronger than other word pairs. Thus, in Figure 2.1., the pair *(breaks, in)*

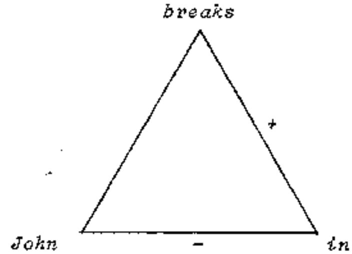


Fig. 2.1. An example of triadic comparison

is judged as more strongly related than either *(John, breaks)* or *(John, in)*; this gives a score of 2. The pair *(John, breaks)* has a score of 1, because it is more strongly related than only one other pair, *(John, in)*, which in turn has a score of 0. If there are more than three words in the sentence, the scores are added for all the triads in which the word pair occurs, yielding the final score for the pair. Other methods of determining the final score are also possible, but we need not describe them here.

(ii) *Assigning Scale Values to Word Pairs*

The subject may be asked to indicate the degree of relatedness of a word pair by means of a number. The most common method for this is the SEVEN-POINT SCALE. The subject indicates his judgment on each word pair by circling a scale value, as shown in Figure 2.2., where *(John, breaks)* has the value of 5, *(John, in)* has 2, and *(breaks, in)* has 6.

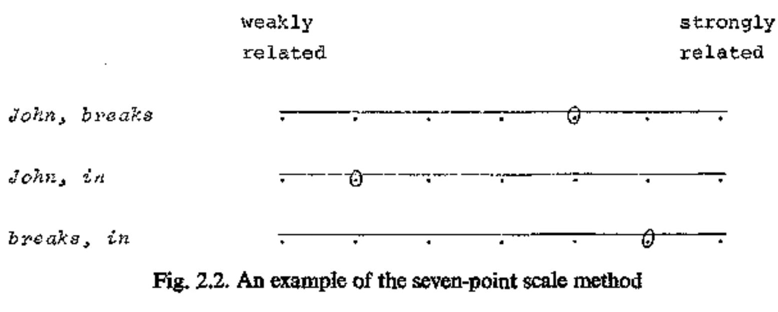


Fig. 2.2. An example of the seven-point scale method

Another common method is that of MAGNITUDE ESTIMATION. For a given word pair (the "standard"), a certain scale value is assigned beforehand, and it will be on the basis of this standard that the subject will make his judgments. Thus if the standard is *(John, breaks)* with an assigned standard value of 50, another pair, which intuitively has half as strong a relation, receives the judgment 25. This could for instance be the case for *(John, in)*. In the same way, a pair which is related twice as strongly will be assigned the value 100 by the subject. This could in the example be the case for the pair *(breaks, in)*. In general the subject will estimate the strength of the relations on the basis of the standard. There are many other methods by which the subject can assign numerical values to word pairs, but only those mentioned here are ordinarily used for this purpose.

(iii) *Word Sorting*

The subject can be presented with the entire sentence, as well as with the individual words, each of which is written on a separate card. He is then asked to sort out the cards into stacks according to the relatedness of the words in the sentence. Thus syntactically related words should be placed in the same stack, while words which have little to do with each other should, as far as possible, be placed in separate stacks. The informant may make as many stacks as he likes, and each of the stacks may contain as many cards as he likes.

In this way each word pair is given a score of either 1 or 0. When two cards are placed in the same stack, they have a relatedness score of 1, and when they are placed in separate stacks, they have a score of 0. Suppose that given the sentence *John breaks in*, a subject makes two stacks, {*John*} and {*breaks, in*}. This renders a score of 1 for *(breaks, in)*, and a score of 0 for the other two pairs. A dichotomy of this sort is not usually very informative. The sorting process would have to be repeated by other subjects (or possibly also the same subject) in order to obtain more gradation in the values. The scores for each word pair could then be added.

The maximum total score for a word pair would then be equal to the number of subjects (or trials); it would be obtained if two words were grouped together by all subjects, or by the same subject at all trials.

Each of these methods of judgment has its own advantages and disadvantages. We shall not discuss them here, but we shall mention them later when necessary.

It is obvious that all of these methods must be accompanied by careful instruction, in which it is explained to the subjects that the problem is one of sentence structure, and not of meaning relations which may by chance exist between the words of the sentence. This can be illustrated for the subjects by means of various examples. Experience has proven that subjects in general have little difficulty in understanding their task.

2.4.2. *A Constituent Model for Relatedness Judgments*

An interpretation theory is necessary in order to connect relatedness judgments to a linguistic theory. The purpose is, of course, to test the linguistic theory on the basis of as plausible an interpretation theory as possible. In this paragraph we shall elaborate a theory of interpretation, by way of example. In the following paragraph we shall mention another model in less detail, although that model is perhaps more promising.

As we have stated, a model for the judgment of syntactic relatedness has two components — a linguistic theory and an interpretation theory. For a simple linguistic theory we take a constituent structure grammar. We must first define the concept of COHESION within this theory. This means that the intuitive notion of cohesion as used by Chomsky and others must be formally related to the linguistic theory, and in particular, with the constituent structure of the sentence. To do this we return to Chomsky's example, the sentence *he decided on the boat on the train*, where *on the train* has less cohesion with *decided* than *decided* has with *on the boat*. In Chomsky's analysis, this is expressed in the structural description in Figure 2.3. We see in the figure that the difference in

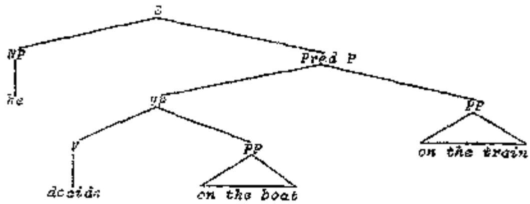


Fig. 2.3. Phrase marker for the sentence *he decided on the boat on the train* (abbreviated)

cohesion is tacitly attributed to a relation of inclusion of constituents in the phrase marker. The verb phrase *VP* is part of the predicate phrase *Pred P*, and this justifies the fact that the cohesion between the parts of the verb phrase *(decide, on the boat)* is greater than the cohesion between one of these and elements of the predicate phrase which lie outside the verb phrase *(on the train)*. A general formulation of this is as follows: the constituents of a sentence vary in cohesion, and the cohesion of a constituent is smaller than the cohesion of its parts. This is still nothing other than a faithful explicit representation of a more or less implicit linguistic notion. Without changing anything essential in the formulation, we can define the concept of cohesion mathematically as follows:

DEFINITION (Cohesion): A real-valued COHESION FUNCTION α is defined over the nodes of a phrase marker *P*, with the following property: if $A \rightarrow B$, then $\alpha(A) < \alpha(B)$, for all nodes *A*, *B* in *P*, where $A \rightarrow B$ means that there is a descending path in *P* from *A* to *B*. The COHESION of a constituent *C*, $\alpha(C)$, is defined as $\alpha(K)$, where *K* is the lowest node in *P* which dominates *C* and only *C*.

It follows from the definition that for every path from root to terminal element, the cohesion values of the nodes increase strictly. Consequently the cohesion of a constituent is necessarily smaller than that of its parts.

The following step is the formulation of the theory of interpretation. This theory must indicate how the strength of the relation between two words, as judged by an informant, is connected with sentence structure. Let us imagine that we have performed such an experiment for a given sentence, and that the results of the experiment are summarized in a relatedness matrix R, in which the strength of the syntactic relation is indicated for every word pair in the sentence. Thus matrix element r_{ij} in R is the score for the degree of relatedness between words i and j. The score is obtained in one of the ways described in the preceding paragraph.[1] The interpretation theory must attempt plausibly to relate the observed r-values to the (theoretical) cohesion values α. An obvious place to begin would be to find the smallest constituent for every word pair (i, j) to which both words belong, and to compare their degree of relatedness with the cohesion value of the constituent. Let us call that constituent the SMALLEST COMMON CONSTITUENT, *SCC*, of the word pair. Each word pair in the sentence evidently has one *SCC* and only one. Thus in Figure 2.3. the smallest common constituent of the word pair *(decide, on)* is the verb phrase *decide on the boat*, with cohesion $\alpha(VP)$; the smallest common constituent of *(he, decide)* is the sentence *he decide on the boat on the train*, with cohesion $\alpha(S)$. Shall we make r_{ij} equal to the cohesion α of the *SCC*? That would not be wise, because the r-values are dependent on the experimental procedure followed. With the word sorting method, for example, the average r-value doubles when the number of subjects is doubled. Also one might expect that there is no linear relationship among the r-values given by the various methods. The word sorting method, for example, makes small differences between pairs with limited syntactic relation, while the seven-point scale yields considerable variations in r for the same pairs. The only thing which we can hope for and expect is that all methods yield the same rank order of r-values. The most careful approach, therefore, is to establish no direct relationship between r-values and α's, but only between the rank order of the r-values and the rank order of the α's. The following

[1] For the moment, we shall not discuss the effect of experimental noise. In fact the interpretation theory only regards real r-values, i.e. those corrected for errors in measurement.

interpretation axiom states that the rank order of the r-values must agree with the rank order of the α's of the smallest common constituents concerned.

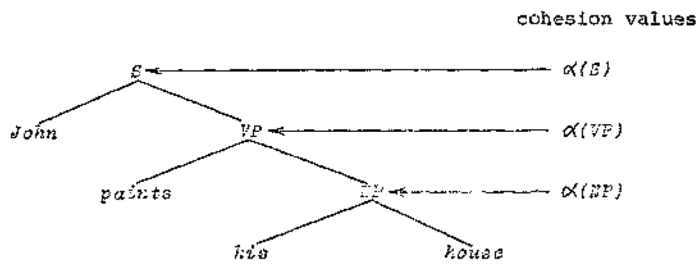
Interpretation axiom: For all words i, j, k, l in the sentence,

$$r_{ij} < r_{kl} \Leftrightarrow \alpha(SCC_{ij}) < \alpha(SCC_{kl}).$$

In this axiom, $\Leftrightarrow$ stands for "if and only if", and SCC_{ij} (SCC_{kl}) stand for the "smallest common constituent of words i and j (k and l)".

Although only inequalities are formulated in the axiom, it follows by exclusion that equal degrees of relatedness go together with equal cohesion values, and vice versa. It may be said that there is a strictly increasing relation between r and α. Equal r-values can, of course, occur by errors in measurement in the experiment, but in the theoretical error-free situation, it is a sufficient and necessary condition that the corresponding α's be equal. In particular it follows from the axiom that word pairs which have the same SCC will also have equal r-values.

EXAMPLE 2.1. In Figure 2.4. a theoretical phrase marker is given for the sentence *John paints his house*, together with the table of smallest



	John	*paints*	*his*	*house*
John	*John*	*S*	*S*	*S*
paints	*S*	*paints*	*VP*	*VP*
his	*S*	*VP*	*his*	*NP*
house	*S*	*VP*	*NP*	*house*

Fig. 2.4. Theoretical phrase marker for the sentence *John paints his house*, with cohesion values and table of smallest common constituents

common constituents for each word pair. On the basis of the definition of cohesion, the following inequality holds for this structure: $\alpha(S) < \alpha(VP) < (NP)$, and from this we can deduce the following equalities and inequalities by means of the interpretation axiom.

equalities: $r(John, paints) = r(John, his) = r(John, house)$
$r(paints, his) = r(paints, house)$

inequalities:

$$r(John, paints) < \begin{cases} r(paints, his) \\ r(paints, house) \\ r(his, house) \end{cases}$$

$$r(John, his) < \begin{cases} r(paints, his) \\ r(paints, house) \\ r(his, house) \end{cases}$$

$$r(John, house) < \begin{cases} r(paints, his) \\ r(paints, house) \\ r(his, house) \end{cases}$$

$$r(paints, his) < r(his, house)$$
$$r(paints, house) < r(his, house)$$

These inequalities are not always independent of each other. Thus $r(John, house) < r(his, house)$ follows from the combination of $r(John, house) < r(paints, his)$ and $r(paints, his) < r(his, house)$.

These predictions on equality and inequality can be tested by means of a judgment experiment. If the results of the experiment conflict with the predictions, the theoretical phrase marker or the interpretation axiom, or both, are incorrect. In judging deviations, account must be taken of errors of measurement. Absolute equalities in particular, will seldom occur. Strictly speaking, then, we must see if the observed relatedness values are equal within the tolerance of the measurement error. The measurement error can likewise change inequalities to equalities, or even to their opposites. However, we shall burden the further discussion in this section as little as possible with statistical considerations, and direct our attention to clear data from which it is possible to draw conclusions regarding our main problem, the relation between formal grammar and interpretation theory.

Given the interpretation axiom, we can study which phrase marker is most fitting for the observed relatedness values for a given sentence. If we have no particular theoretical expectation concerning the phrase marker, we can draw up a list of the predicted equalities and inequalities for every possible phrase marker in order to find the phrase marker which best agrees with the relatedness data. In doing so we should remember that different phrase markers for a single sentence do not always lead to the same number of equalities and inequalities. In general, however, we will certainly have particular theoretical expectations concerning syntactic structure, and it will be possible to limit the test to alternatives within that theoretical domain. The following is an experimental example of this.

For the sentence *the boy has lost a dollar*, only the phrase markers in Figure 2.5. are worth consideration. In an experiment described

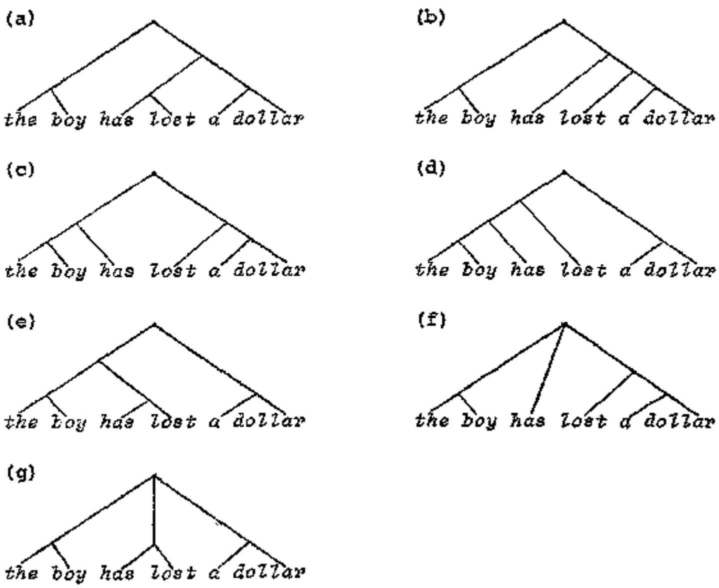


Fig. 2.5. Possible phrase markers for the sentence *the boy has lost a dollar* (node labels omitted)

elsewhere (Levelt 1967a), twenty-four native speakers of English judged this sentence by means of the method of triadic comparison. Table 2.1. shows the relatedness values obtained for the various word pairs. The value for a word pair was obtained by adding the scores for that pair in each triad and for each subject; it is expressed in a percentage.

TABLE 2.1. Relatedness Values for the Sentence *the boy has lost a dollar*

	the	*boy*	*has*	*lost*	*a*	*dollar*
the	—	99	43	29	19	16
boy		—	63	65	16	31
has			—	86	31	40
lost				—	42	70
a					—	94
dollar						—

Table 2.2. shows the number of inequalities predicted by means of the interpretation axiom for phrase markers (a) to (g), as well as the violations of these given Table 2.1. (also expressed in percentages in order to facilitate comparison of the models).

TABLE 2.2. Number of Predicted and Violated Inequalities for Phrase Markers (*a*) to (*g*) in Figure 2.5.

Phrase marker	(*a*)	(*b*)	(*c*)	(*d*)	(*e*)	(*f*)	(*g*)
Predicted Inequalities	64	67	58	67	64	46	36
Violations	9	11	7	12	8	5	0
Percentage of Violations	14	16	12	18	13	11	0

The predicted equalities are not taken into consideration here, but even without a statistical test it is quite clear that the results in this respect are in conflict with the expectations. It was predicted for all seven phrase markers that all relations between *the* or *boy* on the one hand, and *has*, *lost*, *a*, or *dollar* on the other, must be equal. Table 2.1., however, gives the very divergent values 43, 29, 19, 16, 63, 65, 16, and 31 for this. We shall return to this.

As for the inequalities, it is striking that in Table 2.2. only phrase marker (g) agrees perfectly with the data, and all other phrase markers show considerable percentages of deviation. Phrase marker (g) is the least hierarchical of the seven, and consequently the number of predictions (36) is the smallest. This is something which we meet quite regularly: the constituent model predicts less well as the hierarchy becomes more complicated. Theoretical refinements which accompany the complication of the hierarchy are not, in general, reflected in the relatedness judgments.

Sometimes the relatedness judgments for a sentence agree with no phrase marker whatsoever. This is a serious matter for, if we maintain the interpretation axiom, we must then reject the linguistic model as such, and not only some syntactic structure within the model. This gives a fundamental dimension to the investigation of cohesion. More important still than the question as to which phrase marker in a given case best agrees with the relatedness data is the logically preliminary question whether within the linguistic model (phrase structure grammar, dependency grammar, adjunct grammar, etc.) some structural description, in agreement with the relatedness data, can indeed be given. This can of course, only be investigated by seeing whether within the model at least one structural description can be given for each of various different sentences, in agreement with the relatedness data. But to answer the fundamental question, it is now less important to know precisely how that structural description looks, than to know if there is one at all. The problem is thus reduced to the following question: given a formal grammar, which properties must matrix R of relatedness values have in order to be able to find an accurate structural description within that grammatical model?

We shall at this point find that critical property for the constituent model. Let a, b, and c be three random (but different) elements (words) of a sentence s.[1] Let us imagine the three smallest common constituents for a and b, b and c, and a and c, respectively.

[1] The problem is trivial for sentences with fewer than three elements.

It is quite clear that for the three smallest common constituents, one and only one of the four hierarchical relations in Figure 2.5. must apply.

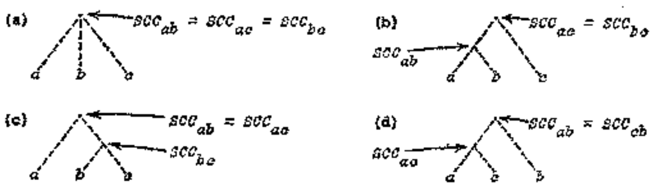


Fig. 2.5. The four possible hierarchies for three elements in a phrase marker (dotted lines indicate paths which can contain other nodes)

If (a) is the case for the phrase marker of s, we have the following definition of cohesion:

(1) $\alpha(SCC_{ab}) = \alpha(SCC_{ac}) = \alpha(SCC_{bc})$

If it is (b) we have the following relation:

(2) $\alpha(SCC_{ab}) > \alpha\,(SCC_{ac}) = \alpha(SCC_{bc})$

If the hierarchical relation is as in (c), we have:

(3) $\alpha(SCC_{ab}) = \alpha(SCC_{ac}) < \alpha(SCC_{bc})$

If (d) is the case, we have:

(4) $\alpha(SCC_{ab}) = \alpha(SCC_{cb}) < \alpha(SCC_{ac})$

By the interpretation axiom, it follows from (1) to (4) that one and only one of the relations (5) to (8) must hold for the observed degrees of relatedness of *a*, *b*, and *c*.

(5) $r_{ab} = r_{ac} = r_{bc}$
(6) $r_{ab} > r_{ac} = r_{bc}$
(7) $r_{ab} = r_{ac} < r_{bc}$
(8) $r_{ab} = r_{cb} < r_{ac}$

These relations (5) to (8) simply mean that r_{ab} must be equal to or greater than the smallest of the two other relations r_{ac} and r_{bc}. This may be summarized as in (9):

(9) $r_{ab} \geqslant \min(r_{ac}, r_{bc})$

It follows from considerations of symmetry that the inequality also holds for every permutation of *a*, *b*, and *c*. (9) is called the ULTRAMETRIC INEQUALITY. In whichever way *a*, *b*, and *c* are chosen, the relatedness values in *R* must satisfy the condition of ultrametric inequality, if representation by phrase marker is to be possible. In a different context, S. C. Johnson (1967) showed that this is not only a necessary condition, but also a sufficient one: if the matrix is ultrametric, there is a tree diagram which agrees with that matrix.

To summarize, then, it holds that the formal constituent model can be tested by establishing whether relatedness matrices satisfy the condition of ultrametric inequality (9) for all triads. If this is not the case within the measurement error, when the interpretation axiom is maintained, the constituent model must be rejected as such.

Until a short time ago (cf. Loosen 1972), however, no algorithms, let alone computer programs, were available for testing the ultrametricity of a matrix. Instead, all sorts of ad hoc means were used which need not be mentioned here (cf. Levelt 1970a; 1970b). We would only point out that Johnson (1967) developed an algorithm which, given an ultrametric matrix *R*, reconstructs the corresponding tree diagram. If the matrix is not completely ultrametric, the algorithm yields two tree diagrams, both of which represent the relations as well as possible in certain (but different) respects. A rule of thumb is that the less matrix *R* satisfies the condition of ultrametric inequality, the more the two tree diagrams differ. Measures of agreement between two tree diagrams have been used as rough indications of the ultrametricity of the matrix *R* (cf. Levelt 1970b for a detailed description of the algorithm and measures of agreement). But it is not necessarily the case that one of the two tree diagrams is also the most fitting constituent structure. The solutions given by the Johnson algorithm are, in effect, solutions which, in terms of formal grammars, are as much as possible in Chomsky normal-form (cf. Volume I, Chapter 2, section 2.3.1.), that is, they have a maximum of hierarchy. Less complicated hierarchies are avoided as much as possible by the program. This program for hierarchical clustering is therefore only of limited interest for our purposes.

We return to our original question: how adequate is the constituent model for the representation of judgments on the syntactic relations between words? In our opinion the answer is that it is inadequate. There are two reasons for this. In the first place, the condition of ultrametric inequality can be violated at will by the use of deep structure relations which cannot be expressed in the constituent structure. In the second place, independently of this, ultrametricity is systematically violated with regard to predicted equalities. We shall treat these two points successively.

The first reason can also be formulated affirmatively: that which is expressed in relatedness judgments is underlying relations among the words of a sentence. If this hypothesis is correct, we can create at will sentences whose relations no phrase marker can represent adequately. The best examples of such sentences are those from which certain words have been deleted transformationally. Take the following sentences for example: (a) *John eats apples and Peter eats pears*, which is related through the deletion of *eats* to (b) *John eats apples and Peter pears*. We should expect the model to fail in dealing with the second sentence, as we see in the following experiment (Levelt 1969; 1970c). Using the seven-point scale method, eight subjects judged all the word pair relations in sentence (a), and eight others, those of sentence (b). (In the original experiment the following equivalent Dutch sentences were used: (a) *Jan eet appels en Piet eet peren*, (b) *Jan eet appels en Piet peren*. In this case, as may be seen, the word order is the same in Dutch as it is in English.) Table 2.3. shows the total scores for the two sentences and the eight subjects (the minimum score is 8, the maximum 56).

The theoretical phrase marker in Figure 2.6.(a) gives ninety-five predictions of inequality for the relations in (a). All of them are confirmed. This constituent representation is also fitting from

TABLE 2.3. Relatedness Values for the Sentences (*a*) *John eats apples and Peter eats pears* (*Jan eet appels en Piet eet peren*) and (*b*) *John eats apples and Peter pears* (*Jan eet appels en Piet peren*).

(*a*)	*John (Jan)*	*eats*$_1$ *(eet)*	*apples (appels)*	*and (en)*	*Peter (Piet)*	*eats*$_2$ *(eet)*	*pears (peren)*
John (Jan)	—	55	48	24	36	9	10
eats$_1$ *(eet)*		—	50	16	10	32	10
apples (appels)			—	17	10	10	31
and (en)				—	19	15	16
Peter (Piet)					—	56	46
eats$_2$ *(eet)*						—	45
pears (peren)							—

(*b*)	*John (Jan)*	*eats (eet)*	*apples (appels)*	*and (en)*	*Peter (Piet)*	*pears (peren)*
John (Jan)	—	52	42	11	35	15
eats (eet)		—	49	15	45	47
apples (appels)			—	11	10	33
and (en)				—	33	24
Peter (Piet)					—	44
pears (peren)						—

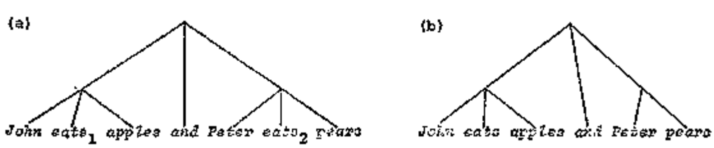


Fig. 2.6. Theoretical phrase marker for (a) *John eats apples and Peter eats pears* and (b) *John eats apples and Peter pears* (omitting category labels)

the point of view of predicted equalities,[1] but inequalities are sufficient for our argument. The corresponding constituent diagram for sentence (b), however, is considerably less in agreement with the relatedness values (Table 2.3.(b)). In fewer than half as many predicted inequalities (44), we find 4 violations, all of which are the result of the high values of *r*(*eats*, *Peter*) and *r*(*eats*, *pears*), respectively 45 and 47. These are precisely the two values for which a deviation would be expected if the subjects do in fact judge the underlying relations, for *Peter* and *pears* are the only surface elements which are directly related to the deleted element *eats*. There is no other structure for (b) which better predicts the inequalities (except, of course, the trivial coordination of all elements, on the basis of which no prediction of inequality whatsoever is possible. That structure, however, fails largely in the case of predictions of equality.) As was our plan, we have thus found a sentence whose relatedness matrix is not ultrametric for predictable reasons. This experiment was repeated with seven other deletion sentences, and the results were similar (Clarisse, unpublished undergraduate thesis, 1969). These findings are in themselves sufficient to justify the rejection of the constituent model (if the interpretation axiom is to be maintained). Before going on to discuss difficulties with equalities, the second argument for the rejection of the model, we might wonder how the model could be modified in order to meet the objections of this first type. We know how the ultrametricity of a matrix can be violated systematically, and we should be able to try to avoid this in the model without introducing important changes in the definition of cohesion or the interpretation axiom.

We therefore take a transformational grammar with a phrase structure base, as in *Aspects*, and we define the cohesion function, not for the surface structure of a sentence, but rather for its deep structure. In all other respects the definition remains the same

[1] The only great deviations from the predictions of equality result from the values for *r*(*John*, *Peter*), *r*(eat_1, eat_2), and *r*(*apples*, *pears*) which are too large. It is rather certain that this is due to a nonsyntactic factor.

as in the original constituent model. The interpretation axiom also remains unchanged.

Let us see how well such a transformational model predicts the relations for our experimental sentence *John eats apples and Peter pears*. We take the diagram in Figure 2.6.(a) as the deep structure of the sentence. We are then faced with the problem of deciding on which deep pair a surface pair must be mapped. This decision must follow from the transformation rules of the grammar. Without defining these explicitly for this sentence, we can consider the relation between *John* and *eats* as a relation between *John* and $eats_1$ in the deep structure. Likewise *r(eats, apples)* can be considered as the expression of the relation between $eats_1$ and *apples*. As for the relations between *Peter*, *pears*, and *eats*, we take *eats* as a reflection of $eats_2$ in the deep structure. For *r(and, eats)* it is irrelevant whether we consider *eats* as $eats_1$ or $eats_2$, while the relation between $eats_1$ and $eats_2$ is not expressed in the judgments of the informant. Some interpretation of the data is therefore necessary before the model can be tested. Explicit rules can be established for that purpose. One of these might be that for a given surface pair the deep pair must be selected in such a way that the smallest common constituent in the deep structure is as small as possible. That is in fact the rule which we have followed here. However we shall not discuss this point further.

With the transformational model, fifty-four inequalities are predicted for sentence (b), and all of them are verified. The transformational model also gives good predictions on inequality and degree of relatedness for other sentences in which deletion transformations have been applied (Clarisse, unpublished undergraduate thesis, 1969).

How adequate is the model for sentences in the derivation of which transformations other than deletions have been applied? In another experiment (Arnolli, unpublished undergraduate thesis, 1969) we have shown that, in agreement with the model, the degrees of relatedness for a simple declarative sentence do not differ statistically from those of its passive and interrogative equivalents. In the following four Dutch sentences, the respective word pairs did not differ significantly in degree of relatedness.

de man betaalde het geld aan een agent
'the man paid the money to a policeman'

het geld werd door de man aan een agent betaald
'the money was paid to a policeman by the man'

betaalde de man het geld aan een agent?
'did the man pay the money to a policeman?'

werd het geld door de man aan een agent betaald?
'was the money paid to a policeman by the man?'

As far as the corresponding words are concerned, these sentences do indeed have the same deep structure in the *Aspects* model. We may conclude from these and other experiments that the transformational formulation of the cohesion model agrees much better with the relatedness data, with regard to inequalities, than with the original constituent model. If such results are maintained, the model can be used for practical purposes, namely, for finding the most appropriate deep structure. Relatedness judgments allow one in a sense to bypass the transformational superstructure of a sentence and to arrive directly at the underlying relations. It is important to notice, however, that the ultrametric inequality is neither necessary nor sufficient for testing the transformational model as such. We have already seen that the matrix for *John eats apples and Peter pears* is not ultrametric, while the transformational model is in complete agreement with the relatedness data as far as inequalities are concerned. The important point is the transformation relation between the surface and deep structures. Ultrametric inequality retains its critical value only for sentences with the same deep structure as surface structure (abstraction made of nonessential changes). Such sentences are called KERNEL SENTENCES. Examples are *John has lost a dollar* and *the man paid the money to a policeman.* Whether or not a sentence is a kernel sentence will, of course, depend closely on the transformational component. If, however, the relatedness matrix for kernel sentences in a given model is not ultrametric within the measurement error, the transformational model must be rejected. Such negative information does not exist at the moment, at least as far as ultrametric inequality predictions are concerned. But the case is different for equality predictions. This brings us to the second argument against the constituent model; the argument will also hold for the transformational extension of the constituent model.

Ultrametricity can be systematically violated in matter of predicted equalities. The introduction of an endocentric construction into the sentence will be sufficient to cause this. We shall limit the discussion to constructions of the type article+noun (*the child, a policeman*, etc.). Whether we test the parsing of the surface structure or that of the deep structure, article and noun in the cohesion determinant phrase marker will always be connected at a relatively low level in the hierarchy. Only at a higher level does the noun phrase as a whole come to be related to the other elements of the sentence. But this means that for every third element x in the sentence the smallest common constituent of article and x is the same as that of noun and x. It follows from the interpretation axiom that with the same degree of cohesion the same relatedness value should be expected for these pairs. For the sentence *the child cried for help*, for example, the theory predicts the following equalities:

$$r(the, cried) = r(child, cried)$$
$$r(the, for) = r(child, for)$$
$$r(the, help) = r(child, help)$$

This holds, no matter what the sentence structure is, provided that the smallest common constituent of *the* and *child* includes no other smallest common constituent. Any theory which allows the contrary is a priori in disagreement with current relatedness data, for the relation between the article and its corresponding noun is always stronger than any other relation in an experimental matrix. But the reader can clearly see that the predicted equalities conflict with intuition; one feels that the relations with the article are systematically weaker than those with the noun, and this is indeed what is regularly found in judgment experiments. For the dozens of sentences with article/noun pairs which we have investigated, we have always found, without exception, that the average strength of the relation between the noun and the other words of the sentence is considerably greater than that between the article and the other words. An example of this is the following. The Dutch sentence *Meester geeft de doos aan Jetty of aan Thea* ('Teacher gives the box to Jetty or to Thea') was presented to eight subjects, who judged the word pair relations on a seven-point scale. The relatedness values (total scores) for *de* 'the' and *doos* 'box' are given in Table 2.4.

TABLE 2.4. Experimental relatedness values for the relations between *de* 'the' and *doos* 'box' on the one hand, and on the other, the remaining words in the sentence *Meester geeft de doos aan Jetty of aan Thea* ('Teacher gives the box to Jetty or to Thea')

	Meester 'Teacher'	*geeft* 'gives'	*aan*$_1$ 'to'	*Jetty* 'Jetty'	*of* 'or'	*aan*$_2$ 'to'	*Thea* 'Thea'
de 'the'	10	11	9	9	9	10	9
doos 'box'	38	45	20	38	9	22	35

r (*de*, *doos*) = 55

The relations with *doos* 'box' are systematically stronger than those with *de* 'the'. Only the minimal relation with *of* 'or' shows the predicted equality. This result is also characteristic for the strength of the effect: the relations with the article are always close to the absolute minimum score (the minimum score is 8 for eight subjects), while those with the noun tend to cluster around the middle of the scale. It is possible to produce systematic deviations from ultrametricity by introducing article/noun constructions into the test sentence. In general, relations with the head of an endocentric construction are systematically stronger than those with the modifiers.

We must add, however, that not all judgment techniques are equally suited for demonstrating this systematic deviation from ultrametric predictions of equality. Martin (1970) investigated (to use our own terminology) the Chomsky normal-form of the pure surface constituent model. Using the Johnson algorithm, he found good hierarchical solutions for two types of sentences, examples of which are *parents were assisting the advanced teenage pupils* and *children who attend regularly appreciate lessons greatly.* The dominant solution for the first sentence type is (in labeled bracketing notation):

$((N_1(Aux\ V))(D(Adj_1(Adj_2\ N_2))))$,

or, for example,

((parents (where assisting)) (the (advanced (teenage pupils)))).

The solution found for the second sentence type is:

$((N_1\ ((Wh\ V_1)\ Adv_1))((V_2\ N_2)\ Adv_2))$,

or, for example,

((children ((who attend) regularly)) ((appreciate lessons) greatly)).

In the first sentence type the main verb is drawn to the subject, as is the case in Uhlenbeck's analysis and in many of our own experiments. This is not the case for the second sentence type. Later we shall see that the degree of relatedness between the main verb and the subject or the object decreases with the length of the subject or the object, and the highest degree of relatedness is obtained by the pronominalization of that subject or object. But we mention Martin's findings here particularly because of the fact that he did not obtain the same great differences between the degrees of relatedness of articles and those of nouns as we regularly found, and which are so much in conflict with the constituent model. The reason for this, however, is quite clear. With minor modifications, Martin used the word-sorting method in his experiments. That method is not suited for demonstrating these systematic inequalities, and that for an obvious reason. As we have seen, the relation between article and noun is among the strongest in the sentence. This is also the case in Martin's experiments: in four out of five cases in the experiment, the subject grouped the article and its corresponding noun together. If there is a third element *X* in weak relation to the article and in strong relation to the noun, it is impossible to distinguish the relation of the article to *X* from that of the noun to *X* when the informant groups the article and the noun together. If *X* is placed in the same stack as the article and the noun, the scores of both *D*, *X* and *N*, *X* (article to *X*, and noun to *X* respectively) are equal to 1, and if *X* is placed in a different stack, both scores are equal to 0. With the word sorting method, then, strongly related words cannot be distinguished with respect to a third element; both relations are always equal. This contributes considerably to the satisfaction of the condition of ultrametric inequality, according to which the two weakest relations among three elements must be equal. In our own experiments with the word sorting method, we also found a high frequency of ultrametricity in the experimental data. The word sorting method camouflages the characteristic deviation peculiar to the constituent model, and therefore should not be used for testing that model. Moreover, Martin does not mention the other source of systematic deviations, the deep structure relations.

We may then conclude that the transformational extension of the constituent model must also be rejected when the interpretation axiom is maintained. The model is not capable of accounting for either the strong relation between the article and the corresponding noun, or the weak relation between the same article and the other words in the sentence. Yet this result is not surprising to the intuition. It shows that the relation between article and noun is asymmetric; the article is dependent on the noun, and the noun is the head of the noun phrase. A phrase structure grammar or constituent model is not suited for the representation of such dependencies (cf. Volume II, section 4.1.). An obvious alternative is to use a dependency grammar as a linguistic theory, and to adapt the formulation of the interpretation axiom accordingly.

2.4.3. *A Dependency Model for Relatedness Judgments*[1]

In the preceding paragraph we found that relatedness judgments are more a reflection of the relations in the deep structure than of those in the surface structure. We suppose in the present paragraph that the dependency model must be a transformational model. Here, too, the theory has two aspects: a linguistic definition and an interpretation axiom. In a dependency grammar the equivalent of cohesion consists of the two notions of dependency and connectedness. We define a dependency function over the nodes of a dependency diagram (for the formal structure of a dependency grammar, see Volume II, Chapter 4, section 4.5.).

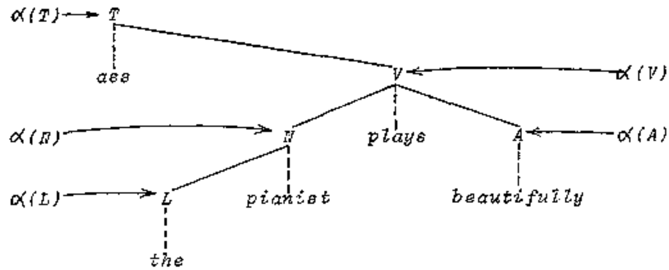
DEFINITION (Dependency). A real-valued DEPENDENCY function α is defined over the nodes of a dependency diagram D, with the property that if $A \rightsquigarrow B$, then $\alpha(A) < \alpha(B)$ for all nodes A, B in D, where $A \rightsquigarrow B$ means that B is directly dependent on A.

The nodes of a dependency diagram thus have values expressed as real numbers; these values increase in all descending paths of the diagram. The head (the start symbol of the grammar) has the smallest degree of dependency.

If we suppose, by convention, that every element in a dependency diagram is dependent on itself, then for every pair of elements there is at least one element on which both are dependent. The FIRST COMMON HEAD *FCH* of two elements in a dependency diagram is the element with the highest dependency value α, on which both elements are dependent. This may be illustrated by the following example.

EXAMPLE 2.2. Figure 2.7. gives a dependency diagram for the underlying structure of the sentence *the pianist plays beautifully*, and an *FCH* table for all pairs of elements in the diagram. N and A, for example, are both dependent on V, but also indirectly on T. The first common head of N and A is the element with the highest

[1] The suggestion of a dependency model as well as other considerations in this paragraph originated in the work of Mr. E. Schils, which will be reported in a separate publication.



FCH	T	D	N	V	A
T	T	T	T	T	T
D	T	L	N	V	V
N	T	N	N	V	V
V	T	V	V	V	V
A	T	V	V	V	A

Fig. 2.7. Hypothetical dependency diagram for the sentence *the pianist plays beautifully,* with degrees of dependency and *FCH* table

dependency value. It follows from the definition of the dependency function that V has a higher dependency value than T, and V is therefore the first common head of N and A. Or consider nodes D and N. They are both dependent on V, but also on N and T. Because $\alpha(N) > \alpha(V) > \alpha(T)$, $FCH_{DN} = N$, as may be seen in the *FCH* table.

We now define the notion of connectedness negatively as follows:

DEFINITION (Disconnectedness). The DEGREE OF DISCONNECTEDNESS of two elements A and B, $\delta(A, B)$ in a dependency diagram is defined as follows: $\delta(A, B) = [\alpha(A) - \alpha(FCH_{AB})] + [\alpha(B) - \alpha(FCH_{AB})] = \alpha(A) + \alpha(B) - 2\alpha\,(FCH_{AB})$.

Two situations can occur here. The first is that in which FCH_{AB} is different from A and B themselves. In Figure 2.7., that is the case for D and A: $FCH_{DA} = V$ and $\delta(D, A) = \alpha(L) - \alpha(V) + \alpha(A) - \alpha(V)$. This is the sum of the two reductions in dependency which occur when we pass from the two elements to V. The other case is that in which one of the elements is the *FCH* of both. This holds, for example, for D and V in Figure 2.7., where V is the first common head of D and V. The disconnectedness is thus $\delta(D, V) = [\alpha(D) - \alpha(V)] + [\alpha(V) - \alpha(V)] = \alpha(D) - \alpha(V)$, which is the difference in the degree of dependency of D and V. In both cases δ is a non-negative real number.[1]

We must now give the interpretation theory which relates experimentally measured degrees of relatedness to this linguistic theory of dependency and connectedness.

Interpretation Axiom. $r_{ij} < r_{kl} \Leftrightarrow \delta_{ij} > \delta_{kl}$, for all words i, j, k, l, in a sentence.

It should be noted that the degree of connectedness of two words is considered to be equal to that of the syntactic category which dominates them directly (it will be remembered that lexical insertion does not take place by dependency rules in a dependency grammar; cf. Volume II, Chapter 4, section 4.5.).

The degree of relatedness of two words is therefore greater to the extent that their connectedness in the dependency diagram is stronger, and vice versa.

Given the interpretation axiom, it is not difficult to reconstruct the dependency diagram which corresponds to a given error-free relatedness matrix R. More precisely, one can reconstruct a connectedness diagram (or GRAPH) which corresponds to the dependency diagram. This calls for some explanation. The interpretation axiom is relatively weak; it does not relate the degrees of relatedness directly to the degrees of dependency in the diagram, but only indirectly, by way of the connectedness of the elements. The axiom says nothing about the direction of the dependency which is at the basis of a given connectedness. Thus we cannot tell from $r(D, N)$ whether $\alpha(D) > \alpha(N)$ or $\alpha(N) > \alpha(D)$, nor do

[1] It is not difficult to show that the degree of disconnectedness δ is a distance metric: (1) $\delta(A, B) = \delta(B, A)$ for every A and B; (2) $\delta(A, A) = 0$ for every A; and (3) $\delta(A, C) \leqslant \delta(A, B) + \delta(B, C)$ for every A, B and C in the dependency diagram (triangular inequality).

we know that D and N are directly connected. We can take either D or N as the head of the dependency relation. Only linguistic considerations can be decisive here, and not the relatedness data. The latter only tell us how the dependency diagram is connected, that is, they show the elements between which relations of direct dependency exist. If we abstract from the transformational character of the model, or limit ourselves to kernel sentences, it holds that for a sentence with n words there are n and only n dependency diagrams which are isomorphous with it as far as connectedness is concerned. Each of the n words can, in effect, be taken as the start symbol in the dependency diagram, and the other dependencies will follow naturally from this. This is illustrated in Figure 2.8.; in it the hypothetical connectedness graph for the sentence *he put his coat on* is given, together with the five dependency diagrams which correspond to it.[1] On close inspection, we see that the interpretation axiom allows us only to find the most accurate connectedness graph for a given relatedness matrix; it does not decide among the various dependency diagrams. But the axiom is nevertheless strong enough, in our opinion: given the root (or start symbol), it determines the form of the dependency diagram. Every linguistic dependency theory will indicate the element which is to be taken as the start symbol. With the interpretation axiom, we suppose that that choice cannot be justified empirically, and this is decidedly a realistic point of view.

In respresenting experimental data, we will take the main verb as the root of dependency diagrams. The graph of Figure 2.8. will thus have the form of diagram (*b*).

How do we find the dependency diagram for an error-free relatedness matrix R? Just as was the case for the Johnson algorithm for hierarchical clustering in the preceding paragraph, we shall refrain in this section from deducing and justifying the dependency algorithm in any detail. We shall illustrate the procedure with an example. Figure 2.9. gives a hypothetical dependency

[1] For the generation of diagrams with split constituent, such as (*c*) and (*d*), more complete dependency rules are necessary than those given in Volume II, Chapter 4, section 4.5.

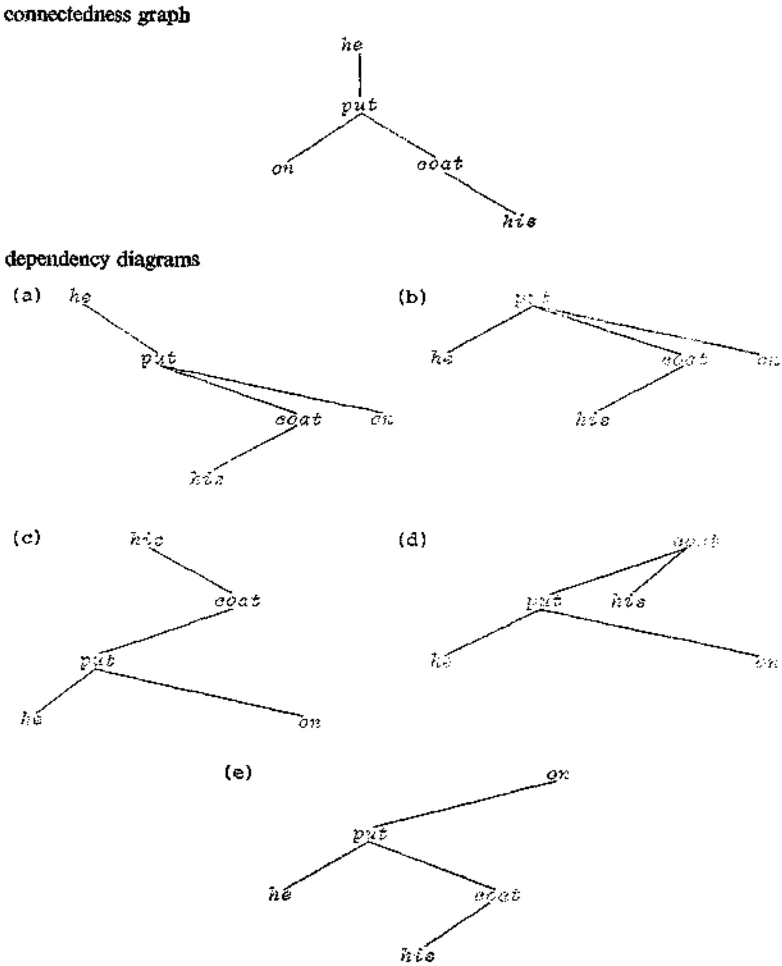
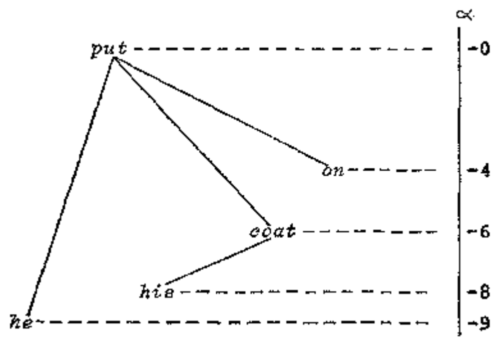


Fig. 2.8. Connectedness graph and corresponding dependency diagrams for the sentence *he put his coat on*

diagram for the sentence *he put his coat on* with the corresponding α-values (*a*) and a δ table with the degrees of disconnectedness for all the word pairs in the sentence (*b*). These degrees of disconnectedness, as the reader can verify, can easily be deduced from the diagram, thanks to the definition of disconnectedness given above. The interpretation axiom states that the rank order of the degrees of relatedness must be the opposite of that of the δ's. The rank numbers for r, from weak (1) to strong (10), are given in a separate table (c) in the figure. Measured r-values will have the

(*a*) Theoretical diagram with dependency values



(*b*) δ-matrix

δ	*he*	*put*	*his*	*coat*	*on*
he	—	9	17	15	13
put		—	8	6	4
his			—	2	12
coat				—	10
on					—

(*c*) Relatedness ranks from weak (1) to strong (10)

r	*he*	*put*	*his*	*coat*	*on*
he	—	6	1	2	3
put		—	7	8	9
his			—	10	4
coat				—	5
on					—

(*d*) Steps in the construction of the graph

rank number	word pair	result
10	*his, coat*	*his—coat*
9	*put, on*	*his—coat* *put—on*
8	*put, coat*	*his—coat* / *put—on*
7	*put, his* (the path is already present)	*his—coat* / *put—on*
6	*he, put*	*his—coat* / *put—on* \\ *he*

(*e*) Dependency diagram

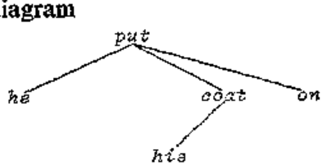


Fig. 2.9. Algorithm for the reconstruction of a dependency diagram from relatedness matrix.

same rank order if the model (disregarding error of measurement) is correct. The problem is now to reconstruct the form of the graph from the experimentally observed order of *r*-values; thence, when the root is selected, we must reconstruct the dependency diagram. The method is as follows:

(a) select the word pair with the highest ranking degree of relatedness;
(b) connect the words, if there is not yet a path between them;
(c) lower the rank by 1, and select the corresponding word pair;
(d) repeat (b) and (c) until the graph is connected, that is, until there is a path between each element and every other element.[1]

[1] It can occur that two or more word pairs are tied for the same rank. In that case, any of the tied pairs may be selected in (a); when (b) has been per-

These steps are shown in Figure 2.9.(*d*), and the resulting dependency diagram, with the main verb as the root, is given in Figure 2.9.(*e*).

Although the construction of the graph in Figure 2.9. is based only on the five strongest relations, the graph is in agreement with the entire matrix. We shall now check this for a few properties of the matrix.

It is not difficult to see that, on the basis of the definitions of dependency and connectedness, the following should be the case: If two elements B and C lie in the path between two other elements A and D, then the connectedness between B and C is greater than that between A and D. By the interpretation axiom, it follows from this that $r(B, C) > r(A, D)$. This holds likewise when the two pairs have one element in common: with a path $A — B — C$ we find $\delta(A, B) < \delta(A, C)$, and therefore $r(A, B) > r(A, C)$. Thus if Figure 2.9.(*c*) correctly represents the relatedness matrix (*c*), then according to the rule mentioned above, the following inequalities hold:

$$r(he, put) > \begin{cases} r(he, his) \\ r(he, coat) \\ r(he, on) \end{cases} \qquad r(put, his) > \begin{cases} r(he, his) \\ r(his, on) \end{cases}$$

$$r(put, coat) > \begin{cases} r(he, coat) \\ r(put, his) \\ r(he, his) \\ r(his, on) \\ r(on, coat) \end{cases} \qquad r(his, coat) > \begin{cases} r(his, put) \\ r(his, he) \\ r(his, on) \end{cases}$$

$$r(put, on) > \begin{cases} r(he, on) \\ r(his, on) \\ r(coat, on) \end{cases}$$

$$r(he, coat) > r(he, his)$$

$$r(coat, on) > r(he, his)$$

Not all of these inequalities are independent; the right hand member of one can be the left hand member of another. In

formed for that word pair, we return to (a) and continue the operation until all the equal ranking pairs are connected. Only when this has been done can we proceed to (c).

fact, only nine of the inequalities in the list are independent; the other nine can be deduced from these. One can easily verify that the matrix in Figure 2.9.(*c*) is correct for all eighteen inequalities predicted in the dependency diagram (*e*).

There are probably more conditions which a relatedness matrix must fulfill in order to correspond to at least one graph or dependency diagram. The fundamental question here, as was the case for the constituent model, concerns the characteristics of the matrix which are necessary and sufficient for the matrix to correspond to at least one graph. But, unlike the case for the hierarchical model, no solution for this has yet been found, as far as we know, though attention has been paid to this question within topology (cf. Goodman 1966).

This problem remains unsolved, and the experimental research on the dependency model is yet in its first stages. Therefore we shall only mention a few preliminary findings which appear to be promising with regard to the model, and a number of points on which problems might be expected.

Within the context of the investigation of another problem, we examined the way in which degrees of relatedness behave under pronominalization (cf. Visser-Bijkerk, unpublished undergraduate thesis, 1969). Every reasonable linguistic theory recognizes that *the boy gave the ice cream to a child* and *he gave the ice cream to a child* have the same structure, with the exception of the substitution of *he* for *the boy*. Likewise, the substitution of *it* for *the ice cream*, or of *him* for *a child*, will also leave the structure unchanged. Three noun phrases can thus be pronominalized in this sentence. Alternate pronominalization of one, two, or all three of those noun phrases will produce seven new sentences, beside the original complete sentence. The eight sentences (including the original) will all have the same structure, with the exception of the pronominalizations. We examined this in the context of the constituent model as well as within that of the dependency model. In the experiment this sentence (in Dutch) was used together with seven others, all with corresponding syntactic structure. The eight sentences were the following:

de jongen gaf het ijsje aan een kind
'the boy gave the ice cream to a child'

de man betaalt het geld aan een agent
'the man pays the money to a policeman'

de miljonair schonk het schilderij aan een pastoor
'the millionaire presented the painting to a priest'

de directeur stuurde het honorarium aan een advocaat
'the director sent the fee to a lawyer'

de meester leende het boek aan een leerling
'the teacher lent the book to a pupil'

de slager overhandigde het vlees aan een klant
'the butcher handed the meat to a customer'

de eigenaar vermaakte het huis aan een invalide
'the owner bequeathed the house to an invalid'

de grossier leverde het hout aan een timmerman
'the wholesaler delivered the wood to a carpenter'

With all the pronominalizations, this gave sixty-four experimental sentences. Each subject was presented with all the forms of pronominalization, and asked to judge them on seven-point scales. Each form was derived from a different sentence content, and the sixty-four sentences were distributed in such a way to eight subjects that each sentence was judged only once. We shall limit our discussion to the results of each form of pronominalization, that is, the totals for the various forms over subject and sentence content; therefore we shall indicate the various words with their category symbols. The sentences on which no pronominalization has been carried out have the form $D_1N_1VD_2N_2$ *to* D_3N_3; those in which the first noun phrase has been pronominalized have the form *he* VD_2N_2 *to* D_3N_3, and so forth. Note that the three articles are all different in Dutch *(de, het, een)*, and thus no confusion was possible.

Analysis showed that the data obtained seriously conflicted with the constituent model. The principal deviation had to do with the predicted equalities for the relations with article and noun. With one exception, the relations with the noun are stronger than those with the corresponding article, quite in agreement with that which was discussed in the preceding paragraph. The single exception is the relation between two articles, which is stronger than that between an article and a noun which does not correspond to it. We shall return to this later. There were also great deviations from the constituent model concerning inequalities. The ultrametricity of the matrices was limited, and alternative phrase markers were always found for the various forms of pronominalization. Only one general tendency could be found in this: when a noun phrase is pronominalized, cohesion with the verb increases. The average degree of relatedness, for example, between N_1 and V (e.g. *boy*, *gave*) is 5.6 (on a scale running from 1 to 7); for *he* and V, it increases to 6.3. For V and N_2 (e.g. *gave*, *ice cream*) we find an average scale value of 4.8, but for V and *it*, the value increases to 5.3. The effect is stronger still for N_3; $r(V, N_3)$ has an average of 3.2, while $r(V, \textit{him})$ has an average scale value of 4.8. It is not surprising, therefore, that in the best fitting phrase marker the main verb is joined to either the subject, the object, or the indirect object, depending on the pronominalization. In other words, one can, at will, elicit the Uhlenbeck or the Chomsky structure (cf. section 2.4.2. of this chapter).

We performed a graph analysis on the same data by means of the algorithm given in Figure 2.9. As the data were not error-free, we were not certain of finding the most suitable dependency diagram. Therefore, for every reconstructed diagram, we examined the degree to which the relatedness data agreed with the inequalities which could be predicted on the basis of that diagram. By making minor variations in the diagrams, we attempted to find better agreement with the data. With a single exception, however, this never led to improved fit. Figure 2.10. shows the most satisfactory solutions, obtained in this way, for the eight forms of pronominalization; the main verbs were used as the roots. Each diagram is accompanied by the percentage of deviations from the inequalities predicted by it.

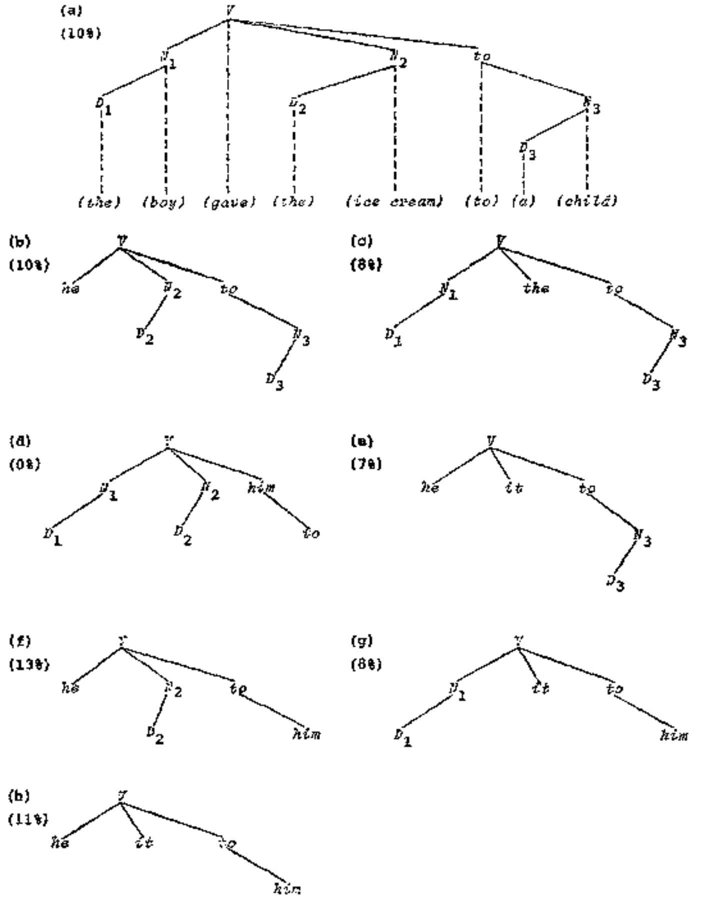


Fig. 2.10. Dependency diagrams for eight forms of pronominalization (percentages of violations are given between parentheses)

The figure shows that the syntactic structure remains constant throughout the eight forms. There is one small exception, however. In (d) *him* and *to* exchange places; otherwise the violations would increase to 9 percent. With this exception, the structures are those which would be expected on linguistic grounds (cf. Volume II, Chapter 4, section 4.5.). The structure, consisting of the verb and its three cases, agentive, objective, and dative, remains constant under pronominalization. If this finding is confirmed by further research, it will be possible to conclude that syntactic relatedness is a concept which can be described in terms of case dependency structure.

The violations reported in Figure 2.10. vary from 0 to 13 percent; these percentages are high enough to consider whether any system can be found in them. It appears that most violations can be explained by a word class effect: degrees of relatedness between words of the same class show a certain inflation. Too strong a relation between artilces, for example, accounts for 25 percent of the violations, and 37 percent of the violations are due to the same effect with nouns or pronouns. Both percentages are too high to be written off to chance. This effect is not syntactic in nature, and no place has yet been made for it in the model.

The experiment reported here by way of example is no proof of the correctness of the dependency model. Further experimentation will certainly lead to modifications and additions. The purpose of this section was to show that to an explicitly formulated grammar an equally explicitly formulated interpretation theory could be added, making it possible to investigate the descriptive adequacy of the linguistic theory. We found that a transformational grammar with a phrase structure grammar as its base is not descriptively adequate in a number of regards, and that a dependency grammar as base avoids many of the difficulties. In both cases, the linguist can set these findings aside by rejecting the interpretation theory. To do so, however, will oblige him to find a better interpretation theory, and it is by no means excluded that this is possible. In that case, the linguist will finally have to attend to a matter which he usually neglects, namely, the theory of the relationship between formal linguistic model and concrete linguistic data.

2.5. CONCEPTUAL FACTORS IN THE JUDGMENT PROCESS

An axiomatic model, however adequate it may be, tells us nothing about the process of judgment itself. It deals only with the output of that process, and not with the way in which that output is produced. This holds for the processes both of judgments on grammaticality and of judgments on syntactic relatedness. It is on introspective grounds alone that we presume conceptual factors of various sorts to play a role in such judgments. The mental representations which we have while the linguistic judgment is formed and the deductions which we make from them are not negligible epiphenomena; they are part of the essence itself of such behavior. Hill (1961) mentions that three of his subjects found the following sentence ungrammatical: *I never heard a green horse smoke a dozen oranges*. But "two changed their votes when it was pointed out that the sentence was strictly true". Conceptual factors are investigated in this framework only in order that they might be eliminated as troublesome variables. No work has yet been done on the problem of how semantic and other conceptual processes in fact determine intuitive linguistic judgments.

NOTE TO SECTION 2.1.

The following table shows the authors' original grammaticality judgments on sentences (1) to (14) and the number of the twenty-four linguists who judged the sentences as ungrammatical.

Sentence	*Author's Judgment*	*Number of Linguists Judging Sentence as Ungrammatical*
(1)	Ungrammatical	9
(2)	Ungrammatical	0
(3)	Ungrammatical	0
(4)	Grammatical	4
(5)	Ungrammatical	7
(6)	Ungrammatical	3
(7)	Grammatical	16
(8)	Grammatical	11
(9)	Ungrammatical	5
(10)	Grammatical	0

(11)	Ungrammatical	1
(12)	Ungrammatical	8
(13)	Grammatical	12
(14)	Ungrammatical	5

The average number of "ungrammatical" judgments was thus 4.2 for the sentences marked ungrammatical by the authors, and 8.6 for those marked grammatical by the authors.

Sentences (17) to (20) were all marked grammatical.

3

GRAMMARS IN MODELS OF THE LANGUAGE USER

The central problem in psycholinguistics is the explanation of primary language usage, that is, speaking and understanding. Language is the means *par excellence* of human communication. By the spoken word (and in a derived form, by the written word), we express our intentions, thoughts, feelings, questions; by it also we are able to decipher the intentions of others. Verbal communication is possible on every subject imaginable. It is a general and considerably flexible medium, supple enough to fit not only the direct topic of the conversation, but also the suppositions shared by speaker and hearer, the special and social relations which exist between them, the perceptions which they share during the conversation, verbal communication which has preceded the conversation, the supposed intentions of the other, and much more still. One can only imagine the number of variables which would have to be taken into account for a complete analysis of the following conversation:

She: Shall I simply go alone to the PTA meeting?
He: You are only reminding me that I dread it.

Contemplation of such examples is enough to drive a psycholinguist to distraction. The flexibility of the use of language forces us to study so many variables that every concrete investigation already suffers from the odium of futility before it even begins. On the other hand, given the importance of the growing insight into human verbal communication, from the points of view of both science and application, the psycholinguist has little alternative but to assume that odium and try little by little to acquire insight into this complex problem.

The only reasonable way of doing this is to isolate a number of variables for close investigation, and to keep all other variables constant. Although reasonable, this approach is not without repercussions for the examination of so complicated a phenomenon. There is the danger that the investigator will lose sight of the whole and will systematically undererstimate or even deny the importance of other variables. The research situation is almost inevitably accompanied by a number of presuppositions which tend to blind one to the limitations of an experiment. In the history of psycholinguistics, unfortunately, this has been more the rule than the exception.

For this reason, even an incomplete general model can act as a corrective to more specific investigation. Not every model, of course, is suited to this purpose. An important requirement for such a model is that it be formulated in the same language as the more specific models on which concrete research is based. Communication must remain possible between general insights and experimental findings. The only general model which could fulfill this requirement at the moment is a computer simulation model which takes the human being as an information-processing system. The main reason for this is the following. In experimental research an effort is made to investigate each procedure in such detail that every step in it can be described explicitly. If this is successful, a Turing machine can be found which can simulate the process (cf. Volume I, Chapter 7); in other words, a computer can imitate the procedure in principle. To know this, one need not go so far as to program a computer for the task, and in fact this is done only rarely. The point is, however, that on the basis of this notion of simulation, and with the theory of Turing machines in the background, the language of artificial intelligence is used more and more for the description of psychological processes. At the moment it is quite common to see cognitive processes represented in the form of flow diagrams, that is, *as if* they were to be programmed. This way of modelling has a respectable theoretical basis: each process which can be formulated explicitly can be represented in this way. The same theoretical basis provides guarantees for the possibility of simulating verbal communication as a whole. Finally, there are universal Turing machines on which every imaginable procedure can be performed. Anything which can be formulated explicitly can therefore be incorporated in principle into such a general model. Here too, it is not necessary to program a computer for such a model, although it would be informative to attempt to do so, as we shall see in the course of this chapter. One can be satisfied to begin with general flow diagrams in which important aspects of the language usage model are represented as empty black boxes; but even in that case the general theory is put in the same language as that used by the experimenter who deals with such matters.

The subject of this chapter is the place of formal grammars in models of the language user. No effort will be made to present a complete survey of the experimental work already done in this field, nor will any effort be made to treat information-producing models exhaustively. We shall only consider the question of how the theory of formal grammars has contributed to both. The nature of that contribution is determined principally by the extent to which formal grammar itself is used as a general model for speaking and understanding. To that extent, psycholinguistic models can be subdivided into ISOMORPHISTIC, SEMI-ISOMORPHISTIC, and NON-ISOMORPHISTIC models.

3.1. ISOMORPHISTIC, SEMI-ISOMORPHISTIC, AND NON-ISOMORPHISTIC MODELS

Opinions differ on the degree of directness with which the grammar is used in speaking and understanding. Some researchers claim that the grammar has a central place in the model of hearer and speaker, while others give it only a peripheral function. The former implicitly or explicitly suppose that the hearer, in understanding a sentence, either literally runs through the list of rules of the grammar by which the sentence is generated, or performs a series of operations, each of which corresponds to a rule in a one-to-one fashion. They thus presuppose an ISOMORPHISM between linguistic rules and psychological operations. An implication of this point of view is that a given partitioning in the linguistic grammar must correspond to a parallel partitioning in the psychological process. As the input and output of every linguistic rule is copied psychologically, this must also hold for groups of rules. If, for example, the formal model is a transformational grammar, the distinction between the base grammar and the transformational component will be reflected in a parallel segmentation of psychological processes; the deep structures would be the output of one process, for example, and the input of another.

Other investigators reject rule-for-rule isomorphism (MICRO-ISOMORPHISM), but maintain the general agreement between the partitioning of the grammar and that of the psychological mechanism. For them, components of the grammar correspond to relatively independent processes in the language user (MACRO-ISOMORPHISM). In their details, these show little structural agreement with the rules of the grammar, but input and output remain linguistically defined entities, such as surface structure and deep structure. This school thus omits isomorphism on the microlevel, but retains it for the major steps. We shall refer to this kind of model with the term SEMI-ISOMORPHISTIC.

Finally, this whole approach may be dropped, and one can attempt to construct a model of the language user which is not patterned after the rules or components of the grammar. We can call such models non-isomorphistic; in them psychological theory is not patterned after the linguistic grammar. The role of grammars in such models is restricted to that of a minor subcomponent, or to the formal representation of nonlinguistic aspects in the model, or to both. The only non-isomorphistic models at present in which formal grammars play such a role are those which were developed within the framework of research on artificial intelligence; they are known as SEMANTIC MODELS. These are usually relatively general models of the sort to which we referred earlier. The term "semantic" might be somewhat confusing here, since to at least one point of view semantics is a subdivision of linguistics, while most of these models are characterized by assumptions which are conceptual rather than linguistic in nature. Apart from their intrinsic significance, these non-isomorphistic theories, by their general character, have cast new light on the relations among linguistics, psycholinguistics, and the theory of formal languages. It is primarily for that reason that we have chosen to discuss them as well in this chapter. They descend, however, from a completely different tradition of research than isomorphistic and semi-isomorphistic models.

Let us first return to isomorphistic and semi-isomorphistic theories. The distinction between these two approaches is a historical one, and it appears in various forms. Isomorphistic theories enjoyed priority in history, almost by necessity. The notion of "a psychology of grammar" came into being precisely when psychologists were once again becoming aware of the importance of linguistic variables in verbal communication. It was the work done in collaboration between Chomsky and Miller around 1960 which caused the rebirth of this interest. Obviously, the first thing to be shown was that linguistic entities such as constituents, transformations, deep structures, etc., play a demonstrable role in the processes of speaking and understanding. This was done by means of what was called the study of the PSYCHOLOGICAL REALITY OF LINGUISTIC CONCEPTS, an understandable, but somewhat misleading term. In effect, it is decidedly not so that without such study the linguistic concepts in question would have no claim to psychological reality. In Volume II, Chapter 1, we showed that the empirical domain of (transformational) linguistics consists precisely of linguistic intuitions. A linguistic concept is psychologically real to the extent that it contributes to the explanation of behavior relative to linguistic judgments, and nothing more is necessary for this. Although the term is misleading, it does indeed have content in that it refers to the question as to whether constructions which are suited to the description of one form of verbal behavior (intuitive judgments) are equally suited to the description of other verbal processes (the comprehension and retention of sentences, etc.). But for this the term STUDIES OF PSYCHOLOGICAL VALIDITY might be more fitting. The fact that originally an affirmative answer was expected to the above question is largely the result of that which we described as the identification of grammar and linguistic competence in Chapter 1 of this volume. Linguistic competence is at the basis of all verbal behavior, and grammar, therefore, is expressed in all verbal processes. Consequently, it should take little effort to prove the psychological reality of grammar on a large scale. Grammatical considerations were in fact so exclusive in this kind of experimental research that in most cases no effort whatsoever was made to produce a general model in which the relationship between grammar and psychological processes is outlined.

Retrospectively, however, we notice that all the models had an implicitly isomorphistic character; the more rules there were in the grammar, the more complicated were the psychological processes. In Matthews' analysis-by-synthesis model (1962), the assumption of isomorphism is made explicitly rather than implicitly, but the model characterizes the period well. It is a model of the hearer in which the grammar is a source from which structural descriptions are generated. The sentence introduced as input is temporarily stored in a memory, and the generator recursively enumerates structural descriptions, the terminal strings of which are compared with the input stored in the memory. When that generated terminal string coincides with the input string stored in the memory (this is established by a "comparator"), the process stops, and the structural description is accepted as the analysis of the sentence introduced as input.

Naturally, the more rules employed in the linguistic description of the sentence, the more time the synthesis process will take. There is thus a close relationship between the grammar and the process of comprehension. From the beginning it was also quite clear that this way of proceeding cannot be done in real time. It was calculated that if the generation of a twenty-word sentence took one second, it would take 10^{42} seconds to find the correct structural description of such a twenty-word sentence introduced as input, a time longer then the history of the earth. Therefore Matthews and others made a number of additions to the model. One of these was the so-called "preliminary analysis"; in it a preprocessor directly produces a structure which corresponds to the sentence in a number of respects. The comparator then shows the differences which remain, and the generator attempts to find a structure more accurate with regard to those aspects of the sentence. Alternation between comparator and generator reduces the differences until they are considered sufficiently small according to some criterion. Also, the preprocessor and the generator are restricted to the generation of sentences which are not greater in length than the input. Unfortunately, however, these and other modifications have not been able to save the model; the only way to do this would be to extend the function of the preprocessor to such a degree that the lion's part of the work would be accomplished by it. In that case, however, we would be dealing with a semi-isomorphistic model in which only the output, and not the mechanism, is linguistically defined. The question as to how the preprocessor really works would then come to the foreground; its answer has never been quite clear in the analysis-by-synthesis model.

As we have stated, also other (implicitly) isomorphistic models have been in the background of studies on the psychological reality of linguistic concepts; in the following we shall mention these briefly where necessary.

It was only after a veritable rage of "reality studies" that psychologists came to realize that there are systematic exceptions to this isomorphism, and that those exceptions indicate that grammar plays a less direct role than was originally thought to be the case. One started studying psychological processes of understanding in their own right, without much recourse to grammar. Only one supposition of the isomorphistic point of view was retained, namely that the output of the hearer model or the input of the speaker model is a linguistically defined object, the deep structure of the sentence. The necessity a priori of building a complete grammar into a model of the language user came to be considered less urgent, and it was possible to make the matter the subject of renewed discussion.

Non-isomorphistic conceptual models lack that supposition as well. Investigators in that camp have initially been indifferent or even hostile to the idea of linguistic parallels.

In the present chapter we shall treat the isomorphistic models first. Because they are directly inspired on formal linguistic theory, they can best be subdivided according to the formal structure of that theory, thus, according to types 3, 2, 1, and 0. Regular models go back, for the most part, to communication theory. Other phrase structure models (types 2 and 1) stem from Chomsky and Miller's formulations (section 3.3.), as do the transformational models (type-0, to be discussed in section 3.4.). The object of semi-ismorphistic models, almost without exception, has been the hearer. Their most characteristic representation is found in the theory of processing strategies and lexical complexity, as developed by Fodor, Garret, and Bever (section 3.5.). Non-isomorphistic conceptual models will be treated in section 3.6., and some general conclusions will be drawn in section 3.7.

3.2. THE LANGUAGE USER AS A FINITE AUTOMATON

It is commonplace that human information processing capacities are limited. If we consider man to be an automaton, he must decidedly be a finite automaton. In theory, then, he should only be able to deal with regular languages (cf. Volume I, Chapters 4 and 5). But we have seen that natural languages are almost certainly of a more complicated sort (cf. Volume II, Chapter 2), and consequently we should conclude that man must necessarily err in the use of his own language. This matter has been sufficiently explained to show us that it is no mysterious paradox. The real questions have to do with the way in which man errs as language user, and whether we can learn something about the nature of his limitations. In terms of automata, we must find out whether we can consider man as a finite automaton limited as such, as a push-down automaton with a limited push-down store, or as a linear-bounded automaton with an upper limit to the input tape, etc. All of these constructions are equivalent to finite automata, but there are great differences in the types of failure which they will show. A limitation on the push-down store is expressed in an upper limit to the number of self-embeddings which can be accepted by the automaton; a limitation of the linear-bounded store places a direct restriction on the size of the sentence. In this paragraph we shall discuss the way in which a finite automaton will err with respect to natural languages, and we shall consider the extent to which the characteristics of that model correspond to those of human linguistic behavior.

The only finite automata which have been examined as models of linguistic behavior are *k*-limited probabilistic automata or higher order Markov sources (cf. Volume II, Chapter 6, section 6.1.). Attempts were made to prove the psychological reality of this model by varying the order (*k*) of the text; the aim of this was to show that as *k* increases (or uncertainty decreases), the text can be more easily processed and memorized. The classical experiment for this was that of Miller and Selfridge (1950). They used zero to seventh order approximations of English, as well as an ordinary text. Some of their examples were given in Volume II, Chapter 6, section 6.1. They calculated the average percentage of words which subjects could reproduce after seeing a series of words once. The series varied in length from ten to fifty words. The results are given in Figure 3.1.; they show that the probability of recall increases to the fourth or fifth order. But from that point no further improvement took place; the level of the ordinary text had been reached. This indicates that a role is played by something other than decreasing uncertainty, and what that precisely is became apparent from later work by Miller (1962). He showed that verbal memory is particularly sensitive to the grouping of words into units of a certain size, and suggested that in the perception of speech that size roughly corresponds to the phrase, i.e. nominal, verbal, and other linguistically defined phrases containing two to six words. If the material cannot be grouped into such linguistic units, it is relatively difficult to memorize; this is the case for the lower order approximations. In higher order approximations, such groups can be formed (cf. Tulving and Patkau 1962), and the capacity of

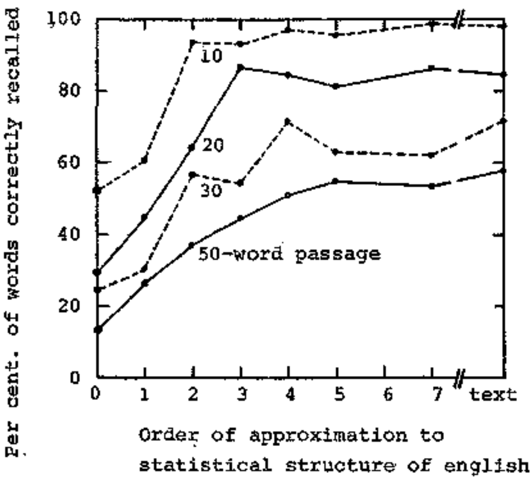


Fig. 3.1. Recall of word strings as a function of order of approximation to English and string length (after Miller & Selfridge).

direct memory is used to a maximum. Meaning-bearing relations between phrases, as in an actual text, do not add much to this. The important variable here is therefore probably the possibility of phrase formation rather than uncertainty. In the following section we shall return to the subject of the phrase as a processing unit.

In any case, the results indicate that man, considered as a Markov source, is no more than 4-limited; limitations of a higher order are not reflected in his performance. For other reasons, however, the 4-limited automaton is an absurd model for the human language user, given considerations of the following kind. Normal speakers and listeners have no difficulty in understanding the sentence *the person, whom you invited recently to come and give a lecture later in the year, seems to be out of town.* If the word *person* in this sentence is changed to *persons*, anyone will immediately realize that *seems* should be changed to *seem*. In general the language user will not err on this dependency, although it spans fifteen words. That is far more than four!

A Markov source with $k = 15$ will contain an enormous number of parameters. Let us calculate that number. We suppose that the source generates word types rather than words (if words themselves are generated, the argument becomes much stronger still), and that there are no more than four word types (an extremely low estimate). The vocabulary thus has four elements, $n = 4$. With the formulas given in Volume II, Chapter 6, section 6.1., we find that the model contains $4^{15} \times (4 - 1)$ parameters, which is more than three billion. A child who wishes to assimilate the language of his environment would thus be obliged to estimate about thirty parameters per second throughout his entire childhood. This is completely unrealistic, especially if we consider that for the estimation of every transition probability at least a few presentations of the word sequence in question are required. A Markov model of the human language user, therefore, does not err in the correct way. With a reasonably limited number of parameters, the model decidedly cannot recognize grammatical dependencies over long sequences, while a human being can do this without difficulty.

Other finite automata have never seriously been studied as models of the language user, but we can expect that examples will always be found in which the model either fails where man does not, or it contains an impossibly high number of parameters.

3.3. NON-REGULAR PHRASE STRUCTURE MODELS

The property of self-embedding (Theorem 2.8. in Volume I) places all non-regular languages outside the reach of finite automata. Only with the addition of an unlimited store can an automaton accept such languages. The reader may also remember that push-down and linear-bounded automata may have a structure analogous to the corresponding grammars; in the proofs of equivalence, each of the transition rules reflects a production of the grammar (cf. the constructions in Volume I, Chapter 5, section 5.2., and Chapter 6, section 6.2.). As a consequence of this, the sentence is accepted in steps which correspond to the structural descriptions in question. In this respect, these are optimal recognition automata, for the history of acceptance precisely reflects the structural description of the sentence.

If these models are selected as models of the human language user with his limited capacity, it will be necessary to limit the store. It follows directly from this that an upper limit will have to be set to the number of times self-embedding can occur in a sentence which is to be processed by the model. If we wish also to maintain the feature according to which the model produces the correct structural description as long as that limit is not attained, the same upper limit will have to be established for all nestings of elements, and not only for self-embedding. A push-down automaton, in particular, retains a nonterminal symbol in the push-down store until all nonterminal symbols in which it was nested have been removed from the store (see Volume I, Chapter 5, section 5.2. for an example of this). A limited push-down automaton, therefore, has an upper limit to the number of nestings, and that limit is the same for self-embedding and other nestings. This also holds, *mutatis mutandis*, for linear-bounded automata.

The question is whether man also has such an upper limit to nesting, and whether that limit is the same for self-embedding and other forms of nesting. The first part of the question can clearly be answered in the affirmative, the second probably in the negative. A whole series of studies (Blumenthal 1966; Fodor and Garrett 1967; Foss and Lynch 1969; Freedle and Craun 1969; Perchonock-Schaefer 1971; Phillips and Miller 1966; Stolz 1967) shows that multiple nesting as well as self-embedding renders a sentence incomprehensible. Chomsky and Miller (1963) give the following example of five-fold nesting:

Anyone who feels that if so many more students whom we haven't actually admitted are sitting in on the course than ones we have that the room had to be changed, then probably auditors will have to be excluded, is likely to agree that the curriculum needs revision.

The sentence is quite incomprehensible on first reading. But it shows no self-embedding. In order to compare self-embedding and other forms of nesting, let us compare the following sentences. Sentences (1) and (2) contain one and two self-embeddings, respectively, and sentences (3) and (4) contain one and two non-self-embedding nestings, respectively.

(1) *if if John comes Peter comes Charles comes*
(2) *the dog that the cat that the mouse killed scratched is large*
(3) *John, who has seen everything, will tell you about it*
(4) *John, who has seen everything you mentioned, will tell you about it*

The last two sentences are strikingly easier to understand than the first two. The human observer can manage two or three nestings, provided that they are not cases of self-embedding. Chomsky and Miller (1963) suggest that the observer not only has a limited memory, but also is subject to the condition that a perceptual operation may not interrupt *itself* more than once (cf. Bever 1970a for further possibilities of explanation).

Whatever the origin of the problem of self-embedding may be, the fact that self-embedding is much more difficult than other forms of nesting indicates that the push-down automaton does not fail in the same way as man.

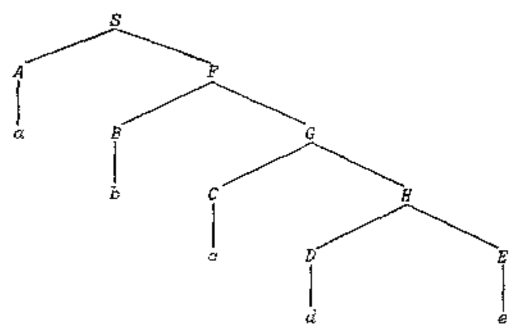
Yet the push-down automaton has sometimes proved to be an apt model for the analysis of some general aspects of speech. An example of this concerns a general characterization of the language of schizophrenics. There is a good deal of literature on the formal aspects of the language of schizophrenics (see, for example, Border 1940 and Ellsworth 1951). In comparison with normal people, schizophrenics use (i) more objects per subject, (ii) fewer qualifications per verb, (iii) fewer different words, (iv) fewer adjectives, (v) fewer adjectives per verb, (vi) shorter sentences, and (vii) more incomplete sentences. These findings are a gold mine for psychiatric interpretation. Each of these various phenomena can be considered as an indicator of certain changes in the emotional and thought structure.

Masters (1970) simulated a push-down automaton on a computer; the automaton was based on a context-free grammar of

English. He had it generate sentences by random procedure, under various limits on the push-down store. With an unlimited store, each output would, of course, be grammatical according to the context-free grammar, but by limiting the store, the automaton produces not only grammatical sentences, but also various partial constructions which cannot be completed because of overflow in the store. Masters varied the number of push-down symbols from two to ten. He showed that above a limit of six, the percentage of incomplete constructions came close to zero (0.8 percent with seven symbols). Apparently then, no more than seven elements need be held in the memory in order to process syntactically nearly all the English sentences produced by this grammar. It is encouraging to note the correspondence of this number with the capacity of man's immediate memory (cf. Miller 1956), though it is difficult to come to judge the quality of Master's grammar. The interesting point is that further reduction of the capacity of the store brings about all the characteristics of the language of schizophrenics mentioned in points (i) to (vii). Although at first sight these characteristics seem very different from each other, apparently they can all be drawn back to one underlying factor, a store of limited capacity. Psychiatrists would do well to investigate whether the immediate memory of schizophrenics is indeed of relatively limited capacity, and if so, what the cause of this can be. It would therefore be pointless to give separate explanations for each of the phenomena mentioned.

Push-down automata and context-free grammars are also the heart of Yngve's model of the speaker (Yngve 1961). The model simply states that the speaker gives a binary leftmost generation of the sentence (cf. Volume I, Chapter 2, section 2.3.4.). Starting with the start symbol *S*, he successively rewrites each leftmost nonterminal symbol in the string. In this way, the various words are derived in the correct syntactic order. Yngve then defines the concept of DEPTH. When a word is derived, it is assigned a number which indicates how many nonterminal elements the string still contains at that moment. The number is equal to that of the push-down symbols (excluding the start symbol) in the store of the corresponding push-down automaton at the moment the word in question is generated. The depth is the number of push-down or nonterminal symbols which remain at that moment. A measure of complexity could be the maximum depth (storage capacity) required by the sentence. Sentence structures which are left-branching have greater depth than those which branch to the right. With binary right-branching there are never more than two nonterminal symbols in the store, while with binary left-branching the maximum is $n - 1$ for a sentence with n words. Compare, for example, the right- and left- branching structures for the sentence *abcde* at the bottom of this page and the top of the next. According to Yngve, right branching structures are less burdensome for the speaker than left branching structures, and consequently the former are predominant in most languages (this also holds for the languages which are usually said to be left-branching languages, such as Turkish; these are really right-branching, but to a lesser degree than other languages). This is called Yngve's DEPTH HYPOTHESIS.

right-branching



Leftmost derivation:

$S \Rightarrow AF \Rightarrow aF \Rightarrow aBG \Rightarrow abG \Rightarrow abCH \Rightarrow$

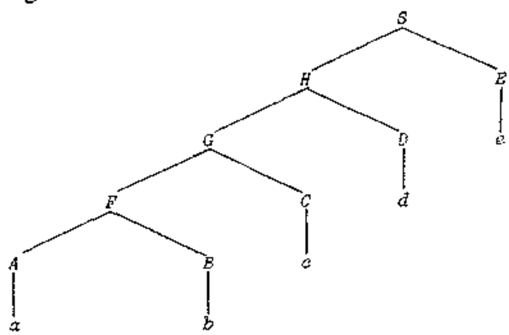
Depth 1 2 1 2

$abcH \Rightarrow abcDE \Rightarrow abcDE \Rightarrow abcde$

1 2 1 0

Maximum Depth: 2

left-branching



Leftmost derivation:

$S \Rightarrow HE \Rightarrow GDE \Rightarrow FCDE \Rightarrow ABCDE \Rightarrow$

$aBCDE \Rightarrow abCDE \Rightarrow abcDE \Rightarrow abcdE \Rightarrow abcde$

Depth: 4 3 2 1 0

Maximum Depth: 4

Miller and Chomsky, on the other hand, state that the memory of the hearer is better served by left-branching constructions (Miller and Chomsky 1963). The hearer must try to process incoming words as rapidly as possible. In other words, he must remove them from his immediate memory by replacing groups of words with a code, by replacing the constituent with the nonterminal symbol. With constructions which are right-branching, this process can begin only when all the words have been received; with left-branching constructions, it can begin at the first word. They conclude that it is unclear why a language should especially serve the interests of the speaker, as Yngve's hypothesis presupposes. However, there is a rather good reason for this: as we shall explain later, the hearer often need not perform any syntactic analysis in order to understand the sentence. The speaker, on the other hand, must cast the sentence in the proper syntactc form. Yngve's hypothesis certainly contains a source of truth.

Beside Yngve's measure of depth, other measures of complexity have been derived from the phrase structure model. The reason for this is that in using an isomorphistic model, one can expect the linguistic complexity of the structure to be expressed in some way in verbal behavior. Rejecting Yngve's measure of depth, Miller and Chomsky (1963) in turn propose a measure which is not dependent on the right-left progression of the structure, but only on the quantity of hierarchy in that structure. The maximum of hierarchy is found in a completely binary tree diagram in which each nonterminal node has two outgoing branches, as in a grammar in Chomsky normal-form (cf. Volume I, Chapter 2, section 2.3.1.). A minimum of hierarchy is found when all terminal elements with multiple branching proceed from a single node. A measure of hierarchy is therefore the node-to-terminal-node ratio or NTN RATIO; this is the number of nodes (including the terminal elements) divided by the number of terminal elements. This ratio would roughly indicate the amount of processing per word necessary to both speaker and hearer. Unlike Yngve's measure of depth, the psychological attractiveness of this ratio has never been proven.

But both these measures have the disadvantage of ignoring completely the lesson of information theory. Given the grammar, which alternative structures are possible and what are their probabilities? A measure of complexity for a phrase structure grammar might more effectively be based on the number of alternative rewrites per nonterminal symbol in the grammar. In the simplest case, one could consider the probabilities of all these alternatives to be equal, and calculate $p(s)$, as given in Volume I, Chapter 3, section 3.4. The measure of complexity would then be the uncertainty, $H = -\log p(s)$. This is in fact the measure of complexity which we used in comparing grammars generated by a grammar-grammar in Volume I, Chapter 8. Further refinements, taking, among other things, conditional probabilities into consideration, then become obvious.

To this point we have discussed explicit type-2 models of the language user (we refer the reader also to Osgood 1963 for a kind of probabilistic context-free model). As far as we know, the value of the linear-bounded automaton as a (type 1-) model has not yet been studied, although it has some rather attractive features, such as a store which, up to a certain limit, adapts to the size of the input sentence.

In the remainder of this section we shall mention work to which the title of "studies on the psychological reality of linguistic concepts" applies more directly. There is a vast literature in which the constituent is taken as a unit of processing in perception, memory, and reproduction of the sentence. We shall not attempt a complete survey of the material here (for further information, see Neisser 1966; Levelt 1966; Fillenbaum 1971; Mehler and de Boysson-Bardies 1971; Loosen 1972). Our aim here is only to offer an idea of the kind of evidence which was sought for the testing of isomorphistic phrase-structure models of the language user.

The most striking feature of all the experiments in question is a lack of solicitude for the psychological details of the processing model. Authors tended to be satisfied with proving the "psychological reality" of a constituent structure, but took little trouble to find the psychological mechanisms which were responsible for this. This is a characteristic of the implicit isomorphistic approach. In Miller and Chomsky (1963) we find at least a rough indication of such a processing model. They consider the psychological reality of constituents to be the consequence of the features of a PRE-PROCESSING MECHANISM. The hearer, in processing a sentence, first tentatively parses the string in smaller and larger phrases on the basis of various indications, such as intonation, function words, articles, and so forth. Thus segmented, the signal is available in immediate memory and will serve in turn as the input of the MAIN PROCESSING which derives the syntactic and semantic relations among the various parts of the sentence. Aside from the fact that the details of the preprocessor are not discussed at all, this distinction between preprocessing and main processing finds little experimental support. Indeed, the whole idea that the psychological reality of linguistic entities such as phrases is caused by features of perceptual processing is used more as a presupposition than as a proposition to be tested experimentally. In many of these "reality studies", the subject is presented a sentence and asked to reproduce it either immediately or after a certain lapse of time, and it thus becomes impossible to distinguish perception from reproduction. Loosen (1972) correctly points out that a so obtained constituent-wise parsing structure could as easily reflect a characteristic of the retrieval process as one of the perceptual mechanism. When the two phases can be distinguished, for example in reaction time experiments in which subjects are not asked to reproduce the sentence, no evidence can be found for a strict temporal separation between preprocessing and main processing. This can be seen, for instance, in the so-called garden path phenomenon, demonstrated by the following example:

The cherry blossoms during summer into full bloom.

We reach a dead end halfway through the sentence by interpreting *blossoms* as a noun, and only at the end can we introduce a correction to this. If the entire sentence were first recorded in the memory and segmented before further interpretation is begun, the correct interpretation would have been given from the very beginning. Such effects can easily be demonstrated experimentally (see the chapter on ambiguity in Flores d'Arcais and Levelt 1970). If there is indeed a preprocessing phase, it will have to do with smaller word groups than the sentence as a whole. The only reasonable interpretation of such a model would then be that although preprocessing and main processing alternate while the sentence is being listened to, they should still be distinguished since syntactic and semantic decisions made in the main processing only concern complete word groups which should therefore have been recognized as such in an earlier preprocessing phase. But on introsprective grounds this is not plausible. It seems for instance possible first to process the relations among the endocenters of phrases, and only later to deal with their internal structure. It is not very likely that the first three words of a sentence such as *Mary watched Trudy who played in the garden* are processed differently than the sentence *Mary watched Trudy*. The verb-to-object relation between *watched* and *Trudy* can be grasped before the relative clause is processed.

We shall now mention two kinds of experiments which are characteristic of the studies of the psychological reality of linguistic concepts; these are the sentence reproduction paradigm and the click experiment. Dozens of other approaches can also be found in the literature on the subject.

We shall begin with an example of the sentence reproduction paradigm. Levelt (1970a) had 120 subjects listen to sentences whose intelligibility had been decreased by the addition of white noise. After each sentence the subject had to write down what he had understood. The following calculation was performed for each sentence. Let i and j stand for two words in a sentence. Of the subjects who had correctly reproduced i we determined the percentage who had also understood j. This percentage was taken as an estimate of the conditional probability that word j can be reproduced if i can be reproduced; the notation for this is $p(j|i)$. If the sentence undergoes a hierarchical analysis, like that of a phrase marker, during the transmission from perception to reproduction, then we can expect the following. Let i, j, and k be three words in the sentence; j and k belong to a phrase to which i does not belong. If the phrase functions as a whole during the transmission (i.e. if it is "psychologically real"), we should expect that $p(j|i) = p(k|i)$. We can develop this reasoning in a way analogous to that in Chapter 2, section 2.4.2., of the present volume, and deduce that a hierarchical organization of the sentence must result in an ULTRAMETRIC matrix of forward[1] transition probabilities. The results of the experiment showed that this ultrametric feature was satisfied strikingly well. Figure 3.2. shows the best fitting binary tree diagrams for two syntactically related sentences in the experiment. They are nearly perfect representations of the observed transition probabilities. The experiment not only showed that a hierarchical analysis takes place, but also that the larger psychological entities generally correspond to linguistic constituents such as *het water onder de brug* 'the water under the bridge', *onder de brug* 'under the bridge', *draait in kolken* 'whirls in eddies', and that syntactically equal sentences elicit the same analysis. The smaller units, however,

[1] Backward conditional probabilities of the type $p(i/j)$ can also be studied. An interesting analysis of this for the present experiment can be found in Loosen (1972).

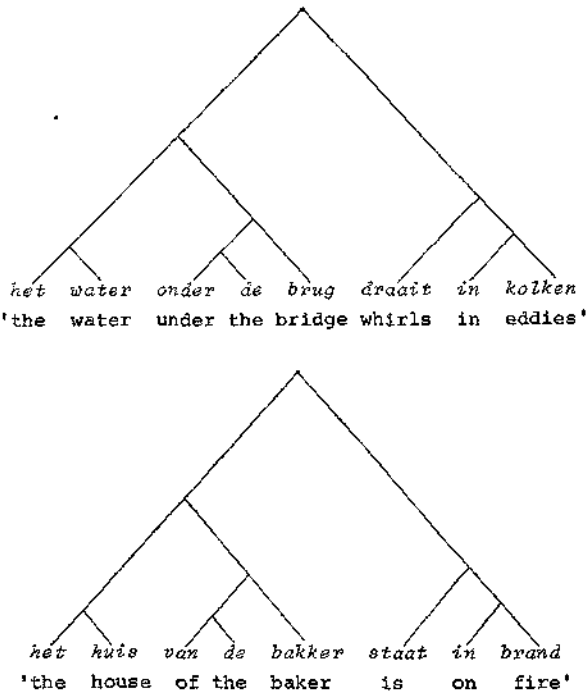


Fig. 3.2. Observed hierarchical analysis for two experimental sentences (after Levelt 1970a)

such as *onder de* 'under the' and *van de* 'of the', do not always agree with the linguistic structure.

It is by no means clear how the hierarchical parsing occurs. We supposed (Levelt 1970a) that it is a feature of the perceptual mechanism. Already in the perceptual phase, the hearer begins to group words into more and more extensive larger phrases. If he is able to understand a given word, the chance increases that he will also understand a later phrase *as a whole*. But nothing in our data would prevent an interpretation in terms of retrieval. The subject was aware that he would be asked to reproduce the sentence. Suppose that he understood a few words (e.g. *is, fire*), but was unsure of a few other words (e.g. *on*). If he constructs a more or less plausible sentence around the words which he has understood, there is a certain chance that he will correctly guess some of the words which he did not understand. The choice is rather simple for a string such as *is? fire*. Loosen (1972) repeated the experiment, but he presented the words in random, rather than syntactic, order. He instructed the subjects before the presentation of the strings of words, to reproduce what they could understand, as far as possible in the form of a sentence. The results of his experiment basically agreed with those of our own. This uncertainty as to the cause of such psychological constituent structures holds for all experiments on sentence reproduction, and in particular for Johnson's (1965) experiment, one of the first on the subject.

The situation is no different for the click experiments. It was at first thought that a method had been found to prove that syntactic parsing of a sentence is operative already during perception. The click procedure is the following. The subject wears earphones, and hears the test sentence in one ear, while at a given moment during the presentation of the sentence, he hears a brief click in the other ear. The subject must later say at which point in the sentence he heard the click. This method was developed for nonsyntactic material by Ladefoged and Broadbent (1960), and was first used by Garrett for the analysis of sentence perception. The characteristic result of such experiments is that the click is not located at the correct place, but rather shifts to the nearest large constituent boundary. The first article in which the click experiment was used as a test of "psychological reality" was titled "The Psychological Reality of Linguistic Segments" (Fodor and Bever 1965). The following example is taken from Garrett, Bever, and Fodor (1966). When the sentence *your hope of marrying Anna was surely impractical* was presented with the click in the middle of the word *Anna*, the subjects described the click as occurring between the words *Anna* and *was*, thus at the major constituent boundary in the sentence. This result was not due to intonation or pause patterns in the sentence. When the word *your* was cut from the sound tape and replaced by *in her*, yielding the sentence *in her hope of marrying Anna was surely impractical*, the subjects located the click between the words *marrying* and *Anna*, once again in agreement with the linguistic segmentation. In the article reporting this experiment (entitled "The Active Use of Grammar in Speech Perception") the authors presupposed that, even without special acoustic indications, the hearer, by making active use of his syntactic knowledge, perceives the sentence in a particular way. Other experiments (Bever, Lackner, and Kirk 1969) and various unpublished material (Fodor, Bever, and Garrett i.p.) show not only that the boundaries between the large constituents attract the click, but also that the underlying structure of the sentence is a determinant for the localization of the click. Thus, for the sentence *they watched the light green car*, subjects showed no tendency to dislocate the click to the boundary between the words *watched* and *the* (a minor constituent boundary), while such a tendency was indeed in evidence for the sentence *they watched the light turn green*, the constituent structure of which is the same at the critical place. The difference is that *the light turn green* is itself an embedded sentence, whereas *the light green car* is not. This seems to indicate that the click experiments confirm the psychological reality of the major and minor constituents, provided that these latter reflect subsentences in the deep structure. Other experiments (Feldmar 1969, Ladefoged 1967, Reber and Anderson 1970, Reber 1973) call the perceptual linguistic interpretation of these findings seriously into question. Although the origin of the click-shift phenomenon has not yet been explained, there is solid experimental ground for the following propositions: (1) if a syntactic factor is operative, it is so by response bias, and not by perceptual mechanisms; (2) the most important perceptual determinant of the click shift is the intonation pattern of the sentence, and not the constituent boundary; (3) nonlinguistic factors play a dominant role in the clickshift. We shall briefly discuss each of these points.

(1) In the original experiments the subjects were asked first to write down the sentence which they heard, and then to indicate the place at which they heard the click. The writing down of the

sentence could give rise to response bias. Feldmar performed the experiment as follows. He presented the sentence twice, each time with the click located at a different place. Two conditions were introduced. In the first of these, the subject was asked to say in which of the two presentations the click occurred earlier; only afterward did he write the sentence down and indicate the places at which he heard the click. In the second condition the subject wrote down the sentence immediately and then marked the positions of the click. The result was that the shift toward major constituent boundaries occurred in the latter case, but not in the former. Obviously, then, the reporting of the sentence is itself a factor which influences subjects in judging the place at which they heard the click. If the subject must first reflect on the position of the click, the syntactic effect is eliminated, or at least decreased.

A more direct and convincing study of response bias was presented by Reber and Anderson; it also led to conclusions, (2) and (3). Their experimental material consisted of strings of six two-syllable words. Strings which were sentences were of the following type: *open roadside markets display tasty products.* The string was presented on one loudspeaker, and the click on another. In principle the click could occur within any syllable but the first and last of the strings, or between any two words. The major constituent boundary lay between the third and fourth words (position 0). After each presentation, subjects were given a printed copy of the sentence and asked to mark the place at which the click occurred. The original click-shift effect was reproduced without difficulty. The condition which was interesting with regard to point (1), however, was the following. A group of subjects was presented with the sentence, but not the click. They were told that there would be a click which they might not notice, but that they would be likely to locate it with more than random probability, as "was the case in other experiments". The subjects showed a strong tendency to locate the fictitious click at position 0, confirming the standard results of experiments of this type. At least in this case, response bias, and not perception, is a sufficient explanation. Response bias could also explain the results obtained with the *Anna* sentence mentioned above, but syntactic factors might have had a perceptual effect as well.

The experiment performed by Reber and Anderson also contained a condition according to which the words of the sentence were not presented in syntactic order, but rather randomly. In that case the subjects made more errors in locating the click, whereas the very factor supposed to be determinant for the clickshift had been eliminated. Yet the tendency to locate the click in the middle of the string (i.e. at position 0) was still observed, although that tendency was somewhat weaker than in the case of the syntactically normal sentences. Although this latter, limited effect might be attributed to a perceptual factor, it would be more natural to consider it as a response bias. Faced with uncertainty, the subject will show a stronger tendency to locate the click at position 0 when given a real sentence than when given a random string of six words.

(2) Another condition of the experiment concerned the comparison of natural and monotonous intonation of the sentence. Of all the stimulus variables employed, this was the most important to the click-shift effect. Not only did the number of errors in the location of the click greatly decrease with monotonous intonation, but the direction bias (i.e. the tendency to move the click to position 0) was also largely eliminated. The characteristic response bias was likewise considerably stronger for sentences presented with normal intonation than for sentences read in monotone in the experiment in which the click was not given, and consequently where only response bias was in question. The most important factor for response bias, thus, is intonation.

(3) The authors repeated the experiment with nonlinguistic stimulus material. The words were replaced by stretches of white noise, represented on the answer sheet as a sequence of six rectangles. Once again the characteristic click-shift toward position 0 was observed. No linguistic factor whatsoever can have come into play here. It was also found in this, as in other experiments, that errors in the location of the click were greater when the click occurred at the beginning of the string than when it occurred at

the end. Two non-linguistic factors may have played a role in this experiment, and consequently in the others. The first of these is immediate memory. As the lapse of time between click and answer decreases, the location becomes more precise. The second factor is less obvious; it has to do with the reason why clicks which occur early in the string are shifted "to the right", and those which occur late in the sentence are shifted "to the left". The authors sought a perceptual explanation. On the basis of Broadbent's single channal hypothesis, they supposed that a subject, once trained, concentrates his attention at first on the string, whether it be a sentence, a series of random words, or of noises. He must then turn his attention to the click when it occurs, and will lose time in doing so, thus slowing down the perception of the click. The situation is reversed when the click occurs near the end of the string. By that time the string has become redundant — certainly if it is well intonated and syntactically correct, but also if it consists of series of noises — and the expectation of the click, which has not yet occurred, increases considerably. Consequently, attention is concentrated on the click-channel, and the perception of the click is somewhat anticipated. It should be noticed, however, that this point can also be explained on the basis of response bias.[1]

The classical click-experiments therefore prove no perceptual constituent effect, and they become considerably less attractive from a psychological point of view when we realize that we are dealing only with response bias. Therefore one could as easily ask the subjects immediately to analyze the sentences by marking off the constituents. This may be seen in experiments in which the hearer reacts during perception. Foss and Lynch (1969), for example, asked subjects to push a reaction key as soon as they heard the letter *b* in the sentence. The result was that reaction times were particularly long near major constituent boundaries (which is quite contrary, moreover, to the idea behind the original click-experiments, according to which such a boundary is a suitable place to change channels and to allow the click to pass). At the

[1] As this translation went to press, a sequel to this experiment was published (Reber 1973), in which the results mentioned above were largely confirmed.

end of a major constituent, there is obviously a greater mental load. Relatively slow reactions are likewise observed after an ambiguous segment, because it is rather difficult at that point to free attention for the extra task. Mental load is also expressed in autonomic reactions: Bever, Lackner, and Kirk (1969) showed that the effect of slight electric shocks on the psychogalvanic reflex varies in function to the moment in the sentence at which such shocks are administered. The effect was more limited when the shock was given near a major constituent boundary. This confirms the impression that mental load at such a moment is relatively great.

In summary we can state that if constituents are indeed a psychological reality, the origin of that reality is unknown. Certainty may be had only as to the effects of intonation pattern and response bias.

3.4. TRANSFORMATIONAL COMPLEXITY AND THE CODING HYPOTHESIS

Thanks to transformational grammar, a principle used in the psychology of language (by Wundt and others) around the turn of the century was reintroduced. The principle is that the superficial word order reflects a more abstract structure of relations and functions: *aussere Sprachform* versus *innere Sprachform.* In the early 1960's the distinction between surface structure and deep structure was the subject of feverish activity in psychological research. Only in retrospect can we see that that work was based on two working hypotheses, which we shall discuss here. These hypotheses are not entirely independent of each other, and both are forms — each in a different sense — of the isomorphism discussed in the first section of this chapter.

(Ia) *The Coding Hypothesis.* The coding hypothesis describes the results of the processing of a sentence by the hearer, or the code in which the sentence is put into memory. This hypothesis has one of two forms, depending on the precise form of the transformational theory used. If the transformations are considered to be paraphrastic, transformations such as the passive or the interrogative must already be marked in the deep structure; the base rules generate such transformation labels as *pass* and *Q*. In this case, the coding hypothesis is as follows: the memory code of a sentence is isomorphous with the deep structure of that sentence. Two further hypotheses are usually directly added to this:

(1b) The transformation labels are retained in the memory without interaction either with the rest of the deep structure (the base relations of subject, object, etc.) or with each other.

(1c) The code for the base relations has priority over transformation labels. The latter are learned with greater difficulty, and are forgotten more easily.

When we refer to the coding hypothesis without further qualification, we mean hypotheses (1a), (1b), and (1c) taken together. If, together with the interpretative semanticists and lexicalists (cf. Volume II, Chapter 3, section 3.4.), or as in the period before the publication of Katz and Postal (1964), we do allow transformations which change meaning, then hypothesis (1a) should be read as follows: the memory code for a sentence is isomorphous with the deep structure plus the list of transformations which are applied in the generation of a sentence. In hypotheses (1b) and (1c), "transformation labels" will then be changed to "transformation-operations". The hypothesis has never been reformulated on the basis of the generative semantic point of view, where the very notion of deep structure is considered to be on rather slippery ground. But the formulations of the coding hypothesis are as numerous as the articles which deal with it. No attempt has ever been made to unify these formulations and make them more precise.

(2) *The Derivational Theory of Complexity (DTC).* According to this hypothesis, the processing of the sentence simulates the transformational derivation of the sentence. The hypothesis is known in several different forms, one of which is the analysis-

by-synthesis model, and another, a model which we shall call the "onion model„. In the analysis-by-synthesis model, the hearer tries to approach the sentence iteratively by attempting various transformational derivations (the generation of the deep structure itself is not considered here) until the difference between the sentence heard and the sentence generated has been eliminated. In the onion model, for every transformation there is a psychological de-transformation. The sentence, as it were, is peeled, transformation by transformation, until the deep structure is accessible. Notice that this theory supposes that the preprocessor has already made the surface structure accessible (we have already expressed our objections to this idea), and that it is possible to reverse the transformational derivation (which, as we have seen in Volume II, Chapter 5, proved not to hold). A point which the two theories have in common is that for every transformation there is a corresponding operation; thus the more complex the transformational structure of the sentence is, the more difficult is the processing. Here we see the isomorphism at its purest, but applied to the microstructure of the transformational derivation rather than to the derivation of constituents, as was the case in the preceding paragraph.

As opposed to the derivational theory of complexity, the coding hypothesis is what we have called isomorphism on the macroscale (in the first paragraph of this chapter). The supposition is that the process by which a string of words is understood runs parallel to the linguistic levels of surface structure interpretation, deep structure interpretation, and semantic interpretation.

Nearly overwhelming evidence was originally presented for both of these hypotheses. But as many of the experiments used have since been completely by-passed, we shall not discuss this evidence in detail here. It seems more efficient to chose a number of characteristic examples, as was done in the preceding paragraph from the best of those studies, and to refer the reader to the bibliography at the end of this volume for further information. It will thus likewise be possible to pay more attention to the arguments which have sounded the death knell for this model.

The coding hypothesis was first studied by Mehler (1963) within the framework of a research program, under the direction of George Miller, in which the use of transformational grammar in psycholinguistics was first introduced. Almost all the studies in the program were characterized by experimental material in which interrogatory, passive, and negative transformations were the principal variables. Thus in Mehler's experiment, sentences of the following eight transformational forms were used:

K (kernel sentence)	*the secretary has typed the paper*
P (passive)	*the paper has been typed by the secretary*
N (negative)	*the secretary has not typed the paper*
Q (question)	*has the secretary typed the paper?*
NQ	*hasn't the secretary typed the paper?*
PN	*the paper hasn't been typed by the secretary*
PQ	*has the paper been typed by the secretary?*
PNQ	*hasn't the paper been typed by the secretary?*

The deep structure of these sentences was considered to consist of the deep structure of the kernel sentence and the additional *P*, *N*, and *Q* labels. Mehler asked his subjects to learn the sentences, and to reproduce them when a prompt word, such as *paper*, was given. It was expected that the sentences would become more difficult to memorize as the number of labels increased. The results confirmed the expectation. Kernel sentences were correctly reproduced in an average of 75 percent of the cases; *N*, *Q*, and *P* sentences in an average of 57 percent of the cases; *NQ*, *NP*, and *PQ* sentences in 47.2 percent of the cases; and *PNQ* sentences in an average of 46 percent of the cases. The most frequent reproduction errors concerned the change of one label (e.g. *K* instead of *P*, or *PN* instead of *PQ*), changes of two or three labels seldom occurred. The distribution of errors corresponded in general to the trinomial distribution expected on the basis of hypothesis (1b). Deviation from the expectation was observed only with regard to *NQ* sentences, which proved to be approximately as easy to learn as *Q* sentences.

Hypothesis (1c) was the object of a study performed by Mehler and Miller (1964). Subjects were given a list of eight sentences to learn. As in the experiment which we have just mentioned, the sentences differed in content, but each of the eight syntactic forms occurred once in the list. When the sentences were learned, the subjects were given a second list of eight sentences to learn in order to cause interference with the first eight sentences. The interference list (*IL*) could differ from the original list (*OL*) in a number of ways. In order to test syntactic interference, the interference list was composed in such a way that the sentences differed in 0, 1, 2, or 3 transformations[1] interference from the original list, although the content was the same. Semantic interference was caused by introducing an interference list of eight sentences of totally different content than the original list, but in which each of the forms, *K*, ..., *PQN*, occurred. As a control, a third group of subjects was given an addition task as interference. All three groups were asked to reproduce the original list. Subjects proved to be very resistant to semantic interference. Syntactic interference, however, was considerable, and the group with the syntactic interference list frequently reproduced sentences in incorrect syntactic forms. It appeared to be possible independently to interfere with a syntactic label in the deep structure. Moreover the experiment gave reason to suppose that the syntactic label was learned only after the rest of the deep structure had been established (hypothesis (1c)).

An experiment dealing with hypothesis (1b) which drew a great deal of attention at the time, was performed by Savin and Perchonock (1965). The writers attempted to measure the memory space taken up by the deep structure. Their method was as follows. The sentence was first presented acoustically, and after five seconds of silence, a string of eight disconnected words was presented. The subjects were asked to reproduce the sentence and the disconnected words as well as they could. The sentence material in the experiment consisted of forty-five kernel sentences, their variants

[1] One of them was a negation transformation, which in those days was treated as a purely syntactic matter.

in the Q, ..., PQN forms, as well as a *Wh* form *(who has typed the paper?)*, and E (emphatic) form *(the secretary did type the paper)*, and an EP form for each of the kernel sentences. When the number of words retained from a passive sentence is subtracted from the number of words retained from the corresponding kernel sentence, the difference, $K - P$, can be considered as the memory space taken up by the transformation label P. This is an operationalization of hypothesis (1b). If those labels are indeed coded independently of each other, the memory load for P can also be estimated on the basis of $E - EP$, $Q - QP$, and $QN - PQN$. The experiment showed that, within the measurement error, the differences were indeed equal. Similar results were obtained for the interrogative, the negative, and the emphatic labels.

Indications of the correctness of hypothesis (1a) may be found in particular in Blumenthal (1967) and Blumenthal and Boakes (1967). They asked subjects to learn a sequence of sentences and then to reproduce them one by one, when a prompt word from the sentence was given (prompted recall). Blumenthal's idea was that under hypothesis (1a) the deep structure of the sentence, not its surface structure, should determine which word would be an effective prompt word. The following two sentences, for example, have the same surface form, but they differ in underlying structure:

(a) *the meat was sold by the pound*
(b) *the meat was sold by the poor*

In sentence (a), *by the pound* modifies *sold*, which is realized in the deep structure as an embedded sentence, approximately as follows: $({}_S$*somebody sold* $({}_S$ *it was by the pound*$)_S$ *the meat*$)_S$. Sentence (b), however, is the passive form of the simple sentence *the poor sold the meat*. Let us examine *pound* and *poor* as prompt words. We can expect that *poor* would be a better instrument for the recall of the sentence in which it occurs than *pound* would be for its respective sentence. *Poor* is an element of the main clause (its subject), while *pound* figures only in the subordinate clause (notice that an additional hypothesis is tacitly given here on the relative importance of the main and subordinate clauses). This expectation was confirmed by the experiment when care was taken that the subjects really understood the sentences (for this, Blumenthal included a paraphrasing task for the subjects). Similar results were obtained with pairs of sentences such as *John is eager to please* and *John is easy to please*. In the first sentence *John* is the subject of the main clause, while in the second, *John* is the object of the subordinate clause in the underlying structure. Indeed, *John* is a more effective prompt word in the first sentence.

The purest test of the derivational theory of complexity may be found in McMahon's dissertation (1963). He asked subjects to push a reaction button as soon as they had judged the correctness of sentences such as the following:

(a) seven (thirteen) precedes thirteen (seven) - *K*
(b) thirteen (seven) is preceded by seven (thirteen) - *P*
(c) thirteen (seven) does not precede seven (thirteen) - *N*
(d) seven (thirteen) is not preceded by thirteen (seven) - *PN*

It appeared that the reaction times increased with the complexity of the transformational structure. Moreover, the reaction time for (d) could be predicted from the reaction times for (a), (b), and (c) from an additive model: $RT(PN) = RT(P) + RT(N) - RT(K)$.

Miller and McKean (1964) reported another much quoted, but at first sight less successful, experiment dealing with hypothesis (2). We refer the reader to Levelt (1966) for a critical analysis of that experiment.

Criticism of this isomorphistic transformational model came slowly but surely in the second half of the sixties. Experimental shortcomings were shown in nearly all the experiments in the series. Foa and Schlesinger (1964) pointed out a number of possible alternative explanations for the results obtained by Mehler. Thus the number of morphemic differences between sentences could determine the nature and number of errors of reproduction. They also indicated methodological errors, such as insufficient control of variables like sentence length and word frequency. For another critique of the experiment, see Howe (1970). Savin's experiments were repeated, but never systematically confirmed.

For critical replications, see Matthews (1968) and Wright (1968). It was Glucksberg and Danks (1969) who proved that, in reality, there was another variable working in this experiment — the time lapse between the presentation and the reproduction of the disconnected words. Reproduction, in effect, begins after the sentence is correctly reported. Depending on the syntax and the length of the sentence, that reporting will take more or less time; the number of words retained is a neat decreasing function of that time lapse. It could be argued that the transformational complexity is nevertheless indirectly expressed by the possibility of reproduction of the sentence. This would be seen in the latency time, that is, the time between the end of the presentation of the sentence and the beginning of the reporting. But these latency times show no simple relation to the transformational complexity of the sentence.

Blumenthal's experiments have likewise often been repeated, with various degrees of success. A problem with all prompted recall experiments is that the two experimental sentences compared are always different. The difference can always be interpreted in a different way than as a difference in deep structure. *Sold* and *poor*, for example, might have a higher degree of association than *sold* and *pound*. Most often totally different sentences are used for the two syntactic conditions, for example, with *children are anxious to play* in the one condition and *Rome is fun to visit* in the other, with *children* and *Rome* as the respective prompt words. The greater effectivity of *children* could then be attributed to a strong association between *children* and *play*. Although nothing in Blumenthal's experiments especially indicates that the main factor is the degree of association, there is nothing to exclude the possibility.

Levelt and Bonarius (1968) eliminated this factor by working with ambiguous Dutch sentences of the type *de studenten zijn te jong om te ontgroenen* which means both 'the students are too young to be initiated' and 'the students are too young to initiate (somebody)'. The two different deep structures were established by presenting the sentences to two different groups of subjects in different unambiguous contexts, so that one group took *the students* as the subject of *initiate*, the other as its object. When this procedure was followed, no difference was found in the effectivity of *the students* as prompt word for the reproduction of the sentence.[1]

McMahon's results were never seriously challenged, but this whole "verification research" has since developed into a separate branch of cognitive psychology (cf. Clark i.p., Wason and Johnson-Laird 1972; Trabasso, Rollins, and Shaughnessy 1971); in it there seems to be little need for an isomorphistic transformational model. Discussion of this field, however, would take us too far from the principal subject of this book.

More interesting than critical refutations are studies in which systematic deviations from hypotheses (1) and (2) are shown. An early example of such a study is by Fillenbaum (1966). He showed that in deviations from the coding hypothesis (1b) there certainly is interaction between the content of the sentence and the transformational labels. He used groups of four sentences, such as the following:

(a) *the fireman is dead*
(b) *the fireman is alive*
(c) *the fireman is not dead*
(d) *the fireman is not alive*

He asked each subject to memorize one of these sentences, and a number of other sentences. Later the subject was asked to recognize which of the four sentences, (a), (b), (c), or (d), he had memorized. The coding hypothesis predicts confusion between (a) and (c), and between (b) and (d). In each of those pairs, both sentences have the same basic relations, and differ from each other only with regard to the transformation label, and according to hypothesis (1c) this is most easily forgotten. Fillenbaum, however, found that such confusion seldom took place, while the sentences with the same meaning — (a) and (d), and (b) and (c) — were often confused. He concluded that what the subject retains is not the deep structure,

[1] Notice, however, that *the students* is in both cases the subject of the underlying main clause *the students are too young*.

but the gist of the sentence. It should be pointed out that Fillenbaum explicitly instructed his subjects to "try to get the gist or sense of each sentence". When the subjects are instructed to memorize and to reproduce the sentences verbatim, the coding hypothesis is strongly confirmed. Thus Clark and Card (1969) found that with sentences of the type *A is (not)* $\begin{matrix}\textit{better}\\ \textit{worse}\end{matrix}$ *than B*, the confusion between *not better* and *better* is greater than that between *not better* and *worse*. Obviously, the response pattern is strongly dependent on the instructions given to the subjects. The coding hypothesis is only confirmed when a particular method of memorization is explicitly or implicitly offered. That this hypothesis has decidedly no general validity is clear from the following experiment, performed by Bransford, Barclay, and Franks (1972), in which the strategy of memorization is oriented toward the construction of a visual representation. Under such circumstances, obviously, the coding hypothesis is not confirmed. The two following sentences were offered to subjects:

(a) *three turtles rested on the floating log and a fish swam beneath it*
(b) *three turtles rested on the floating log and a fish swam beneath them*

The subjects could not tell afterwards which of the sentences they had heard, although the deep structures of the sentences are quite different. The following sentences were also given:

(c) *three turtles rested beside the floating log and a fish swam beneath it*
(d) *three turtles rested beside the floating log and a fish swam beneath them*

In this case memory was perfect. The representation in the memory is obviously not that of the deep structure, but some nonlinguistic representation of the situation described. Only in the case of the second pair of sentences are there two different imaginary situations; with the first pair, the situation is the same for both sentences. Paivio (1971a; 1971b) pointed out that sentences are retained in the form of concrete visuo-spatial images and that this imagination factor is controlled in hardly any of the classical experiments. See Kintsch (1972), however, for a refutation of a too facile spatial imagery interpretation of the verbal memory.

Yet a general procedure is given with such experiments for the refutation of the coding hypothesis. In other words, hypothesis (1) can best explain a syntactic effect in situations in which the gist, the visual imagery, etc., is held constant. The coding hypothesis is therefore condemned to be of relatively minor importance in the theory of language perception where the transmission of the gist or reference of the message is precisely the essential factor.

Hypothesis (2) could be correct, even if the coding hypothesis is incorrect. A sentence can be more difficult to understand if its transformational structure is more complicated, regardless of whether or not the hearer reconstructs the deep structure. A series of experiments by Fodor, Garrett, and Bever (1967; 1968) was directed to the elicitation of systematic deviations from the derivational theory of complexity. The subject heard or read a sentence and was asked to repeat it as soon as possible in his own words. The latency time, the time between presentation and reaction, was recorded and used as a measure of the difficulty of the processing. (Notice that in such tasks, a reproduction factor also plays a role, but that role was not investigated.) Thought experiments alone are enough to show that the derivational theory of complexity must err here, as in fact it did in these experiments. Compare the following sentences:

(a) *the red house is on fire*
(b) *the house which is red, is on fire*

Sentence (b) ought to be easier to process because it has a simpler transformational derivation. In the *Aspects* model, prefixed adjectives like those in (a) are derived from relative clauses like those in (b). Sentence (a) is therefore more complex, contrary to a common sense expectation. The simple addition of an adjective to a noun should lead to important increases in the complexity of the sentence: the *Aspects* model gives no less than three transformations for the generation of a prefixed adjective. In one of their experiments, however, the authors show that the addition of an adjective does not increase complexity. Examples of deletion are still stronger. In the following sentences, transformational complexity increases from sentence (c) to sentence (d), and from sentence (d) to sentence (e).

(c) *John swims faster than Bob swims*
(d) *John swims faster than Bob* (deletion of *swim*)
(e) *John swims faster than Bob does* (insertion of *do*)

But it is clear that sentence (c) is not the easiest to understand; it is, on the contrary, the most difficult. This had already been shown by Fodor, Jenkins, and Saporta (1965). Deletion transformations obviously do serve a purpose.

These and similar examples make isomorphistic transformational theories improbable. The experiments by Fodor et al. were in fact intended to show that although there is some connection between the difficulty of processing and syntactic structure, it is of a completely different nature than the isomorphistic model would suggest. We shall presently go more deeply into the ideas behind these experiments and their conclusions, but we would first point out that these studies are in essence still based on the coding hypothesis. They suppose that at a certain stage the output of the processing of the sentence is its deep structure. Although the two articles of 1967 and 1968 have effectively refuted isomorphism on the microscale, they left isomorphism untouched as far as the major steps in the process of comprehension are concerned. This is what we called semi-isomorphism in the first section of this chapter.

3.5. PERCEPTUAL STRATEGIES

The following quotation from Fodor and Garrett (1967) shows how the derivational theory of complexity was rejected while maintaining the coding hypothesis.

The most profound problem in psycholinguistics is perhaps to specify the nature of the relation between the grammar and the recognition routine. We have seen that the only a priori requirement upon that relation is simply that the recognition routine must recover the structural descriptions output by the grammar.

It is clear from the context that the authors mean "deep structure" by the term "structural description". But no derivational theory of complexity is needed for this. The idea of the authors is that direct conclusions regarding the deep structure configuration can be drawn from certain properties of the surface structure without need of referring to de-transformations or anything else. Such (hypothetical) processes are called FUNCTIONAL RELATION STRATEGIES. We shall mention a few of these later, but it should first be noted that this point of view supposes that those strategies concern a sentence which has already been segmented to a certain degree by a preprocessor, and that that segmentation is more or less in agreement with the surface structure. Fodor, Garrett, and Bever (1968)

have presupposed as input to the sentence recognition process a representation of the sentence which makes at least a crude segmentation, including the identification of the main verb.

In section 3.3. of this chapter, we have seen that there is not much ground for this supposition, and that segmentation can as well be the output of the processing as it can be its input. Nevertheless it is probable that certain elements in the sentence, and above all its prosody, can give strong indications that some words belong together and that others do not; at various stages in the processing, this can be important information. On the whole, however, this does not maintain that segmentation completely precedes further processing. One could easily imagine that some decisions on segmentation are made only after a schema of functional relations has been composed. Be this as it may, decisions on the segmentation of the sentence are made at some time during the processing. The term SEGMENTATION STRATEGIES refers to the way in which the hearer does this, and to the information on the basis of which he does it.

We shall first mention a number of segmentation strategies which have been proposed. We shall limit the discussion, however, to a few comments. A systematic treatment of this field is in an advanced stage of preparation (Fodor, Garrett, and Bever i.p.). But at present only a few suggestive references are available in the literature; their significance cannot be judged without a coherent theory (cf. Bever 1970a; 1970b).

SEGMENTATION STRATEGIES. It is supposed that segmentations are preferably of a form which is tuned to the deep structure; in particular, the sentence would be examined on word groups which correspond to the subsentences in the deep structure. Such strategies may be called MAIN AND SUBORDINATE CLAUSE STRATEGIES. Fodor and Garrett (1967) showed that if the relative pronoun is present, it is an important cue for such strategies. In a series of experiments, they proved that the omission of this pronoun — which is often possible in English — makes the processing of a sentence more difficult. Of the following two sentences, sentence (1) takes more time to paraphrase than sentence (2).

(1) *the man the dog bit died*
(2) *the man whom the dog bit died*

The experiments were set up in such a way that the greater transformational complexity of sentence (1) could not be considered as the cause of the difference. The prosody of the sentence can also be an indication of the place at which a subordinate clause interrupts the main clause. Not only Fodor and Garret have found that the processing of sentences like sentence (1) was considerably facilitated when it was spoken with expressive intonation; others also have proven the role of prosody in the identification of phrases. Levelt, Zwanenburg, and Ouweneel (1970), for example, found that ambiguous French sentences such as *on a tourné ce film intéressant pour les étudiants* (which can mean either 'they showed this film, which is of interest to the students' or 'they showed this interesting film to the students') were understood correctly only when spoken with expressive intonation (as opposed to natural intonation). The speaker, therefore, can if necessary provide the information needed for distinguishing the constituent *intéressant pour les étudiants* as a subordinate clause, and the hearer evidently makes use of that information. Finally, there are various conjunctions which could index syntactic clauses: *but or*, *because*, etc. Little research has yet been done on these.

Smaller phrases should also be recognized as such; this holds in particular for noun phrases. There are indications that noun phrase strategies (NP STRATEGIES) exist. One such strategy for Dutch might function as follows: (i) interpret each occurrence of the following words in the first place as an article (*D*), *de*, *het*, and *een* (these are the three Dutch articles); (ii) check whether *D* is followed by a word which is of category *N*; (iii) interpret the sequence $D + N$ as a noun phrase *NP*. In experiments using sentences like the Dutch sentences (1) and (2) below, we found that, with visual presentation, sentences of type (1) were always rapidly and correctly paraphrased by subjects, while sentences of type (2) yielded longer latency times and more errors (cf. Keers, unpublished undergraduate thesis 1968, and Stehouwer, unpublished undergraduate thesis, 1969).

(1) *het jongetje merkte dat het vlees lekker smaakte*
'the little boy noticed that the meat tasted delicious'

(2) *het jongetje merkte dat het vlees lekker vond*
'the little boy noticed that he found the meat delicious'

This was possible in the experiment since in Dutch both the singular definite neuter article and the singular neuter pronoun are the word *het*. Introspection as well as the errors made showed that the subjects held strongly to the interpretation of *het vlees* as a noun phrase in sentence (2), instead of interpreting *het* as a pronoun, referring to *het jongetje*, followed by a noun. Let us mention in passing that the detection of noun phrases poses a major problem in the artificial (computer) processing of language; we shall return to this subject in the following paragraph. In this connection, however, we would point out that Brandt Corstius (1970) has developed a program which isolates noun phrases in Dutch texts. The program is based on a context-free grammar and nearly infallibly marks every noun phrase which occurs in good Dutch. Although the aim of the program is not to simulate human linguistic perception, some of the errors which it makes are typically human errors. Thus, like our subjects, the program misinterprets sentence (2), and obstinately considers *het vlees* to be a noun phrase. It would not be surprising if other noun phrases not recognized by the program would also lead to difficulties for human beings.

FUNCTIONAL RELATION STRATEGIES. Supposing that the hearer is able to distinguish main clauses from subordinate clauses, as well as main verbs, noun phrases, and other phrases, how does he proceed to determine the semantic relations, also called "functional relations" between words? Many possibilities can be imagined, but only little experimental work has been done on the subject. One of the earliest propositions (Fodor and Garrett 1967; Levelt 1967b) was that the hearer can derive parts of the deep structure configuration from the lexical structure of the verb. Such a strategy is called a LEXICAL STRATEGY. We quote from Levelt (1967b):

There appear to be considerable restrictions on the use of certain words. If one such word occurs, the listener knows at once that the syntactic restrictions in question are realized. If, for example, the word *convince* occurs in a sentence, we know immediately that there must be a *somebody* and a *something* such that *somebody* is convinced of *something*. It is possible that both be explicitly mentioned in the sentence. This, for example, is the case for *John convinces Peter of his error*. The word *convinces* can at once elicit deep structure relations in the hearer, deep structures in which *Peter* and *his error* fit like the keys of a lock. However, it is not necessary that both *somebody* and *something* be explicitly mentioned in the sentence. This is the case for the sentence *convincing is a difficult matter;* yet the word *convincing* here indicates that there is somebody who is to be convinced of *something*. The transformational grammar also indicates these elements in the description of the deep structure of such a sentence. The hearer can interpret the *somebody* and the *something* only on the basis of the context in which the sentence is spoken. The following might be said, for example: *John cannot convince Peter that he is wrong. Convincing is a difficult matter.* Further interpretation of *somebody* and *something* then becomes an easy matter.

Sometimes information also lies in the non-linguistic context. The point here is that certain words directly indicate the existence of certain grammatical relations. The occurrence of such a word in a sentence can be the means for the hearer to decide directly on a particular deep structure.

The earliest experiments on lexical strategies may be found in Fodor, Garrett, and Bever (1968). We shall mention the most characteristic results here. The authors made the non-trivial prediction that of the following two sentences, sentence (1) is more difficult to understand than sentence (2).

(1) *the box the man the child saw carried was empty*
(2) *the box the man the child hit carried was empty*

The sentences differ only in the main verb, *saw* as opposed to *hit*. The prediction is based on the different lexical structure of those verbs. In the lexicon, they have the following subcategorizations:

$$\textit{hit}\ [+\ -\ NP]\ \text{and}\ \textit{see}\ \begin{cases}[+\ -\ NP]\\ [+\ -\ S]\end{cases}$$

Beside its normal noun phrase object, *see* can also have a complement *(I see John walking)*, by which the object of the main clause is itself a clause. *See* can thus occur in more deep structure contexts than *hit*, and it is therefore less informative. When subjects were asked to paraphrase the sentences, the result was that significantly more errors were made with sentence (1) than with sentence (2).[1]

In Volume II, Chapter 4, section 4.5., we gave a dependency representation of case relations. That is also a fitting formalization for the description of lexical strategies. The lexical information for the verb contains the cases with which that verb may be connected. The verb *give* induces the schema (A^*OD) in the hearer, or in other words, the procedure looks in the sentence for an agentive,

[1] Objection could be raised against the use of the word *carried* in the sentences of the experiment. It could too easily be interpreted in the construction with *saw* as a past participle. The same objection could be raised for a verb such as *take*. We would point out, however, that the authors also used sentences other than (1) and (2), and came to the same results.

an objective, and a dative. Parts of this procedure can be performed by testing the sentence for "case-related features". For the agentive, for example, we look for a word with the characteristic [+ animate], etc. We saw in the same paragraph that cases are sometimes marked by prepositions (*by*, *with*, etc.), or by suffixes. Therefore an efficient strategy would be first to test the sentence for such characteristics, and only later to test them for case-related lexical features. No serious experimental work has yet been done, however, on the perceptual importance of prepositions. Inflected languages often carry case information in the suffix structure of nouns. This holds in particular for Finnish. Levelt and Bonarius (1968) studied the effects of that information in an experiment in which sentences were to be reproduced on the basis of case-marked prompt words.

But words other than verbs and prepositions can also carry direct information relative to the underlying relations. Fodor and Garrett showed that the relative pronoun has more than just the segmentation function which we have seen. For example, expressive ("segmenting") prosody was never as effective for the comprehensibility of constructions such as *the man the dog bit died* as the insertion of the relative pronoun: *the man whom the dog bit died*. The relative pronoun contains specific information on the syntactic relations between main and subordinate clauses which could facilitate the processing of the sentence. The sequence NP_1 + *Rel* + NP_2 + V_t can occur in an English surface structure only if NP_2 (in the example, *the dog*) is the subject of the simple transitive verb V_t *(bit)*, the object of which is NP_1 *(the man)*. This information is lost in sentences where *Rel (whom)* is deleted.

If lexical strategies can provide the hearer with hypotheses on the deep structure configuration of the sentence, the task of filling in the various open spaces in that configuration remains. We have already seen that morphemically realized case characteristics (such as prepositions) can play a role in this, but no experimental work has yet been done on the subject. We also pointed out that case-related features, such as [+ animate] for the agentive function, can be used. In a somewhat broader connection the term SEMANTIC STRATEGIES is used to refer to the latter because decisions are made on grounds of the semantic characteristics of words. Finally, functions can be assigned to phrases on grounds of their order in the sentence. This is called a WORD-ORDER STRATEGY. We shall first discuss a number of word-order strategies, and then turn our attention to semantic strategies.

A word-order strategy for which some evidence exists involves interpreting every sequence $NP + (Aux +)\ V + NP$ as agentive — action — object. This, of course, will often be successful, and it seems that we have a natural tendency to do this, as may be seen in the meaningless string *the dur sefted the dat*. The articles are sufficient to make us presume that we are dealing with noun phrases, and the past tense morpheme *ed* leads the observer to the conclusion that he is dealing with a verb. The critical sequence $NP + V + NP$ is thus present. The interpretation is clear, and without any problem we paraphrase the sentence with *the dat is sefted by the dur*. It is even more interesting to examine situations in which the strategy is not applicable or where it would produce an awkward effect. The strategy is not applicable in passive sentences in which the agentive and the objective have exchanged places and the word *by* has been added. The often proven difference in comprehensibility between active and passive sentences could be ascribed to the fact that this strategy cannot be applied to the latter. But it can also account for other phenomena. Of the following two sentences, for example, sentence (1) is easier to understand than sentence (2), as shown in an experiment performed by Mehler and Carey (1968).

(1) *they are fixing benches* (progressive form)
(2) *they are performing monkeys* (participial form)

The strategy works only for the progressive form; in participial constructions it leads to the incorrect conclusion that NP_2 *(monkeys)* is the object of V *(performing)*. The strategy also leads to errors of interpretation with nested constructions. Thus Bever (personal communication) showed that subjects were extremely difficult to convince that the doubly nested construction *the* *editor authors the newspaper hired liked laughed* should not be read as an ungrammatical form beginning with the *NP* + *V* + *NP* construction *the editor authors the newspaper* (for further examples, see Bever 1970a; 1970b).

The input of semantic strategies consists of case-related, and, in general, semantic features. As we have mentioned, active sentences are generally easier to understand than passive sentences. Of the following, sentence (1) is easier than sentence (2).

(1) *the cow followed the horse*
(2) *the horse was followed by the cow*

Slobin (1966) showed that this characteristic difference does not occur in the following sentences (3) and (4).

(3) *the dog ate the cookie*
(4) *the cookie was eaten by the dog*

The explanation for this in terms of case is that for sentences (3) and (4) the agentive related feature [+ animate] is found only in *dog* and not in *cookie*. The agent is thus determined unequivocally. This is not the case for sentences (1) and (2), where both *horse* and *cow* have the feature [+ animate]. There it will be necessary to use a different strategy, such as the *NP* + *V* + *NP* strategy. This fails, however, with sentence (2). Children at an early age give the correct interpretation of sentence (4), while at the same time they do not do so for sentence (2), where either *horse* or *cow* is chosen at random as the agentive (Turner and Rommetveit 1968). Semantic strategies are apparently available earlier than syntactic and word-order strategies.

Semantic factors also play a role in the results obtained by Schlesinger (1966). He proved that of the following two sentences, the doubly nested construction (1) is easier to understand than sentence (2).

(1) *the question the girl the lion bit answered was complex*
(2) *the lion the dog the monkey chased bit died*

The reason for this is that in sentence (1) there are semantic limitations on the case roles of noun phrases. Situations in which girls who bite lions offer answers to questions are rather unlikely, so that the interpretation of the sentence should be straightforward on semantic grounds only. But also we see from this example that there is only a vague distinction between linguistic selection restrictions and that which may be called "knowledge of the world" In fact, that distinction might well be superfluous for psycholinguistic purposes. It is only a small step from the above example to the following two sentences, taken from Garrett (1970).

(3) *the boy chased the dog with a bone*
(4) *the boy chased the dog with a car*

Both sentences are ambiguous, but it is on semantic or, rather, on conceptual grounds that the hearer decides that *bone* in the first sentence belongs with *dog*, and that *car* in the second sentence belongs with *boy*. This touches on the essential question of which knowledge is linguistic and which is not. We shall return to this question in the following section. It should suffice here to point out that ambiguous sentences are particularly appropriate material for the study of the hierarchy of strategies. To this end, sentences could be constructed which have one interpretation in one strategy, and another in another strategy. It would thus be possible, for example, to weigh a semantic strategy against the $NP + V + NP$ strategy on the basis of ambiguous sentences such as *they are lecturing professors,* in which the semantic strategy will yield the participial interpretation *(lecturing* will be taken to modify *professors)*, and the word-order strategy will interpret *are lecturing* as the progressive form. For a rather complete survey of the literature and problems involved in the experimental study of ambiguous sentences, we refer the reader to Flores d'Arcais and Levelt (1970). This concludes our remarks on processing strategies.

Semi-isomorphistic models were given a new source of inspiration in modern work in the field of artificial parsing. We do not refer so much to special purpose programs, such as Brandt Corstius' program for Dutch mentioned above, as to a new style of general parsing programs which have been so successful that it can be said without exaggeration that the problem of automatic syntactic analysis has been solved in principle. The idea behind these programs lies in the work done by Thorne and his collaborators at Edinburgh. We shall briefly return to this in section 3.6.4. of this chapter. The point here is the establishment of the fact that these programs differ from the original programs for transformational analysis (e.g. Zwicky, et al. 1965) precisely in the same way as semi-isomorphistic psycholinguistic models differ from isomorphistic ones. At first attempts were made to reverse the grammar in the computer; transformational derivations were undone step by step, as in the onion model of the hearer mentioned above. Thorne, however, paid less attention to the rules of the grammar than to the structural descriptions generated by them. His aim was to produce both the surface structure and the deep structure (*Aspects* model), by having the program process the sentence in one run from left to right by analogy with the human observer. The result was what is now known as an AUGMENTED TRANSITION NETWORK, a sort of extended finite automaton (cf. section 3.6.4. of this chapter). Although it was constructed exclusively for linguistic purposes, the strong analogy with the hearer which was one of the aims of the development of the program, resulted in its exhibiting a number of human traits which call for some attention in psycholinguistic investigation. The program, for example, makes only a limited use of memory. In particular, for most words in the lexicon, it contains no information on syntactic categories. Only for articles, prepositions, and other grammatical formatives is the category stored. The others are deduced from suffixes, affixes and word order, just as man does with strings like *the dur sefted the dat*. Moreover, surface and deep structures are derived simultaneously. The fact that this is evidently possible refutes the obstinate supposition in psychology that a syntactic preprocessing, the output of which is a rough parsing, is necessary for an adequate perception model. While this program is an inspiration for psycholinguists and much discussion can be heard on the subject, no publications have appeared in which it is investigated for its simulation value, and the same holds for the second generation of programs of this type (cf. section 3.6.4. of this chapter).[1]

3.6. CONCEPTUAL MODELS

In this section we shall consider a number of general models of the language user in which no attempt is made to establish direct relations with the grammar. In other words, the models which we are to discuss have never been based on syntactic suppositions because most non-isomorphistic models proceed from the tradition of artificial intelligence — the study of information processing systems. Let us consider the relation between the theory of artificial intelligence and psychological models of the language user. We are dealing here with a completely different approach, which, on the whole, is not an extension of the models in the preceding paragraphs. The theory of artificial intelligence is a general one. Human in-

[1] As this book was going to press, an article by Kaplan (1972) appeared which contains a study of the psychological importance of augmented transition networks. Kaplan makes it plausible that networks can be made which are equivalent to an (*Aspects* type) transformational grammar, and which in their parsing follow strategies such as those described above. The article gives, in particular, a number of functional relation strategies elaborated in detail. The perceptual complexity of a sentence is determined in the model by the number of transitions which the automaton must make in order to accept the sentence.

To the extent that such a transition network works, we return to a strict isomorphistic model; each psychological understanding operation corresponds to a linguistic transition rule. But the situation is quite different from that given in section 3.4. of this chapter. It is no longer the psychological theory which is adapted to the grammar, but rather the grammar which is written for the representation of psychological processing operations. If such a network at the same time provides all input sentences with their correct grammatical parsing, this new isomorphism is of a more acceptable kind than the naïve isomorphism discussed in section 3.4. of this chapter. Quite correctly, Kaplan is careful concerning the possibility of generalizing this approach.

During the translation of this book another article appeared in which augmented transition networks are used in a strongly psychological way. Simmons and Slocum (1972) present a sentence-generating system in which the nodes represent word meanings and paths represent mainly case relations. The model can certainly be considered as a speaker model.

formation processing is, within the theory, a special case, just as processing by computer or other systems. Language usage can be studied from this general point of view; one can develop language processing systems without posing the question as to the extent to which such systems are adequate for human behavior. It is even argued that this method is the most fruitful: if the general problem is solved, it should not be difficult to decipher the organization of a concrete language processing system such as man. We would like to make three remarks on this.

(1) No convincing demonstration of the fruitfulness of this approach has yet been given. Until now we have only seen that no clear boundary can be drawn between general language processing systems and rough computer models of human linguistic behavior. The latter type of investigation comes under the category of "computer simulation research". It aims at having a computer imitate certain forms of human behavior. The development of a general theory of language processing appears to be so basically dependent on what we know about human linguistic behavior, that, for the present, that theory will not go beyond the study of computer simulation, let alone fruitfully produce feedback for it.

(2) The empirical basis of language processing models, even in their reduced form, is quite limited. No more than incidental verification is available for any of the models which will be mentioned in this paragraph, and even this is sometimes lacking. Likewise, it is mostly unclear what kind of empirical results could *verify a given model, and authors are only rarely explicit on this* point. In the rare cases of experimental testing, additional assumptions, not essential to the model, are nearly always made, and consequently we never know whether it is the model or the assumptions which are being tested.

(3) The fact that man can be described as an information processing system is less enlightening than it seems to be at first sight, despite the commonness of the idea.

In the introduction to this chapter we showed that a general model which is formulated in the language of information processing systems has the advantage of being able to communicate with concrete experimentation, because anything which can be described explicitly can be described as a computer procedure. Everything which can ever be formulated explicitly concerning human linguistic behavior can thus in principle be put into such a model. Whilé we have also pointed out that this proposition has a solid basis in computing theory, we cannot say that it gives reason for optimism. The situation is no different in our opinion from that which inspired mechanismic philosophers at the beginning of the eighteenth century to their irresponsible optimism. They expected that the future of the universe, including the future of the human mind, would, with the development of natural science, swiftly become predictable. That expectation was based, among other things, on Laplace's idea that the future of the universe lay completely contained in the position, velocity, and direction of movement of all parts of the universe at a given moment. But even if that idea would prove to be correct, no such expectation could be based on it, given the impossibility of determining the position, velocity, and direction of movement of all parts of the universe. The idea of man as an information processing system is no different. It would be just as difficult to program the universal Turing machine which simulates human linguistic behavior as it is to write up all *the details* of raising a child to be a language-using adult. Moreover, such simulation could never be the goal of an empirical theory. A theory should not attempt to imitate reality in all its details; it should rather strive for the strongest possible generalizations on reality in the most economical possible way. It is not at all clear, however, how the metaphor of man as an information processing system can lead to strong generalizations concerning human verbal behavior.

Although computers have played an important role in the computer simulation of human linguistic behavior, it is incorrect to consider computer programs as psychological theories, as was often done in the study of simulation. Theoretical principles can also be formulated independently, and much information processing theory which is explicit and can be tested has never been put on the computer. The computer program, moreover, is dependent in many details on the computer employed, the compiler, etc. A psychological theory is at least one step of abstraction away from this. In the necessarily concise discussion in this section, we shall avoid computer jargon as much as possible, and limit ourselves to the treatment of a number of general theoretical principles. By doing so, we hope to put the role of grammar in a model of the language user into a larger context, and thus to relativize it.

3.6.1. *General Organization of the Models*

The models which may be found in literature diverge considerably in organization. Some of them have been developed only for certain aspects of language usage, such as the organization of word memory, while others are more general theories which are less elaborate with regard to various details. These more general theories differ enormously in their internal organization. We shall first mention a number of components which occur in some or in all of the theories, and give a rough indication of their function. Later we shall treat some of those components in greater detail.

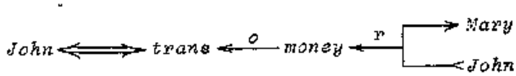
A trait common to all models of this sort is that their basis is a system of concepts, a CONCEPTUAL BASIS. In the ideal case, this basis includes a representation of the inside and outside world, and that representation is intelligent to the extent that it makes various inferences possible concerning that inside and outside world. In its most general form (and, for the moment, its ideal form), the basis should contain knowledge on the effect of own actions, knowledge on the temporal, spatial, and causal characteristics of the physical environment, as well as a model of the conversation partner — assumptions on his knowledge and intentions. In concrete models, no more than minor and relatively arbitrary portions of this have been elaborated. However this may be, the conceptual basis distinguishes these models from all the more or less isomorphistic models treated earlier. The conceptual basis characteristically is not linguistic in nature.

The other components connect the basis to the linguistic input and output. For the analysis of the input, most systems include a smaller or a larger SYNTACTIC ANALYZER. The connection between this analyzer and the conceptual basis generally goes through a SEMANTIC SYSTEM, which at least includes a dictionary in which words are related to their conceptual meanings. The input can also contain nonlinguistic information — perceptions of the internal or external surroundings. This is processed by what we will call the EYE. Finally, the system must be able to respond, either in linguistic or in nonlinguistic form. We shall call these the TEXT-GENERATOR and the HAND, respectively.

3.6.2. *The Conceptual Basis*

The conceptual basis contains the knowledge conveyed by the messages in the language. It is a representation of the internal and the external world. The representation is constructed in such a way that it is possible to draw conclusions concerning this information, to add new information, to answer questions on the information, and so forth. Generally speaking, it is common to all models that at least three units of information can be re-represented in the basis: *objects, relations,* and *properties.* The definitions of these three units of information differ somewhat from one model to another, but for the present we can think of objects (ideas) as everything to which a noun phrase in the language can refer. Relations and properties say something about the objects; they are predicates. Relations are predicates over one or more objects (arguments), and properties are predicates which can have only one argument (one-place relations). Properties are usually expressed in the language as modifiers.

Nearly all the theories allow relations to function as the arguments of other relations; relations (including properties), therefore, can themselves be treated as objects. But it is at this point that the correspondence among the various theories comes to an end. There are great differences in the way in which relations and objects are represented in the various theories, but this is not the place to go into this in detail. For an excellent survey of the subject, see Frijda (1972). In order to show how different these various conceptions can be, we shall only show how the information that John gives money to Mary is represented in the respective theories of Winograd (1972) and Schank (1972). For Winograd, *give* (not the word, but the concept) is considered as a relation (indicated by the symbol ≠) which can have three arguments (each of which is indicated by the symbol :). The situation is thus represented as (≠ *give* :*John* :*Mary* :*money*). This is analogous to the way in which an operator grammar (cf. Volume II, Chapter 4, section 4.3.) writes the base forms of sentences. While Winograd considers the concept *give* as a three-place relation, Schank regards it as an object of a particular kind, an action concept. The action to which the word *give* refers is a concept which Schank indicates with *trans*. *John*, *Mary*, and *money* are also objects, and are called nominal concepts. Schank connects the four objects *(trans, John, Mary* and *money)* by means of three relations which he calls "dependency relations". The first relation connects *John* and *trans*; it is an abstract actor-relation which is indicated by ⇔. The second connects *trans* and *money*; Schank calls this the objective case, *o*, indicated by $\overset{o}{\leftarrow}$. The third relation is the recipient case, *r*, which shows from whom to whom the action *(trans)* is directed (from *John* to *Mary* in the case of our example); it is indicated by the fork shown below, in which we give all the conceptual information according to Schank's theory.



Whereas in Winograd's and other systems everything which is expressed as a verb in the language can have the form of a relation (and consequently the number of relations is in principle great), for Schank the number of possible relations is limited to four abstract case relations (objective, instrumental, recipient, and directive), or five, if the actor relation ⇔ is also counted.[1] In

[1] For some reason which is not clear, Schank does not call this a case relation. The reader should, by the way, bear in mind that these are conceptual, not syntactic, case relations.

some systems, however, the number of possible relations is even further reduced; Kempen (1970) allows only one fundamental relation, the inclusion.

Beside the number of relations and the representations of actions (object versus relation representation), the systems differ in the number of arguments which they allow for the predicate. Winograd allows in principle that any number of objects figure in a relation, while Simmons (1970) and Frijda (1972) do not allow more than two objects for each relation (these objects, however, can in turn also be relations).

The kind of information which we have discussed until now can be called SIMPLE INFORMATION. Another, more complicated kind of information, however, should also be specified in the basis — information with quantifiers like "every" or "some", with the logical connectives such as "implies", "and", "or", "if...then", and with negations. It is impossible to identify such COMPLEX INFORMATION in the simple form of relations between objects. Compare the information which is contained in the following sentence: *If some tables or chairs are outside, not all the furniture is inside.* Representation of this demands a formal language such as predicate logic. It is striking to see how some models completely deny information of this kind, or at least neglect it (cf., for example, Schank 1972).

The representation of knowledge in the basis must be intelligent. New information must be easy to add, questions must be easy to translate into the format of that information, and it must be possible to perform various deductive and, if possible, also inductive procedures. In this regard, Winograd (1972) classifies the models according to five categories:

(1) Specific Systems. These are developed only in order to react intelligently to questions on a specific area of information. Many of the older programs were of this kind. The STUDENT program by Bobrow (1964), for example, was good for the solution of what is called "algebraic word problems". These were analyzed and represented as linear equations. The solution of the problem thus consisted of the solution of a system of linear equations.

Another example is Brandt Corstius' program (1970) which includes quadratic equations and does a similar but better job, particularly in the translation of ordinary Dutch into the representation of the base, the system of linear equations. Such programs, however, from the point of view of simulation, are too one-sided.

(2) Systems with a Text Basis. To a large extent the objects and relations in these systems are words, sometimes with an additional formalization. The information is stored, in slightly edited input and output format, and the intelligence of the system resides in an association network of references. A good example of this is Quillian's (1967) program in which the basis consists of a system of dictionary definitions. Each of the words has a list in which is indicated which other words occur in its definition, with mention of the type of relation. The other words, in turn, are themselves heads of definition lists, and so forth, so that a whole network of definitions is present. One can ask the computer what the relation is between two words, and the answer will be constructed by finding the shortest path through the network from the one word to the other and producing the chain of the words concerned in the output. PROTOSYNTEX I by Simmons, et al. (1962) also falls into this category, as does Kempen's (1970) model. The answers given by such systems are in fact mere portions of the network, and not so much intelligent conclusions drawn from the information stored. This is, of course, not a necessary consequence of the fact of having a text basis. But no attempt has ever been made to develop a strong deductive system for a text basis. In other words, it is not known to what extent knowledge can be stored efficiently (from the point of view of storage, deduction, and retrieval) in terms of words taken from the natural language.

(3) Limited Logic Systems. Relations and objects in these systems are stored in a formal language, and not in the form of an English text, but the representation is limited to what we have above called simple information. This can be called a network of elementary predicates. Although the formal languages used for the various models differ somewhat, they all approach that which

is called FUNCTIONAL NOTATION, i.e. the logical notation for propositional functions with zero or more variables containing no quantifiers. Such systems can only process input of the same sort. Their intelligence resides in the subprogram which mediates between the English language input (and output) and the basis, that is, the semantic component. Some of these programs were effective in the translation of a complex English sentence into a series of predicates in functional notation, which in turn could be compared in that form with the available data in the basis (cf., for example, Simmons 1970).

(4) General Deductive Systems. These were developed for the intelligent storage of complex information. The functional notation is extended to a complete predicate logic, and the information in the basis consists of a set of complex predicates. This way of representing knowledge has great advantages. If a question is asked of the system and the semantic component is able to translate the question into predicate logic notation (i.e. as a theorem to be proven), then the answering of the question consists of finding the proof. The axioms for this are the predicates which are stored in the basis. The attractive point here is that there are uniform proof procedures for first-order predicate logic.[1] This means that there are procedures which guarantee that if a proof of the proposition is at all possible, that proof will be found, regardless of the subject to which the proposition refers (Robinson 1965). Such conceptual bases thus have a considerable degree of generalness. Question-answer procedures are completely independent of the content of the question, and are steered exclusively by the logical notation; information on any arbitrary subject can be stored and used, and all the information relevant to the answering of the question will be found. See, for example, Green and Raphael (1968) for applications.

Though the generality of such a basis is a great improvement over earlier systems, it is also true that, with it, the system easily

[1] First order predicate logic with quantifiers is not decidable as such. However, by the introduction of certain limitations to the order of quantifiers in the formulas, decidability can be attained (cf. Kleene 1967).

becomes impractical and "unpsychological". As the conceptual basis grows, a uniform proof procedure becomes terribly time consuming because there is no means whatsoever to limit the search of the basis in a reasonable way. Human beings, however, are apt to search in the right place. The subject of the question usually limits the searching process much more than is logically justifiable, with the consequence that the answer is given in a relatively short time, and with only little chance that relevant information has been overlooked.

(5) Systems with Procedural Deduction. These are not only suited to storing complex information in logical notation; they also give a considerable degree of structure to the information stored. The structure is established in the form of procedures. Every unit of information is represented in logical notation or an equivalent form, just as in a general deductive system, but at the same time it contains directions on how that information can be used. Suppose, for example, that we ask the system whether the Pope is fallible. The question is first translated by the semantic system into logical notation as a proposition to be proven: *fallible (Pope)*. Let us suppose that the system is not well educated, and that neither this proposition nor its negation are available in its conceptual basis. The program must therefore deduce the proposition from the axioms which are present in the basis. Suppose further that the system does know that everything human is fallible. In the procedural system, such information is established as a procedure which says "*for fallible (X), test human (X)*". PLANNER, a programming language for such procedural propositions, was developed by Hewitt (1969). The system deduces *fallible (Pope)* by giving itself a new goal, namely, to find *human (Pope)*. It searches for this in the set of axioms which are characterized by *Y (Pope)*, and let us suppose that it finds the information *human (Pope)*. It immediately comes to the conclusion *fallible (Pope)*. The form of the procedure for information can be much more complicated than in this example. In particular, a whole hierarchy of tests may be needed. If *human (X)* yields nothing, reference can be made to a secondary goal in the proce-

dure, for example, *living (X)*, etc. In the least favorable of cases, a program like PLANNER can still function as a uniform proof procedure, and ultimately miss no relevant axiom. Winograd's (1972) model has such a procedural conceptual basis. It serves for the representation of a world of blocks, boxes, and other physical objects, which can be observed by an eye and manipulated by a hand.[1] The system understands and produces a written natural language. It is possible to hold a conversation on the block world, and the system can execute all sorts of commands. The following is a specimen of conversation:

Q. Had you touched any pyramid before you put the green one on the little cube?
A. Yes, the green one
Q. When did you pick it up?
A. While I was stacking up the red cube, a large red block and a large green cube
Q. Why?
A. To get rid of it
Q. Why did you do that?
A. To clear off the red cube
Q. Why did you clear off that cube?
A. To put it on a large green cube
Q. Why did you do that?
A. To stack up the red cube, a large red block and a large green cube
Q. Why did you do that?
A. Because you asked me to
etc.

In this model the procedural basis proves to be an extraordinarily fruitful approach. The representation of knowledge is extremely

[1] This hand is not in fact realized, but the output of the program includes the proper instructions to the imaginary hand. The eye is nothing other than the input of the initial coordinates and the names of the objects. A logbook of self-initiated changes is kept. The perceptual system is thus limited, or in fact absent.

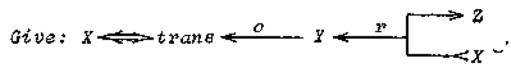
flexible for the purposes. It does not follow, however, that if the base is expanded considerably, the same degree of flexibility and speed will be maintained. Nevertheless this format of representation is decidedly promising. It should be added that objections can be made to the way in which Winograd represents the block world. From a psychological point of view it is naïve to establish the size and place of objects in the form of Euclidian coordinates, as Winograd does. This unpsychological representation of knowledge makes judgment on the simulation capacity of the model rather difficult (as the study was an artificial intelligence study, simulation in the strict sense was not the aim, but on the other hand it is significant that the article was published in a psychological journal).

In more general terms, we should state that at the moment there is no domain of knowledge for which a psychologically justfiable formal representation has been developed. There is great need of a "psychological physics" which would define the naïve knowledge of man in his physical surroundings. Take, for example, the sentence *because it was very slanted, the cat slid off the roof.* There is no linguistic reason whatsoever to take the pronoun *it* by preference as referring to the roof (in fact, there are reasons for the contrary). Nevertheless we understand that it was the roof which was slanted and not the cat, thanks to our naïve physical knowledge. The experiment by Bransford, et al., mentioned earlier (cf. section 3.4.) should also be interpreted in terms of such a representation. More particularly, such a naïve physics should describe our knowledge concerning the location of objects (inside, outside, in front of, behind, on top of, etc.), of causality, substance, permanence, etc., and of functions of objects (to put something on, to be sat upon, to cut with, etc.). The foundations of such a study of naïve physics have been laid in the schools of Piaget and of Michotte, but until now such theories of knowledge have neither been formalized nor incorporated into models of the language user.

Still more deplorable is the lack of naïve theory of motivation. How does man represent his own and others' motivations, intentions, actions? At present there is no psychological predicate logic of motivation (see, however, Nowakowska 1973). As long as these fields have not been developed, models of the language user will continue to show important hiatuses.

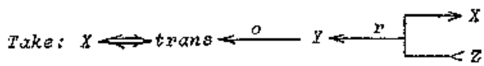
3.6.3. *The Semantic System*

The semantic system mediates between the syntactic analyzer and the conceptual basis. Its purpose is to connect the words in the text correctly to the concepts in the basis. In the first place, a dictionary, or internal lexicon, will be necessary. The dictionary shows how words and concepts are related. The way in which this is done is quite different in each of the models. Schank, for example, defines verbs in terms of the conceptual configurations to which they can refer (possibly more than one for a given verb). Thus we find the following dictionary entry for the verb *give*:



where *X* is *human*
Y is *physical obj.*
Z is *human*

The verb *take* has almost the same dictionary definition; the conceptual action is the same, but the variables have been exchanged:



This shows that it is the conceptual configuration which defines the verb. Schank gives fewer details on the dictionary definitions of nouns, but in any case these definitions contain such characteristics as *human*, *physical obj.*, etc. In Schank's model, part of the process of understanding is a lexical strategy in the sense used by Fodor, et al. (i.p.). A relational structure is derived from the verb, and further information is fitted into that structure. Although we once again have a system of case and case related features, it should be pointed out that these are now conceptual and not linguistic entities. Schank takes care to distinguish them from linguistic cases.

For Winograd, on the other hand, words are defined by procedures. Some words, especially function words, are defined by syntactic procedures. Other words, especially content words, are defined by conceptual procedures. A word such as *powerful* is given in the lexicon as "having the property *power*", where power is the concept in the information basis. Semantic relations with other words pass through the procedures which surround this concept in the base. In Winograd's system, thus, the lexicon carries little information in itself. This contrasts with systems with a text basis, such as Quillian's (1967) model. In these there is no distinction between the conceptual and the semantic systems; instead, there is an associative network of word definitions. A real, although unnecessary disadvantage of such representations is their limited deductive capacity. From a purely semantic point of view, on the other hand, one of their advantages is that they often allow an easy simulation of associative relations between words. The system searches for the shortest connection in the network of semantic definitions, and this is not necessarily the most logical relation between the corresponding concepts. Yet, in understanding language we often make use of such jumps in thought which are directed more by the words than by the concepts. In some models, finally, careful distinction is made between words and concepts, but no deep attention is given to the problem of their interaction. This may be seen in the model developed by Rumelhart, Lindsay, and Norman (1972) which was meant in the first place as a model of verbal memory. It inventively combines features of the Schank and Quillian models, but pays relatively little attention to the problems of input and output. Consequently the semantic component is not worked out in much detail.

Beside indicating the relations between simple words and concepts (or procedures), the semantic system must show how word meanings are connected, given a certain syntactic structure. For this it is necessary that the internal lexicon give the grammatical categories of the words, or that these be derivable in some other way by the syntactic analyzer. On the basis of this, the syntactic analyzer can determine how the meanings of individual words should be connected, and it is this which the semantic system must perform. In Winograd's model, there are separate semantic subprograms for the various phrases: one each for noun phrases (which, in general, correspond to conceptual objects), for prepositional phrases, for adjective phrases, for verb phrases, and for clauses and sentences. The aim of each of these subprograms is the finding of a conceptual representation for the component concerned. For this, it has access both to the syntactic analyzer (if necessary, it can ask for more syntactic details) and to the procedures in the basis. Semantic, syntactic, and conceptual analyses constantly alternate, guided by the priorities in the various procedures. We cannot go more deeply into the details of these programs here.

In Schank's model, this "sentential semantics", as he calls this part of the semantic system, has only a limited role. It combines the words in such a way that corresponding semantic features fit into the spaces of the case network of the verb in question. In doing so, it can use redundancies in the system of semantic features (thus everything with the feature *human* also has the feature *animate*, and so forth). Such redundancies are also used in Winograd's system. When this work of fitting into the conceptual network meets no further conceptual problems (it could, for example, conflict with the foregoing context), the semantic work is completed as far as the sentence structure is concerned.

A final task of the semantic system is interpretation in the light of earlier text or conversation. This has to do, among other things, with the treatment of pronouns and of other words and phrases which can only be interpreted by reference to preceding sentences. Winograd's system keeps a sort of logbook in which the order of conceptual representations is recorded, as obtained in the course of an input text. The order determines the hierarchy of references in the semantic procedures. Many models lack this option, or at least a detailed implementation of it in a computer program.

Before we go on to the discussion of the syntactic analyzer, we must, in this connection, consider the question which was touched upon in the treatment of semantic strategies in paragraph 5 of this chapter: to what extent are selection restrictions linguistic in nature? Are they systematic properties of the combinability of words, or is that possibility of combining words above all determined by nonlinguistic conceptual factors? In conceptual models the latter alternative is always taken. If a conceptual basis is used, the role of semantic and syntactic features appears to be very limited. They serve in the testing of a given conceptualization, but if it does not work, they are ignored. For every selection restriction, one can find exceptions by finding the correct conceptual surroundings. We need no computer simulation to see this. Linguists tell us that the object of *drink* must have the characteristic [+*liquid*]. But a sentence such as *we drank a delicious cup of molten iron* is quite as ungrammatical as *we ate a whole plate of tea.* The fact that these linguistic selection restrictions are satisfied does not guarantee grammaticality any more than ungrammaticality is guaranteed by the fact that they are violated. The ungrammatical sentence *he drank the whole dish of ice cream* (violation of the liquidity restriction) is completely acceptable if it is clear from the context that the ice cream had melted. Should *ice cream* therefore be given two alternative characteristics in the lexicon [+*solid*] and [+*liquid*]? That would mean removing the content of the concept of selection restrictions. The point is, of course, that we are dealing with a conceptualization of *drinkability,* usually absent in molten iron, and sometimes present in ice cream. In neither of the cases is it a systematic characteristic of the word; rather, it is only a semantic characteristic of a conceptual situation. It is better, therefore, to consider selection restrictions as useful linguistic summaries of nonlinguistic conditions. Likewise it is often better to consider subcategorization as a rough conceptual categorization than as a strict linguistic characteristic of words. The distinction, for example, between count and mass nouns *(bicycle* as opposed to *water)* is in fact a conceptual distinction within "naïve physics". It is indeed the case that syntactic characteristics are connected to enumerability, but enumerability itself is not bound to the words themselves, but rather only to the concepts to which those words refer in a given context. If there is great regularity in the reference, one can, as it were, short circuit the theory by treating the subcategorization as a grammatical property. But in this case, a principle called the *principle of diminishing returns* by Lyons (1968) comes into play: each grammatical refinement concerns a smaller group of sentences, and at the limit each new sentence which is not completely justified by the grammar will demand the introduction of an idiosyncratic rule. The reason for this is that selection restrictions and some or all of the forms of subcategorization ultimately go back to properties of concepts of which grammatical categories are only rough summaries. Language is not a closed system.

3.6.4. *The Syntactic Analyzer*

The emphasis on syntactic analysis varies considerably from model to model. Quillian (1967) gives little room to syntax in his model, and hardly any syntactic analysis is performed in the understanding of a sentence. The input words lead directly to the activation of nodes in the basis, and the model finds the shortest path between the words. This is usually sufficient for the construction of an answer: "a generative grammar need not be in any sense a "component" of the underlying language processor" writes Quillian (1968). Only for the formulation of the answer does he consider some grammar necessary. For this, in his opinion, a kind of transformational component must be built in, but there is little or no need of a base grammar. For a model with a basis such as Quillian's (1967), this is a trivial conclusion because the sentences are nearly ready-made available in the basis, and syntactic rules are necessary only for the production of transformational variants of those sentences. Problems do arise, however, in more realistic models with more abstract bases.

Some of these models are almost exclusively oriented to the understanding of sentences. Their production capacity is limited, e.g.

to the giving of yes/no answers, numbers, or simple cliché sentences. This holds for Raphael's (1968) SIR, Bobrow's (1964) STUDENT, Weizenbaum's (1966) ELIZA, and other early programs. All of these models practically did without syntactic analysis. In the circles dealing with simulation at that time, the impression reigned that a syntactic component was more or less superfluous. Since 1969, however, a clear reaction to this can be observed. As the conceptual basis of the models moves farther away from text and less specifically concerns a particular subject (such as linear equations) and at the same time more attention is paid to flexible text production, there appears to be a growing need for a rather extensive syntactic analysis. It is on this point that the new developments in the field of automatic syntactic analysis, which we have already mentioned, is beginning to bear fruit.

Thorne's analysis program has since been followed in the work of Bobrow and Fraser (1969), Sager (1967; 1971 — this is probably a completely independent development), Winograd (1972), and Woods (1970). Woods calls the analysis model an augmented transition network. This is essentially a finite automaton which is augmented in the following sense. State transitions are caused by the input not only of terminal elements, but also of nonterminal elements. To determine whether a given nonterminal symbol is present at a given transition, a subroutine is used, which, again as a finite automaton, determines whether the following portion of text belongs to the category in question. When this subautomaton reaches a final state, it is reported that the original transition can be made. In this form, such an automaton has the capacity of a push-down automaton. All the programs mentioned, however, call for further conditions on the execution of a transition, by which, certainly at least for Woods' and Winograd's programs, the capacity of a Turing machine is obtained. The quasi-finite automaton at the heart of these programs guarantees that the sentence is scanned once from left to right: at each new word there is a limited number of syntactically possible continuations, and the subroutines test which of these continuations is in fact present.

The augmented transition network approach is neutral with respect to the grammar used. Every explicit grammar can be implemented on it. Thorne's original work was based on an *Aspects* grammar. Sager's program, which is the most detailed, proceeded from an adjunct grammar, while Winograd's syntactic analyzer is based on Halliday's systemic grammar. The differences among grammars become considerably less conspicuous when the grammars are written in the form of analysis programs. The origin of Woods' syntax, for example, is difficult to determine. When such a syntactic analysis is integrated into a conceptual model, new problems, of course, occur. It would not be wise, for example, to have the syntactic analysis strictly precede the semantic and conceptual analyses. This integration is executed in detail only in Winograd's model. In that program the syntactic analyzer can call upon semantic and conceptual subroutines at any time in order to solve syntactic problems, or to test analyses on their conceptual tenability. According to Winograd, it is better to consider an augmented transition network more generally as a program which must perform particular subroutines for each transition. The subroutines may be of a syntactic kind, or they may lead to calling upon semantic or conceptual procedures.

3.6.5. *The Text-Generator*

A syntactic component is indispensable to a system which must produce a text. This, however, is the Achilles' heel of most conceptual models, for the text generated (in all cases it is in the form of computer print) is always simple or trite. Of course, fixed expressions and the filling in of standard schemas should play a certain role in every simulation program. *I don't know*, *what do you mean*?, *which X*? are patterns which the human language user also has ready at hand. But more than this is needed. Winograd's program can form not only such fixed expressions, but also new answers to *why*, *when*, and *how* questions concerning the block world and its manipulation. We have already given an example of a conversation thus produced. A *why* question is answered by calling upon the consecutive aims which have led to the answer or the action to which the question refers. We have seen how the program would answer the question *Is the Pope fallible?* If after receiving the affirmative response, we ask *why?*, the intermediate aim *human (Pope)* is called upon, and the answer becomes *because the Pope is human.* For this, of course, the program must be able to translate conceptual information into natural language. In fact Winograd's program can only speak about the block world. It contains more or less ad hoc schemas for nominalization and the formation of sentences in dependency on the question asked. Some style is added to this by the introduction of pronouns where a given object has already been mentioned in the sentence, or by using a number and a plural form when more than one object of the same kind are referred to *(three cubes*, instead of *a cube and a cube and a cube)*. However there is still no general system according to which information in functional logical notation, or an equivalent form of it, can be translated into natural language.

3.6.6. *The "Hand"*

The hand need only be discussed briefly. Motor programs are lacking in all models except that of Winograd, but, as we have seen, even there that component is based on a psychologically naïve representation of a block world. The motor program translates instructions such as *grasp (X)* or *get rid of (X)* into three-dimensional movement instructions. It is only worth mentioning here that also in this case the program can generate subtasks if a particular action cannot be performed. If the instructions are *grasp (X)* while *X* is under *Y*, the subtask *get rid of (Y)* is first generated (compare with the conversation given in section 3.6.4.). Unfortunately studies of the relationships between verbal instructions and motor actions are not only lacking in artificial intelligence work, but also in psycholinguistics this is still a virginal subject.

3.5.7. *The "Eye" and the Theory of Pattern Grammars*

None of the models contains a nonlinguistic perceptual component. As we have already seen, in Winograd's program this is only mentioned *pro forma.* If someone were to manipulate the block world, it would not be noticed. The only input is the original position coordinates of the various objects, and changes executed by the model itself are recorded in a logbook. It is a bit like the eye of a blind man who walks on familiar ground. Naturally it is necessary that nonlinguistic visual input should lead to modifications in the representation of the surroundings in the conceptual basis. The problem of how such information can be deduced has not yet been studied in natural language programs.

It may be expected, however, that in the near future attempts will be made to integrate the rapidly developing theory of pattern grammars, and more generally, work in the field of scene analysis, into models of the language user. What is a pattern grammar? Just as the linguistic grammars treated in this book generate sentences, pattern grammars generate n-dimensional patterns. The simplest case is a one-dimensional pattern with black and white squares as its elements. The hierarchy of formal grammars for such patterns is precisely the Chomsky hierarchy discussed in Volume I. The terminal vocabulary consists of two elements, a and b, where a is interpreted as "black square", and b as "white square". If other shades or colors are admitted, these will correspond to further elements in the terminal vocabulary. The grammars describe how the black and white (or other) squares can be placed in sequence. Wagenaar (1972) discusses regular patterns of this kind, and a number of their psychological properties. Just as with sentences generated by a linguistic grammar, the pattern grammar assigns a structural description to each pattern.

The pattern can also be expanded to two dimensions, that is, we can give rules for horizontal and vertical arrangement of the elements. The grammar will then generate two-dimensional patterns.

Seen from the point of view of patterns, pattern grammars can best be subdivided according to the following three characteristics: (1) the definition of the terminal elements, (2) the number of dimensions, and (3) the place in the Chomsky hierarchy (if there is one for the given class of patterns).

In the just given example, the terminal elements were squares. Grammars with such elements are called ARRAY GRAMMARS or MATRIX GRAMMARS. The production rules are rewrites of arrays, configurations of black, white, or other colored areas. There is a Chomsky hierarchy (regular/context-free/context-sensitive) for two-dimensional matrix grammars, cf. Chang 1971).

It is also possible to take any other form, in *n* dimensions, as the terminal elements; these can be lines, ovals, angles, or anything else. In order to give the rules of arrangement for such elements, one can define points of connection in each element. There may be two points (Shaw 1969), or an indefinite number of points (Feder 1971). Such grammars are called PLEX GRAMMARS, these also define a Chomsky hierarchy of patterns.

The notion of "point of connection" can also be generalized. Each terminal element can be provided with a system of coordinates. In the rewrite rules, one can indicate at which points a terminal element is connected to other elements. Such grammars are called COORDINATE GRAMMARS (Anderson 1968). As a consequence of the properties of continuity of these grammars, they do not define a Chomsky hierarchy.

Finally, there are pattern grammars in which the terminal elements are arbitrary labeled graphs. These are networks of nodes and the connections among the nodes. Nodes and connections can differ from each other in various respects. The nature of each is indicated by a label. Such a graph may be called a "web", and the corresponding grammars may be called WEB GRAMMARS (Rosenfield and Pfaltz 1969). Plex grammars are particular cases of web grammars. There is a Chomsky hierarchy for two-dimensional web grammars.

If a Chomsky hierarchy exists, one can also expect that recognition automata can be constructed. In other words, it should be possible to construct systems which can accept given classes of patterns, and to reconstruct the corresponding structural descriptions while doing so.

How can pattern grammars be used in a model of the language user? This question takes us to the more general problem of scene analysis. Scene analysis deals with the inferences which can be made from a two-dimensional picture as to how a three-dimensional layout or scene may be. In general it is not possible to map pictures and scenes in a one-to-one fashion. On the one hand, not all pictures can be mapped on a scene (under reasonable restrictions). Examples are the Devil's fork, Penrose's figure, and some of the famous Escher graphics. On the other hand, most pictures correspond to an infinity of possible solid objects. Numerous examples of this have been given by Ames. If the domain of three-dimensional scenes is well-defined and, by experience or otherwise, sufficiently restricted for the language user, he might be able to make intelligent and quick inference from the pictures to plausible scenes. Picture grammars may enter the analysis of such processes by specifying the well-formedness conditions on pictures, i.e. by generating the language of possible two-dimensional pictures, while providing them with structural descriptions which are three-dimensional representations. As in natural language understanding programs, such grammars might be helpful in formulating the parsing heuristics, although, once again, one should not expect such parsing programs to be grammars-in-reverse.

In fact, as in Winograd's language parsing program, one should expect intelligent parsing of pictures to be strongly semantic. Possible three-dimensional representations should agree with available conceptual information. If the subject expects that the object is a table, he might try a representation where there is a flat horizontal surface. If this is successful, the conceptual base might then decide that the object is fit to support something, to work at, etc., because such functional properties are part of the definition of a table. For an excellent review of intelligent picture processing, see Sutherland (1973).

Dynamic structural descriptions likewise must undergo semantic interpretation. The work performed by Michotte (1964) in this field has been exemplary. It is based on certain sensory movement relations between objects and shows how these are interpreted perceptually. Under certain circumstances a characteristic structural description comes into being; Michotte calls this an AMPLIATION. This is then conceptually represented as a form of causality. Minor sensory variations can influence the perceptual structural description considerably. Thus we obtain the class of perceptions which we indicate with descriptions such as "object *A* pushes object *B*", "*A* launches *B*", "*A* releases *B*", "*A* obstructs *B*", and so forth. All of these words express conceptualizations, in which the property "causal" is an integral part of the relation between *A* and *B*. They differ in additional characteristics.

Of course the "eye" is intended to do much more than interpret visual patterns. We have used the word for all forms of sensory input and introspection. In particular, the perceptions of the intentions of oneself and of another fall into this category. This field of knowledge, however, is a blank page from a formal point of view, and even a vague prediction of future developments would lack any foundation in reality.

3.7. GRAMMARS AND MODELS OF THE LANGUAGE USER

Let us return to the principal theme of this chapter, the question as to the relation between grammars and models of the language user. This question has two sides. The first of these is the question concerning the place of a linguistic grammar in the theory of the language user. The second is the more general question of the applicability of the theory of formal grammars to models of the language user.

Let us first consider the linguistic aspect. In the beginning of the present volume, we discussed Chomsky's conception of the coincidence of linguistic grammar and the creative linguistic capacity of the language user. For those who hold that point of view, the linguistic grammar is necessarily an integral part of a model of the language user. We showed that the empirical basis on which linguistics rests makes it highly improbable that this conception is correct.

The original form of the study of the psychological reality of grammars, as we have seen, was based on the assumption of isomorphism between linguistic rules and psychological operations. This isomorphism is not a logical conclusion from Chomsky's proposition, although Chomsky highly encouraged the development of such an isomorphistic psycholinguistics. This line of research clearly showed that if the grammar should be part of the model of the language user, this will certainly not take on the form of isomorphism. Isomorphistic models, regular as well as context-free ones, proved to be useful only for the analysis of rough statistical aspects of human linguistic behavior. Moreover cases constantly occurred in which the process of understanding was obviously directed by nonlinguistic factors, such as visual representations and knowledge of the situation.

This brought us into contact with a completely different tradition in which the basic assumption had always been conceptual: the study of artificial intelligence and computer simulation. In spite of the hardly surprising fact that at the present stage of development no model in that tradition simulates the human language user very well, the models cast a new light on the question as to the place of the grammar in a model of the language user. In fact the answer to the question is rather independent of the actual success of the simulation. That success is namely highly dependent on the degree to which the various components of the model are elaborated in detail, while the question is mainly concerned with the nature of the relation between those components. It was by way of the general approach from the point of view of information processing systems that the linguistically inspired approach was brought back into perspective. Models in this line are all characterized by their conceptual intelligent basis. The messages in the language concern a nonlinguistic system of concepts. As soon as one introduces such a conceptual system, however (and no reasonable psychological objection can be brought against doing so), the role of the grammar, semantics included, becomes less extensive than was suggested in the linguistic tradition. In the first place the creative linguistic capacity of the language user is shifted in an obvious way to the conceptual basis. It is there that the information is combined in order to make various inferences possible, and it is there that new concepts come into being. Only in the second place can this newly formed information be expressed linguistically. The creative aspect of human language is largely accounted for in these models by a nonlinguistic subsystem.

To what extent, then, is the grammar still a necessary component? There are as many opinions on this point as there are models. At first there was a tendency to minimalize the role of grammar, but we could ascribe that either to the text basis which was sometimes chosen, or to the limited conceptual domain of some models. In the newer models, the need for syntactic and semantic analyses increases with the level of abstraction and the size of the conceptual systems. When the basis becomes less linguistic, the road from linguistic input to conceptual representation grows longer. Winograd's model includes extensive syntactic and semantic analysis programs, and it appears that these will have to be expanded even further if the conceptual basis is to be given a more realistic capacity. Moreover it is not known how much more syntax will be needed in order to provide the output of the model with any more than extremely meager possibilities. One can therefore expect that grammars will indeed go on being an important part of such models. Four qualifications should, however, be added to this.

(1) Grammars in such models are not isomorphistic with linguistic grammars. No one-to-one relation with the rules of the grammar may be seen in the parts of either the analysis program or the synthesis program.

(2) The model grammars are also not equivalent to linguistic grammars. The set of sentences accepted includes the grammatical sentences, but it also contains much more. In other words, the model grammar is less detailed than the linguistic grammar. The reason for this is clear. The criterion for acceptability of the sentence no longer resides in the possiblity of syntactic-semantic

analysis, but rather in the possibility of finding a conceptual representation for it. Thus we have seen that the whole mechanism of selection restrictions, or at least an important part of it, need not be defined in the linguistic components of the model. One can, of course, by the same right, argue that such things are equally out of place in linguistic grammars in the first place (this is in fact what Harris does: cf. Volume II, Chapter 4, section 4.3.), but this idea is not widely held.

(3) Even when such an internal grammar is available, only a minimal use is made of it in the comprehension of many, if not most, sentences. As Schank emphasizes in his model, it will often be possible to go directly from individual words to conceptual representation. Only when ambiguities occur will further syntactic analysis be needed. The idea is that, in general, the internal lexicon, together with the deductive capacity of the conceptual basis, will be sufficient for the comprehension of sentences. The use of grammar will naturally be more intensive in the production of speech or text. But here, too, it will generally not be necessary that the grammar exercise control over selection restrictions, and so forth. The conceptual input in the text-generator will tend to be such that that kind of restriction will be satisfied automatically. And if this is not the case, then there is apparently good reason for the violation of such restrictions.

(4) As we have already pointed out, it is not the internal grammar which accounts for the creative aspect of human language, but rather the intelligent conceptual basis.

These four qualifications are not only statements of fact concerning computer simulation models, but, given the development in psycholinguistic research in recent years, they can also be considered as a realistic evaluation of the relation between linguistic grammar and human language usage, and consequently, as the most important conclusions of this chapter.

Regarding the role of formal grammars in models of the language user, we can conclude that a considerable nonlinguistic expansion may be expected in the future. We have seen that the input and output of each component of a simulation program are formal expressions, and even without computer simulation, a theory of the language user will have to give explicit formal representations for the coding of the information in the various components. Such coding systems are formal languages, and their analysis is a part of the theory of formal grammars. But there is still a great distance between the available formal languages whose mathematical structure is known, and the psychologically attractive coding systems, whose formal structure is not well known. The mathematical structure of predicate logic, for example, is well known, but it is hardly clear how suited that formal language is for the representation of conceptual information. On the other hand, the mathematical properties of perceptual coding systems which were developed from empirical work in psychology (see, for example, Leeuwenberg 1971) are not sufficiently known, and no parsing programs have yet been developed for them. Consequently, it is not possible at present to connect them to a linguistic or a conceptual subsystem in the theory.

In summary, then, we see that on the one hand the role of linguistic grammars in models of the language user is diminishing in a way, although it is decidedly not about to disappear, and on the other hand, the theory of formal languages and grammars appears to be of increasing importance for the nonlinguistic aspects of such models.

4

GRAMMARS AND LANGUAGE ACQUISITION

4.1. ASPECTS OF LANGUAGE ACQUISITION

The extent of the theme "grammar and language acquisition" depends on what is understood by the term grammar. If grammar is taken in the limited syntactic sense of Chomsky's *Syntactic Structures*, the theme concerns one interesting aspect of language development — the growth of sentence structure. As it is quite impossible to study as a whole the development of language in the child, it would by all means be acceptable to limit one's research to one important aspect, such as syntax. But this may be done, as we have seen in the preceding chapter, only on condition that one does not lose sight of the whole, for the development of syntax is not a closed system, but part of the total cognitive growth of the child. The expansion which the notion of grammar has undergone in the direction of semantics since about 1965 has recently begun to bear fruit in the study of language development. The stiff interaction between syntactic theory and developmental psychology which could be observed for a few years, is now giving way, thanks to a growing semantic interest, to a more integrated psycholinguistic approach to language development.

In the study of language development, however, the period from 1963 to 1968 was characterized by a strong accent on syntax. Syntactic grammars were written for the various stages of language development in the child between the ages of one and a half and three years, the period in which sentence structure develops most strikingly. Research was much inspired by the theory of formal grammars, both directly and through its applications in linguistics. As this is the subject of this book, we shall spend the first half of this chapter on a discussion of that interaction. In section 4.2. we first treat Chomsky's HYPOTHESIS-TESTING MODEL which stems from the theory of formal languages; some of its consequences can be evaluated in the light of later developments. In section 4.3. we shall discuss the concretization which the model underwent in the light of developing transformational grammar. This can be called a RATIONALISTIC acquisition theory, as opposed to an EMPIRICIST theory. Although most researchers pointed out at some time that syntactic development is only a part of general cognitive growth, this seldom amounted to more than lip service. There was in fact a tacit assumption in studies inspired on transformational grammar to the effect that grammatical development can be described as a rather closed system. Around 1968 this attitude began to change gradually. The question as to the origin of universal grammatical forms and their systematic development in children's languages could no longer be put off by the simplistic label "inborn" or derived forms of it. It became clear that at least two other points of view were necessary for an explanatory theory (in the sense of Volume II, Chapter 1, section 1.2.). The first of these concerns the general possibilities and limitations of the child's information processing (perception, memory, etc.). We shall call these PROCESS FACTORS. In section 4.4. we shall give a few incidental examples of these. The second point of view is that of the intention of the sentence; here one often speaks of SEMANTIC FACTORS. In the final paragraph of this chapter, section 4.5., we shall discuss a number of aspects of this, under the more general heading of CONCEPTUAL FACTORS. Semantic analysis of a child's language is impossible without a study of how the system of concepts — called the "conceptual basis" in the preceding chapter — develops in the child. In this discussion there will be no pretention to completeness, and the choice of topics for discussion will once again be determined by the formal language theme of this book.

4.2. LAD, A GENERAL INFERENCE SCHEMA

This schema was first described by Chomsky (1962). After mentioning that the child acquires the language of his environment in a strikingly short time and without special training, Chomsky proposes that linguists and psycholinguists consider the construction of a formal system, which, given the same input as the child (a set of linguistic utterances), will produce the same output as the child, a grammar of the language. He calls this a language learning device, and in later publications renames it a LANGUAGE ACQUISITION DEVICE, or simply LAD. In Chomsky's opinion, if such a language acquisition device is successfully constructed, its structure would be an hypothesis on the innate intellectual capacities which the child uses in the learning of a language.

For the construction of LAD, we must have at our disposal a linguistic theory which defines the class of possible grammars from which the child, and therefore also LAD, can choose. Such a theory must be a general characterization of natural languages, or a theory of linguistic universals. In terms used in Volume I, Chapter 8, this is the HYPOTHESIS SPACE of LAD.

One then must consider the input of LAD. Chomsky names a number of possibilities: LAD might only be given positive instances (called TEXT PRESENTATION in Volume I, Chapter 8), or both positive and negative instances (INFORMANT PRESENTATION), or even pairs of positive and negative instances (which might be called CORRECTIONS). Other input might also be required, such as meaning, but Chomsky (1962) rejects this as being probably irrelevant:

> For example, it might be maintained, not without plausibility, that semantic information of some sort is essential even if the formalized grammar that is the output of the device does not contain statements of direct semantic nature. Here, care is necessary. It may well be that a child given only the input of (2) [LAD] as nonsense elements would not come to learn the principles of sentence formation. This is not necessarily a relevant observation, however, even if true. It may only indicate that meaningfulness and semantic function provide the motivation for language learning, while playing no necessary part in its mechanism, which is what concerns us here.

And Chomsky repeats essentially the same argument in *Aspects* (1965, p. 33). Syntactic input of a certain composition is for LAD what was called OBSERVATION SPACE in Volume I, Chapter 8.

LAD must also dispose of an EVALUATION PROCEDURE. This is needed in order to be able to choose from among those grammars in the hypothesis space which are capable of accounting for all observations. The evaluation procedure must lead to the choice of an optimal grammar. What the criterion for this must be is not defined by Chomsky, but he suggests that it will have to do with the intuitive adequacy of the structural descriptions which a grammar assigns to sentences. A condition for evaluation by LAD is that a mechanism be included by which, given a grammar G and a sentence s in $L(G)$, the structural description of s can be derived in terms of G.

Chomsky and Miller (1963: 276-277) add to this that LAD must also be provided with HEURISTIC PROCEDURES, by which a number of promising grammars in the hypothesis space can be evaluated rapidly and in greater detail. Such procedures should comprise a set of intelligent induction methods which can accelerate the learning process. But Chomsky and Miller also point out that an a priori limitation of the hypothesis space is much more favorable to a rapid learning process than a large battery of heuristic methods:

> The proper division of labor between heuristic methods and specification of form remains to be decided, of course, but too much faith should not be put in the powers of induction, even when aided by intelligent heuristics, to discover the right grammar. After all, stupid people learn to talk, but even the brightest apes do not.

And in *Aspects,* where the theory is once again summarized, we read,

> This requires a precise and narrow delimitation of the notion "generative grammar" — a restrictive and rich hypothesis concerning the universal properties that determine the form of the language.

Basically LAD is nothing other than a schema for the analysis of the language acquisition situation. It makes explicit the relations among a priori hypothesis space, the nature and extent of observations, and the deductive capacity of the child, which together lead to the discovery of the grammar. Chomsky correctly indicates the need to choose between an hypothesis space a priori limited on the one hand, and a strong combination of observations and intelligent heuristics on the other. In *Aspects*, he relates that choice to two characteristic conceptions which can be found in literature on language learning; he calls these "empiricist" and "rationalistic". The empiricist point of view, in terms of LAD, is that there is hardly any limitation on the a priori hypothesis space, and that LAD includes strong heuristic principles by which, with any input, it can ultimately deduce the grammar. Within this framework much remains to be said on the nature of these heuristic principles. One might, for example, imagine general association and generalization principles, and so forth, but that is beside the point here. For the empiricist point of view, the point is that there is no, or very little, limitation on the hypothesis space with which the child begins his language acquisition. In other words, LAD would then contain no linguistic theory worth mentioning. The rationalistic conception, on the other hand, sees LAD's power in a restrictive a priori hypothesis space; here the heuristic principles become relatively unimportant. Here no inductive effort, no difficult learning process, is needed. According to Chomsky (1965:51), already Humboldt held the view that

> one cannot really teach language, but can only present the conditions under which it will develop spontaneously in the mind in its own way.

Chomsky then states that the essential question in comparing these two points of view is the question, mentioned above, concerning the possibility of constructing LAD, or, as he calls it, the ADEQUACY IN PRINCIPLE of the theory of language acquisition. Only when that question is answered, either in an empiricist or in a rationalistic sense, can we think of how the acquisition algorithm of LAD should be composed in order to have properties comparable to those of the child's language acquisition, especially as far as the REAL TIME ASPECTS of the model are concerned. He then informs us in *Aspects*:

> In fact, the second question [as to the *real time* algorithm] has rarely been raised in any serious way in connection with empiricist views... since study of the first question [the adequacy in principle] has been sufficient to rule out whatever explicit proposals of an essentially empiricist character have emerged in modern discussions of language acquisition.

This, however, is a highly one-sided statement. In 1965 the question of the feasibility of language acquisition algorithms had not yet been solved, and it became evident from the later work by Gold and others that it was not only impossible to construct an empiricist LAD, but also a rationalistic one.

Before we go into this, however, we should point out that Chomsky's schema of analysis, as well as his own rationalistic position within it, were highly stimulating to the study of language acquisition in the 1960's. On the one hand, linguists once again realized how important it is to develop a general linguistic theory in close interaction with the study of language acquistion. On the other hand, developmental psychologists began consciously to search — at first, not without success — for the early universals in the language of the child. In these the hypothesis space of LAD should be reflected in a direct way. In the following paragraph we shall discuss the way in which general linguistic theory and language acquisition theory are connected *in concreto*. For the present we shall only say that, thanks to the LAD model, renewed attention was paid to the study of the relations between linguistic universals and early language acquisition. One could also observe a growing interest in the nature of the input, the observation space of LAD and therefore also of the child. Interesting new attempts were made to find out what the linguistic environment of the child really consists of, and to discover the basis on which the child modifies his language. We shall also return to this later. Finally, research employing various experimental means was performed on the structural descriptions which children implicitly assign to sentences. Several new techniques, such as the use of play-acting and imitation, were developed for this.

This research did much to awaken interest, but, rather remarkably, no one attempted to answer Chomsky's basic question as to whether LAD could be constructed in the first place. That possibility was simply taken for granted. Braine (1971) was the first psycholinguist to examine the LAD schema as such, and to declare it intrinsically impossible as a model of language acquisition. This whole development took place entirely without contact with the parallel research which was being done on grammatical inference, which we have discussed in Volume I, Chapter 8 (to which should be added that those who performed that research were in their turn rather aware of the psycholinguistic problems). Braine's treatment and rejection of the LAD model could, in fact, have been more complete on the basis of the results obtained in that field. In fact, in his discussion Braine confused the LAD schema itself with its rationalistic pole, as we will show presently.

Let us first have a closer look at the question of the possibility, in principle, of constructing LAD. LAD must do two things: in the hypothesis space it must find a subset of weakly equivalent grammars for L, and on the basis of an evaluation procedure it must decide which of those grammars is the best.

Gold (1967) — cf. Volume I, Chapter 8 — deals only with the first problem, whether an observationally adequate grammar for L can be found in the limit. As we have seen, he tried to determine whether it was possible to construct a Turing machine for various input conditions and language classes, by which the language is learnable in this weak sense of the word. This is precisely Chomsky's basic question, although the application of Gold's conclusions is too optimistic in a number of respects. In the first place, if LAD is to be a theory of the language-acquiring child, it can never be a Turing machine. We could temporarily get around this objection by supposing that the finite properties of the child do indeed lead to characteristic human errors in the learning process. In the second place, Gold had to suppose that the presentations are complete, that is, that every positive and negative instance (every string over V_T^+ for informant presentation, and every sentence in L for text presentation) appears within a finite amount of time in the information sequence. This assumption will become more important when the secondary question of the real time properties of the system will be treated. However this may be, Gold's assumptions are more tolerant than can be allowed for LAD, and conclusions concerning LAD must therefore be at least as strong as Gold's results.

Table 8.2. in Volume I shows that only finite languages are learnable by means of text presentation (with only positive instances). And for finite languages, the only possible algorithm is based on the idea that the whole language is observed in finite time. If one wished to maintain that for the learning situation natural languages can be considered as finite sets, then there could be no doubt as to the extremely large size of the language, and LAD would consequently have impossible real time properties. So, for text presentation, LAD cannot be constructed by any schema whatsoever, be it empiricist or rationalistic.

Informant presentation is much more favorable. There is a learning algorithm in principle even for the class of primitive recursive languages. Is this encouraging for the possibility of constructing LAD? There are two reasons for answering this question in the negative. The first of these reasons is of limited significance, but as a programmatic question, it is worth considering. In Volume II, Chapter 5, we showed that the *Aspects* grammar and its later developments precisely define the class of type-0 languages. Table 8.2. in Volume I shows that in principle such languages are unlearnable, even with informant presentation. This is an extreme form of the empiricist learning situation: the hypothesis space is unlimited. There is thus no heuristic possibility whatsoever for the discovery of the grammar. It is therefore necessary, even from the empiricist point of view, and obviously also from the rationalistic point of view, to limit the hypothesis space to the level of primitive recursive. The second reason for doubt as to the possibility of constructing LAD — and this from any point of view — is Braine's argument that the child, at best, is in a situation of text presentation, not of informant presentation. There are indeed strong arguments to support this, and we shall mention a number of them. (1) By social and cultural circumstances, the speech of many children is never corrected; that is, a string of words is never marked as incorrect. Nevertheless such children learn the language. (2) When strings are indeed marked as incorrect, this has strikingly little effect. Experiences of Braine (1971) and McNeil (1966) and experiments by Brown (1970) show that directed corrections, even in the form of expansions (for example, Child: *Eve lunch*; Mother: *Eve is having lunch*), have little or no effect on language development. Such negative information cannot be considered as input for LAD if the child never reacts to it. (3) Learnability-in-principle demands a complete presentation. This implies that it should be possible for any negative instance to occur in a finite amount of time. A study by Ervin-Tripp (1971) showed, however, that the language spoken to children is strikingly grammatical, while utterances not addressed to the child are generally ignored by him, and therefore cannot be considered as real input. Ungrammatical sentences, thus, are not only not marked as such, but also occur only seldom in the input. However, for every realistic real time mechanism, positive and negative instances should occur with about the same frequency, and this is decidedly not the case for the language-acquiring child. (4) One might argue that the lack of reaction to the ungrammatical utterance of the child (ungrammatical, that is, with respect to the adult language) is an indirect indication of ungrammaticality. It is indeed the case that ungrammatical sentences stand a greater chance of being unintelligible than grammatical sentences, and consequently the adult or the other child has also a greater chance of not giving an adequate reaction. This argument, however, is a two-edged sword. At first nearly all utterances made by the child are ungrammatical, but every utterance which can be interpreted can nevertheless be followed by an adequate reaction. It is impossible that grammaticality be a condition for reaction, and the child who supposes that it is, will hopelessly confuse the positive instances with the negative.

With a text presentation, as we have said, it will be impossible, in principle, to construct LAD, and any linguistic or psychological program of research within that framework is doomed to failure.

It must be emphasized that this holds not only for a rationalistic LAD, but also for an empiricist one. Braine (1971), who rejects the possibility of constructing LAD on grounds of the relative absence of negative information for the child, goes on to replace LAD with what he calls a DISCOVERN PROCEDURES acquisition model, a model with strong inductive possibilities. In doing so, however, he was not aware that he did not depart from the LAD schema at all, but only from its rationalistic pole. His model has all the LAD characteristics: on the basis of a text presentation as the minimum input it generates a grammar as the output, by means of a very wide hypothesis space and a number of strong heuristic principles (to which we shall return in section 4.3. of this chapter). But this is precisely the empiricist version of LAD, and we can conclude from Braine's own arguments that this discovery procedures theory is quite as impossible to realize as the model he rejects. Only if essentially different aspects are added, such as meaning input, can one avoid the above mentioned difficulties. It should be said that Braine does indeed do this (cf. section 4.3.), but his argument for the discovery procedures model is not based on those additions.

This of course does not mean that the heuristic principles proposed by Braine are a priori without empirical foundation. Quite the contrary is true. But the point here is that the basic question as to whether LAD can be constructed must be answered in the negative, from either a rationalistic or an empiricist point of view, and that we must therefore suppose that the language acquiring child must receive information other than text presentation, if he is to learn his language. One might well think of semantic information in this connection. Although we shall see later that a particular form of text presentation does offer certain possibilities, it remains difficult to imagine that anything but semantic information will prove to be effective. (Of course one could introduce the grammar itself into the model, but that would not only be trivial, it would also be quite unrealistic for a model of language acquisition.) We can therefore conclude that the role of semantic input is not simply incidental, as Chomsky suggests, but that it is entirely essential. Without something like semantic input, the language will not be learnable. The minimum formal requirements on the form of the semantic input are still a completely open question, however. One could consider a whole gamut of information, from simple paraphrases of sentences (as parents sometimes make) to a complete visual, acoustic, and motor acting out of the intention of the sentences. This is an interesting and as yet untouched field of formal research.[1]

In the rest of this section we shall concentrate our attention on the two remaining problems, the evaluation procedure and the real time characteristics of the model of language acquisition. The discussion of these leads to the consideration of presentation procedures other than Gold's text presentation, and, consequently, to a number of qualifications of the results just mentioned.

In Volume I, Chapter 8, we treated the Bayes-type evaluation procedure for context-free grammars which was given by Horning. For the development of that theory it was necessary to define the notion of learnability in a weaker form, as follows: there is a procedure such that every nonoptimal grammar is rejected within a finite amount of time. With this definition and a stochastic text presentation, the class of nonambiguous context-free grammars is learnable, provided that an a priori probability distribution or a complexity distribution is given for that class. If we are prepared to accept such a weak form of learnability in a model of language acquisition, we might wonder once again how far we can come without semantic information. It should first be noted that stochastic text presentation seems to be an acceptable model for the input of the child. On the one hand Horning shows that considerable deviations from stochastic text do not noticeably influence the results of his analysis, and, on the other hand, that natural language is a sufficient approximation of stochastic text. Moreover, a real probability distribution over the hypothesis space is not essential to Horning's argument, provided that in one way

[1] Semantic relations define equivalence classes over the sentences of a language. Thus the class of paraphrases is defined for every sentence (relation: equality of interpretation). Is a language learnable if a set of semantic relations is offered in the form of equivalence classes?

or another a complexity distribution is given. The probabilistic aspect is therefore less essential than it appears. Still a probabilistic model of language acquisition has many advantages which are lacking in a deterministic model such as LAD or as Gold's version of LAD. We mean the advantages regarding the basic question of the feasibility- in-principle of LAD. It is the case, for example, that a stochastic model is noise resistant, a realistic characteristic. The child might indeed occasionally hear utterances which are ungrammatical, but are not marked as such. With Gold's procedure, the child would then be permanently confused. Horning shows that his procedure is highly resistent to such noise. An experiment by Braine (1971) also shows that human beings have the same characteristic. He presented adult subjects with sentences from an artificial language with the following grammar:

$S \rightarrow B'\, q\, A'\, r$	$B' \rightarrow B\, f$
$S \rightarrow p\, A'\, B'$	$A \rightarrow \{a_1, a_2, \ldots, a_6\}$
$A' \rightarrow A\, f$	$B \rightarrow \{b_1, b_2, \ldots, b_6\}$

The lower case letters belong to the terminal vocabulary, but in fact in this experiment they formed meaningless but pronounceable syllables. Together with grammatical sentences, the subjects were also presented with ungrammatical strings; these latter made up 7 percent of the total number of strings presented. The ungrammatical strings were first and second order approximations of the language (the language is finite, and consequently, with the assumption of a uniform probability distribution over L, all conditional probabilities are known precisely). The ungrammatical strings were three to eleven words long. The degree of learning was tested by means of as yet unpresented sentences of the languages, either in the form of a completion test (filling in omitted words), or in the form of a recognition text. A control group was presented only with grammatical sentences. Both groups showed a high degree of learning; many of the subjects attained full mastery of the language, although none could formulate the rules. The most striking point was that the addition of ungrammatical sentences ("noise") had no negative effect. Most subjects realized slowly but surely that there were a few strange sentences among the others, but they simply ignored them. The noise resistance of Horning's model is therefore a psychologically attractive characteristic.

But, on the basis of Horning's results, it is not possible to reject in principle the possibility of constructing a stochastic model of language acquisition. All that can be said at present is that nothing is known on that possibility for anything higher than the nonambiguous context-free languages. As for the learning of natural languages, the possibility of constructing such a stochastic model of language acquisition is still an undecided question.

But however the solution of this may be, it seems that such a model is bound to fail with respect to the real time aspects. Horning (1969), who investigated various heuristic procedures with the intention of accelerating the learning process, writes:

> It is clear, however, that grammars as large as the ALGOL-60 grammar will not be attainable simply by improving the deductive processing,

and further:

> But adequate grammars for natural languages are certainly more complex than the ALGOL-60 grammar, and the range of observed natural languages is sufficiently large to require a rather rich hypothesis space — probably much richer than anything we have considered in this study.

On the basis of real time considerations, therefore, this model is also out of the question. It holds here, too, that more input of a different nature than (stochastic) text will be needed. In this connection Horning remarks that the child might receive a completely different kind of presentation of his language:

> he is confronted with a very limited subset, both in syntax and vocabulary, which is gradually (albeit haphazardly) expanded as his competence grows.

This is an important suggestion. The hypothesis space of the child would then be limited at first, and the language addressed to him would fall more or less into the same class; it would thus be possible to develop an optimal subgrammar. This would be incorporated at a later stage into a new hypothesis space of larger grammars, and the observation space (the language of adults) would in turn adapt to this, and so forth. This would amount to an "intelligent" presentation of the language. It is interesting in this connection to refer to a number of studies done at Berkeley (cf. Ervin-Tripp 1971) on the subject of the language which adults address to children (for further references see Historical and Bibliographical Remarks). That language proved to be simple. It consisted of short sentences, included few conjunctions, passive forms, and subordinate clauses; on the other hand, it contained many imitations of the child's language. Paraphrases and expansions of the child's utterances were to be found, but the general syntactic form of the child's language was nevertheless maintained. Moreover, it was shown that mothers allow the complexity of their sentences to grow with the age of the child, so that, as Ervin-Tripp points out, "the input maintains a consistent relation to the child's interpretative skill". One might say that the child learns a miniature language, and that the parents adapt themselves to the limited hypothesis space of the child.

Although Horning's proposition agrees well with the facts, we are once again in uncertainty. The boundaries of learnability with such an "intelligent" presentation are not known. For natural languages, ambiguity will continue to constitute a serious problem, but for the present we can only state that, with this type of presentation, learnability without additional semantic input cannot yet be excluded a priori.

But even in that case it is not clear why semantic input in the model of language acquisition must necessarily occupy a secondary place. The acquisition mechanism might be able to work much more efficiently if it can dispose of such information at the same time. Finally, it is less a question of what is possible than of what is in fact the case.

It is tragic to cut off from the domain of research the large field of cognitive relations which are found in early sentences (...) by assuming *a priori* that there are no interesting problems in their acquisition. Dogmatism without evidence is to say the least presumptuous (Ervin-Tripp 1971).

4.3. UNIVERSALS OF ACQUISITION FROM THE RATIONALISTIC AND THE EMPIRICIST POINTS OF VIEW

Although every theory of language development which has a syntactic input and a grammar as output can be brought under the LAD schema, it is an historical fact that only theories of a rationalistic strain were explicitly based on that schema. As we have already stated above, rationalistic here means the point of view according to which LAD disposes of a very limited hypothesis space. Thus the language-acquiring child would have a strong starting position: he has implicit knowledge of the universal characteristics of human language, and for a large part those universals determine the characteristics of the specific language which he is to learn. Inference is needed only for the nonuniversal, specific characteristics. McNeill (1966; 1971) is the most extreme exponent of this point of view. The following quotation shows how he relates language development to general linguistics.

However, by accepting linguistic theory as a description of a child's capacity for language, one is led to a very natural explanation of the development of abstract linguistic features. Apparently many (but not all) features in the deep structure of sentences correspond to linguistic universals (Chomsky, 1965). The general form of human language is largely defined in its underlying structure. There is, in addition, the general idea of a grammatical transformation, which likewise contributes to the general form of human language as a formal universal. However, particular transformations (...) are unique, and so must be acquired from a corpus of speech. Accordingly we may think of LAD (or children) *making* such universal features as the basic grammatical relations abstract by acquiring the particular transformations of the language to which they are exposed. We may even say that languages possess such features as the basic grammatical relations as abstractions because they first correspond to children's basic capacity for language, but are subsequently buried under a mass of particular transformations. Accordingly we should expect to find that the earliest grammatical production of children will contain the abstract features of the deep structure but few of the locally appropriate transformations. Young children should "talk" deep structures directly. And that is precisely what an examination of children's early speech shows (Miller and McNeill 1968).

The child thus begins by nature with a grammar which is an important part of the base component of a transformational grammar. He talks deep structures, or better, in "deep sentences". He later learns the transformations specific to the language with the help of a kind of foreknowledge concerning the structure of transformations. We see here, albeit in another form, the isomorphism of the preceding chapter. Just as the grammar generates a sentence from the deep structure to the surface structure, the language usage of the child develops from deep sentences to surface sentences.

But the empiricist extreme of LAD, on the other hand, does not lead to the conclusion that the child begins with talking deep sentences. In that point of view, the child has in fact little a priori insight into the language of his environment, and can learn only because of strong inductive principles. Inference of the grammar is principally based on regularities noticed in the observed language. Such regularities will have to be visible in surface phenomena. By induction, the child can then make connections between the surface regularities he notices, a new level of abstraction is then reached, and so forth. This is precisely the model which Braine (1971) proposes. As we have mentioned in the preceding paragraph, Braine incorrectly opposed his model to LAD, while in fact it is opposed only to the rationalistic version of LAD which historically became confused with LAD itself. Braine correctly calls his version a DISCOVERY PROCEDURES model. In its most simple form, it has a READER and a MEMORY. The reader scans the input (linguistic utterances) and determines what the characteristics of that input are. For the input *John walks*, for example, it would, among other things, register "two words", "*John* + word", "word + *walks*", and so forth. These characteristics are stored in the memory. In the simplest form of the model, the memory consists of a number of layers. The characteristics of the input are first put into the uppermost layer. If a characteristic is again observed in later input, it is moved to the next layer, etc. Frequently occurring characteristics are ultimately placed in the lowest layer, the permanent memory. The reader can in turn use the characteristics stored in the permanent memory for a more efficient analysis of new input. It might find that the input has certain combinations of characteristics, and register this fact as a feature in the uppermost layer of the memory. The model is further refined in a number of ways. Features in the intermediate layers are supposed to disappear slowly, thus protecting the model against noise. It is also supposed that the permanent memory at first contains little information, only short input sentences are analyzed, and only some of their features are registered. Other properties have also been added to this model.

This theory thus predicts that the child will begin by registering the frequent and regular characteristics of short sentences. In principle these can as easily be specific to the language as universal. If the first speech of the child is guided by knowledge thus obtained, it is at least not necessary that the child should speak in deep sentences. In that respect, the rationalistic model thus makes stronger predictions. The empiricist model, on the other hand, is very sensitive to frequency. Frequently occurring characteristics (of short sentences) are registered more rapidly in the permanent memory than infrequently occurring ones. We might thus expect a certain frequency matching between the language of the child and the language of his environment. This is not predicted by the rationalistic model.

As was shown in the preceding paragraph, both theories are on rather weak ground as long as no additional information is introduced into the model. Braine (1971) does, however, introduce additional information by pointing out the importance of the semantic input (more will be said on this in section 4.5.). In the present section, however, we shall only discuss the simple syntactic versions of the two models, and in doing so will not do complete justice to Braine's ideas. The theory of McNeill, on the other hand, still fits completely into the LAD schema.

Later we shall mention a few of the many studies which have been done in the field of tension between the rationalistic and the empiricist theories. Before doing so, however, we must clarify a number of notions which have often and unnecessarily troubled the discussion, and still lead to all sorts of confusion.

(1) We have already seen that LAD as an inference schema is incorrectly confused with its rationalistic pole. Rationalistic in this connection means nothing other than that the model disposes of a limited a priori hypothesis space.

(2) Although foreknowledge necessarily results in universals, that is, generally or universally occurring characteristics of human language (because each acquired grammar ultimately satisfies all the characteristic properties of the a priori hypothesis space), it does not hold that all universals must lie in the hypothesis space. Both general characteristics of the observation space (regularities in the observed language) and general features of the inference mechanism (such as the perceptual and cognitive faculties of the child) can lead to language universals. It is thus not the case that only rationalistic models are capable of explaining the existence of universals, although this is often argued. Characteristics proceeding from the limitations of the hypothesis space are called STRONG UNIVERSALS by McNeill (1971). He calls universals proceeding from the nature of the inference mechanism WEAK UNIVERSALS. He does not mention the possibility that the observation space might yield additional universal characteristics.

(3) Within the rationalist camp it is often held that universal characteristics are necessarily innate and not learned. But this is the result of two errors in thinking. In the first place, the possibility that universals might proceed from characteristics in the observation space is overlooked, as McNeill does. In the second place it is tacitly supposed that that which is present in the organism at the beginning of language learning is innate. This can be interpreted in two ways, to one point of view the proposition is incorrect, and to the other it is meaningless, If "innate" is taken to mean

"given at birth" in the traditional sense, it implies that the child learns nothing between birth and the beginning of language learning, but this is absurd. The proposition becomes meaningless when later learning is nevertheless excluded and innate is taken to mean "genetically given". That which is given in the genes can develop only in interaction with its surroundings (the intracellular and extracellular prenatal environment at first, and the internal and external surroundings of the organism later). It is quite arbitrary to call some of these interactions learning and others not, and the question becomes semantic, rather than empirical. It is wiser to avoid words like innate, as ethologists have since long taught us (cf. Hinde 1966). This, however, is not to deny the high degree of specificity of languages. The genetic equipment of the human is in some way particularly suited for the development of language. See Lenneberg (1967) for a thorough treatment of this subject.[1] This does not mean that other animals are incapable of acquiring a productive language form. Work done by the Gardners (cf. Brown 1970) casts a whole new light on this. The Gardners taught sign language to their pet chimpanzee, Washoe. Washoe developed a good vocabulary, and made understandable sentences up to four words long. The experiment ended when Washoe reached maturity. Of course Washoe's language, although much more complex than any other attained by an animal, remained different from children's languages. Washoe used a free word order, for example. But these and other findings by the Gardners (1969) show that chimpanzees possess considerable linguistic potentialities.

(4) Finally, confusion is frequently caused by the identification of the empiricist version of LAD with behavioristic learning theory. An empiricist theory only states that the organism disposes of little or no foreknowledge of the grammar, but that it can deduce the grammar by means of strong heuristic procedures. These procedures are part of the a priori equipment of the child. Just as

[1] We do not share Lenneberg's conception that language is so much biological that it has the usual critical maturation period, in any case not as far as syntactic and semantic structures are concerned. In our opinion, these can as easily be learned at any later age.

in the rationalistic model the nature of the foreknowledge in question must still be defined more completely, empiricists still face the task of analyzing the structure of the inference mechanism of which they speak. Some (together with Staats 1971) will take first recourse to stimulus response mechanisms, such as association, generalization, and discrimination, but that is by no means intrinsic to the empiricist model of language acquisition. At present, however, the only thing which is known with precision on this point is that all regular grammars, as well as all the more complex grammars which fall into Miller and Chomsky's (1963) *tote schema*, are learnable by means of a certain *S-R* mechanism in which correct responses are confirmed and incorrect responses are not confirmed (Suppes 1969). Suppes rightly points out, however, that such a model is quite far away from the observations. It is as if a computer program were written in the form of a network of flip-flops. It works well in the end, but it does not increase our insight into what actually happens during learning. An empiricist theory should be explicit on the components of the inference mechanism, or, in other words, on the subroutines. The way in which they could be realized by means of the elementary *S-R* mechanisms — or any other units for that matter — is only of secondary importance to an empiricist theory; it is not an essential part of the theory any more than the physiological basis of the foreknowledge is an essential part of a rationalistic theory.

To summarize, we can ask, from rationalistic and empiricist points of view, what may be expected concerning the development of syntax in the child.

The *rationalistic model* states that the linguistic development enjoys a high degree of independence from the observations. All children will at first use the same syntactic structures and talk in "deep sentences", regardless of their social, cultural, or linguistic circumstances. Training will do little to change this. The later transformational development, on the other hand, is specific to the language and can be ascribed either to the a priori hypothesis space (strong universals) or to the a priori characteristics of the acquisition mechanism (weak universals).

The *empiricist model* states that the language of the child will at first reflect the superficial characteristics of the corpus presented to him (or at least, those of that which he actually observes). This corpus, and consequently the earliest linguistic behavior, will vary with the social, cultural, and linguistic circumstances of the child. Therefore it is not expected that children will at first use the same syntax, nor that they will talk in "deep sentences". They will, on the other hand, be sensitive to training (systematic influence on the input). Where universal phenomena occur in children's languages, it should be possible to ascribe them to universal characteristics of the corpora observed, or to universal characteristics of the information processing mechanism.

What is the evidence? In the 1960's much activity took place in the field of syntactic language development. It would be impossible to attempt to give a complete account of that work within the scope of this book. We refer the reader to the short survey given by Slobin (1971a), or to the detailed and complete discussion by Braine (1972). We shall limit the discussion here to a few data concerning the first two-word sentences of the child.

Most of the studies were characterized by the analysis of extensive corpora of speech, collected within short periods of time. Each study dealt with one or only a few children, but was worked out in great depth. Most often the speech of the child (including dialogues with others) was recorded on tape and processed later. This information was usually completed by the addition of data on the situational context, and sometimes of experimental data, such as the results of imitation and comprehension tasks. By the examination of the same child at various stages of development, longitudinal data were also collected on a number of them.

The first three studies in the period in question were those of Braine (1963), Brown and Fraser (1963), and Miller and Ervin (1964). They all contained analyses of the brief period of two-word sentences (the child is about a year and a half old), and it was felt that striking agreement was found in syntactic structure. That agreement can be expressed in the following PIVOT GRAMMAR:

$S \rightarrow P_1 + O, \; S \rightarrow O + P_2, \; S \rightarrow O$

Braine called P_1 and P_2 PIVOT categories. They contain a small number of frequently used words, which can only occur exclusively in the first position (P_1) or exclusively in the second position (P_2). All other words fall into the open class O. A typical P_1 word is *allgone*, and a typical P_2 word is *on*. Characteristic sentences are *allgone shoe*, *allgone bandage*, *shoe on*, *bandage on*. The two-word sentence period is not completely free of still another construction, $O + O$, but it is argued that that form is infrequent at first.

Rationalist authors considered this a first universal phenomenon,[1] the expression of an innate grammatical relation which in the language of adults could be defined only over the deep structure. The pivot-open construction, in particular, was taken as a relation of modification (McNeill 1966), and P and O were considered to stand for the modifier and the head, respectively. The class of modifiers is at first extensive, containing, for example, demonstrative pronouns *(this, that)*, articles *(a, the)*, adjectives *(big)*, and possessive pronouns *(my, your)*. Later, according to McNeill, these categories are further differentiated, as may be seen in the distributional properties of the first three-word sentences (*that my car* occurs, while *my that car* does not). Details of this analysis are not important for the present purposes. What is important is that on the basis of these distributional characteristics it was decided that there are fundamental grammatical relations which are apparently innate. In the *Aspects* model these could only be defined over the deep structure, but in the language of the child, they simply appear as surface constructions. The rationalist argument thus rests on a rather high degree of interpretation, that is, (a) the assignment of categories to words on grounds of distributional analysis, and (b) the assignment of universal relation names to the (hierarchic) order relations between the categories. Concerning the pivot grammar, both of these points appear to be quite arbitrary. As for (a), it can be pointed out that

[1] Looking back at the literature, it is striking to notice how so few English-speaking children have been made responsible for so many universals.

the pivot grammar is by no means an exhaustive description of the distributional relations. Even in the original material on which McNeill based his universality argument, deviant constructions appear. Thus we find $P + P$ constructions in Brown and Fraser's material, and in all the material some pivot words can occur in either the first or the second position. Such words obviously belong to two grammatical classes (P_1 and P_2), but this is not further analyzed. On (b) we can say that the assignment of fundamental grammatical relations to such pivot-open constructions as well as to open-open constructions is an arbitrary matter from a distributional point of view. It is quite clear that one must often be able to assign more than one grammatical interpretation to a given order of categories. This is most evident for ambiguous two-word sentences. As an example of this, Bloom (1970) mentions the sequence *Mommy sock*, which in one situation can clearly mean "Mommy's sock",a nd in another, "Mommy is putting on my sock". McNeill also freely gives multiple interpretations to certain category sequences. However this means that children at first do not simply speak in "deep sentences" in which the relations should be immediately evident, but that they express different functional relations sometimes in the same word order. But this means that if functional relations, such as those of subject, object, and modification, are to be defined configurationally, as in the *Aspects* model, a transformational instead of a purely phrase-structure grammar will be needed for the level of the two-word sentences. This conclusion is also drawn by Bloom, as we shall see in the following section.

There is still another problem with the rationalistic interpretation of the pivot grammar, even if we suppose that it is observationally adequate. This problem has to do with the distributional differentiation of categories in later development. The supposed universality of syntactic categories in the base grammars of various languages forces McNeill to find a rationalistic genetic explanation. In his original theory (1966) he stated that those categories grow by a gradual differentiation on the basis of the primitive categories P and O. This differentiation is first expressed in increasing distributional refinement, an example of which has just been given (cf.

Chapter 1, section 1.3. for further details). But if this is so, it is difficult to understand why some words which will later become adjectives are at first found in the *P* class while others are found in class *O*. How, in the course of language development, they manage to arrive in the same category is a riddle. Therefore McNeill had to change his position (1971). He states that for every word not only a category feature, but also one or more "contextual features" are learned. In the lexicon of a child at a given stage of development, for example the word *big* might have the category feature *Adj* as well as the following two contextual features: [+ — *NP*] and [+*NP* —]. The child can thus make constructions such as *big house* and *house big*, both of which, despite the difference in word order, express a universal modification relation, namely the adjunction of an adjective to a nominal element. The word order of the first two-word sentences will depend upon which of the two contextual features is acquired first by the child. It may be one feature for one adjective, and the other feature for another. In both cases the feature lacking will be added at a later stage of development when this is required by the language in question. Consequently the final category structures will converge, or better, in spite of the distributional differences, there was never any difference in category structure. Notice that McNeill uses the same notation for these contextual features as is used in *Aspects* for subcategory features (cf. Volume II, Chapter 3, section 3.1.1.). But McNeill expands the notion considerably. Thus a contextual feature can show that a word can be the subject of a sentence [+ — *Pred P*], which is not the case in *Aspects*. All McNeill is actually doing here is replacing the context-free base grammar with what is essentially a categorial grammar (cf. Volume II, Chapter 4, section 4.2.). The word order is determined by a set of contextual features; in terms of a categorial grammar this means that one or more (complex) categories are assigned to each word. The child then need only learn the set of categories for the various words, and the word order in the sentence constructions will then directly follow from the mechanism of the categorial grammar. McNeill tries to save the pivot grammar in this way, while maintaining the rationalistic point of view. But when we formulate his theory of contextual features as a categorial grammar, where words have multiple categories acquired in an indefinite order, we see that McNeill abandons precisely the most characteristic property of the pivot grammar, namely, fixed word order. With the contextual features, every pair of words can occur in any order, provided that there is sufficient choice among the complex categories for the words. Or, in McNeill's formulation, each unexplained word order can be accounted for by the addition of a new contextual feature without the necessity of readjusting the category. These categories are universal and given from the beginning. In this way McNeill saves the rationalistic point of view by making it untestable. But at the same time the pivot grammar is implicitly rejected.

More recently, however, the pivot grammar has been rejected much more explicitly. Bowerman (1971) studied a Finnish child who showed no evidence of a pivot grammar. Two of the three children studied by Bloom (1970) likewise exhibited no pivot grammar. Schaerlaekens (1973) in a strikingly complete study of the two-word phase of six Dutch-speaking children (two sets of triplets) also found no evidence of a pivot grammar. Moreover, using explicit criteria, she made a more detailed study of the corpus of one of the children with the purpose of trying to find a pivot grammar for it, but this did not prove possible.

Concerning the first two-word sentences of the child, therefore, we can safely conclude that the rationalistic expectation of those sentences having the same syntactic structure for all children cannot be confirmed. There are remarkable differences in word-order even at this first stage of development (notice that we did not as yet formulate anything concerning semantic universals, but are still dealing with purely syntactic statements).

What about the predictions made on the basis of the empiricist model? The empiricist version of LAD predicts feature matching: the word order in the earliest sentences should reflect the dominant word order in the language of the environment, or at least that of the language addressed to the child. But empiricist researchers have never seriously tried to prove this. The interlinguistic research on language acquisition done at Berkeley (cf. Slobin 1970 among others) showed that many, if not all, children in various linguistic environments begin with a few pivot-like constructions, and that they most often use the dominant word order of their surroundings. In cases where the word order is very free (as, for example, in Russian) and there is therefore no basis for adaptation to surroundings as far as word order is concerned, children nevertheless appear to choose a fixed word order and to maintain it. More precise data may be found in Bowerman (1971), where the author treats naturalistic observations of verbal communication between a Finnish child and its mother. When the average length of the child's sentences was 1.42 morphemes, the language of mother and child showed the following frequencies in the word orders of subject (*S*), verb (*V*), and object (*O*) (notice that these categories were not defined distributionally, but semantically, in Bowerman's study. It is generally not difficult to decide which word is the subject and which is the object if one knows what the discussion is about.)

	SV	*VS*	*VO*	*OV*	*SVO*	*OSV*	*OVS*	*VSO*	*SOV*
Mother	47	5	16	3	32	0	1	1	1
Child	44	4	4	1	7	1	0	0	1

The child has obviously taken over the dominant word order in Finnish, and in particular the most frequent *SV* and *SVO* patterns. In Schaerlaekens' work, although the imitation of word order was not the object of separate study, rather strong arguments can be found in support of the word order imitation theory in two-word sentences. The genitive relation, for example, which Schaerlaekens calls the "relation of fixed allocation" (and which once again is defined semantically and not distributionally) for each of the six children, is reflected in the fixed word order POSSESSION-POSSESSOR: *auto Piet* 'car Piet', *boek madame* 'book lady', *boot Diederik* 'boat Diederik', *koek Gijs* 'cookie Gijs', *poep kindje* 'bottom child', *koets Karel* 'wheelbarrow Karel'. The genitive relation in English usually shows precisely the opposite order. Bloom reports that her subjects used the word order POSSESSOR-POSSESSION: *Kathryn sock, Wendy hair, baby raisin*, etc. It is quite possible that in Dutch the genitive relation is predominantly expressed by way of constructions with *van* 'of' and *heeft* 'has', while in English the *NP*'s+*NP* construction is more frequent. However, precise data on this are lacking.

Slobin (1970) is quite aware of the systematic differences in word order, but he apparently is not inclined to draw the empiricist conclusion from this. He writes,

> If you ignore word order, and read through transcriptions of two-word utterances in the various languages we have studied, the utterances read like direct translations of one another.

They agree with respect to the various stages of development (children never begin with sentences more than two words long, although they produce long sequences of babbling noises, and can link a few sentences together which have to do with a single thematic situation). The sentences are telegraphic, that is, they consist principally of content words rather than function words. They are always without inflection, articles, or copula. This is enough to make Slobin remark "what is remarkable at first glance is the uniformity in rate and pattern of development".

Concerning the possibility of training, another prediction made on the basis of empiricist suppositions, there are several incidental examples which indicate that in the first phase of language development the child is insensitive to explicit attempts at correction of his language (McNeill 1966; Braine 1971). Training programs appear to be effective at later stages, when the child is three or four years old. But at that point it is mainly a question of the transformational structure, and both rationalists and empiricists recognize the necessity of learning this. The only nontransformational study of receptivity to training is only loosely related to children's language acquisition; it dealt with adults' acquisition of simple artificial languages. We refer the reader to Miller (1967) and to the excellent survey by Smith and Braine (1972). The conclusion drawn by Miller is that it is nearly impossible to learn artificial languages without access to semantic information. Using a different method of experimentation, however, Smith and Braine come to the opposite conclusion. They showed which aspects of the meaningless language could be learned and which could not. An interesting result was that adults could learn an artificial pivot grammar of the form $S \rightarrow P_1 + O$, $S \rightarrow O + P_2$, $S \rightarrow O$ only to a certain extent. In fact, all they could learn was that P_1 elements always occur in the first position, P_2 elements always occur in the second position, and some words could also occur alone. Expressed in rules, this gives: $S \rightarrow P_1 + W$, $S \rightarrow W + P_2$, $S \rightarrow W$, where $W = P_1, P_2$, or O. The learning of positions is seen here to the extent that the first and the last positions are learned, but this does not lead to a complete differentiation of the three categories. If this is a general characteristic of the human inference mechanism, the two-word situation can be interpreted in the following way. Sentences spoken to small children contain frequently occurring words, some of which usually appear in the first position *(hi, look)*, while others usually appear in the last position *(on, off)*. The child will begin to suppose the grammar just mentioned, and not the pivot grammar. The fact that the child is then additionally able to differentiate class O, while the adult is not, might be the consequence of an a priori hypothesis, or of semantic knowledge: the child would simply know that *hi on* has no possible interpretation in his world.

The early two-word stage, to which we have limited the discussion in this paragraph, can, of course, give no decisive answer on the empiricist and rationalistic predictions concerning the transformational development. Nevertheless, before drawing up a balance sheet, we would first make a number of remarks on that subject. In itself, transformational development is a highly interesting field of research. Thanks to transformational grammar, aspect of language development which had never been studied before have become accessible to research. We refer the reader, for example, to the splendid work of Bellugi (1967) on the acquisition of the negative in English, and to publications by Menyuk (1969) and C. Chomsky (1969).

According to the rationalists, the universal base matures more or less without help. The acquisition of transformations, however, is highly dependent on the nature of the input, and because that which is learned comes later than that which is innate, it is predicted that the development of transformations will occur relatively late. This prediction agrees with the facts. But this is a spurious argument in favor of the rationalist position. By definition, a transformation is a relation between constituent structures. Their acquisition, therefore, supposes that those structures are present to start with, and thus stated, there is no empirical argument. But if we ask whether the result of the transformations is reflected in the first simple sentences of the child as the empiricists predict, the answer will simply be affirmative. If the child says *plat eendje* 'flat duckling' (Schaerlaekens), he is not speaking a "deep sentence", because adjectives are transformationally derived from propositions like *eendje is plat* 'duckling is flat'. The traces of transformations which can be found in the language of the adult can also be found in the first utterances of the child, completely in agreement with the empiricist point of view.

If we now consider the whole state of affairs, we must conclude that some aspects of the observation space do indeed have a specific influence on early syntax. A purely rationalistic point of view must be rejected. On the other hand, even from a purely syntactic point of view, there is remarkable agreement in the language development of children in different cultures and linguistic surroundings. This calls for some explanation. We have seen that the reasons for this may be sought in (i) the hypothesis space, (ii) the deductive information processing procedures, and in (iii) the observation space. Rationalists accentuate the first two, empiricists the latter two. Let us examine these possibilities.

The investigation of the hypothesis space, essential for the rationalist school, is seriously handicapped by the lack of independent insight into the structure of the base grammar. As we have seen in Volume II, Chapter 5, as long as an *Aspects*-type formalization is used, nearly every base will be universal for purely logical reasons. It is impossible to choose among such bases on empirical grounds. But until that universal base is well defined, it is useless to establish on it the explanation for the evidence of universals in children's languages. On the other hand, it is quite obvious that under these circumstances no empirical problem will be solved simply by defining the universal base in terms of the universals which are to be found in children's languages. In our opinion, therefore, in the near future rationalists cannot be expected to gain much credit on this point, although it is the only ground on which the nationalistic point of view could be verified and the empiricist rejected. This type of confrontation is not to be expected soon. However, it is not at all excluded that the empiricist stand will be verified and the rationalistic rejected as we shall see presently.

For both rationalists and empiricists the heuristic procedures would be an acceptable source of universals. They are McNeill's "weak universals". The future here is rather promising. Not only will it be possible for both camps to cooperate in research, but that research will be empirical in nature. It is quite possible, in effect, to collect data on the information processing capacities of the small child, and to do so by nonlinguistic means. One could then examine the way those processing procedures operate on linguistic input. A number of recent publications (Bever 1970a; 1970b; Slobin 1971b) deal with such process factors in language development. We shall return to this in the following section.

Research on the observation space, finally, is essential for the exclusive proof of the empiricist version of LAD as opposed to the rationalist one. The study reported by Ervin-Tripp (1971) on the language which mothers speak to their children is a start in this direction. Not only is such research possible; it is also of great interest to theory. The investigation of receptivity to training also falls into this category (on this point, see a number of studies by Brown, et al., in Brown 1970). It may seem to be unsatisfactory, however, to attempt to explain the universals in children's languages on the basis of the universals in the languages presented to them, for the question as to how those universals come into being in the first place remains open. Yet we cannot exclude that possibility of explantion. The problem is basically one of "the chicken and the egg", and there is no reason to seek the solution only in the egg.

4.4. PROCESS FACTORS IN LANGUAGE ACQUISITION

The argument that universal characteristics in the language development of the child proceed largely or completely from general characteristics of cognitive development is not new. It has always been an axiom of Piaget's school at Geneva; see, for example, the work of Sinclair-de Zwart (1967). But it is not easy to discover why certain phenomena which occur in language development must necessarily be the consequence of circumstances in the field of concept formation and the information processing capacities of the growing child. Such research concerning syntactic phenomena in the child's language is just beginning. Bever (1970a; 1970b) shows that some linguistic rules might be the natural consequence of general perceptual characteristics of the child. With evidence which is largely incidental, he shows that some perceptual strategies for language processing (cf. Chapter 3, section 3.5.) are essentially not specifically linguistic, but are also operative in the recording of other kinds of information. This latter involves an important obligation in this kind of research, for there is real danger that universal linguistic phenomena may simply be translated into general cognitive principles, without independent — that is, on a nonlinguistic basis — proof of the existence of such principles. This may be seen in much of the literature of Wundt's school of the psychology of language, above all in the work of van Ginneken (1904) who explained the word order used by children on the basis of the temporal order of images. This amounts to nothing other than terminological magic if this order is not determined independently. No proposition in the literature on the subject is completely immune to this objection, and the real research has still to begin.

Only for the sake of orientation, we shall now mention a number of attempts to account for linguistic universals by reference to general information processing capacities. Most of the examples have been borrowed from Slobin (1971b).

One of the first information processing capacities of the child is attention to the order of elements. A universal linguistic consequence of this would be that the earliest sentences would reflect the dominant word order in the language of the surroundings. In the preceding paragraph we showed that there is some evidence for this. But two points should not escape our attention. In the first place, the early capacity for the processing of order must still be proven on the basis of nonverbal tasks. Slobin simply supposes that this is the case. In the second place, the word "universal" refers to the child's way of reacting linguistically (for example, in maintaining word order), and not to the final grammatical form. This latter will in fact be *different* in various linguistic environments, in consequence of this principle. This comes close to the proposition that humans are universally different.

A better example is: the end of an information sequence receives primary attention.[1] This proposition must also be proven independently, and the notion of information sequence calls for further definition. The experiment by Smith and Braine in which a pivot-like artificial language was used (cf. section 4.3.) could be taken in this sense. They found that subjects rapidly discovered which elements could occur in the last position (but the first position was also marked). On the basis of this principle, Slobin predicts that suffixes and postpositions will universally be acquired earlier than prefixes and prepositions. There is some evidence for this, for example, the fact that French-speaking children, in learning the negation *ne...pas*, acquire *pas* earlier than *ne*. Slobin also gives further instances of this kind.

Other examples have to do with the tendency to avoid interruptions in information processing (thus, permutations which occur with some transformations are acquired at a late stage of development), with the tendency to avoid exceptions (so that after a short period weak forms are preferred to strong: *breaked, mans*), and

[1] Slobin's formulation of this is linguistic: "pay attention to the end of words".

with semantic processing principles, on which we shall have more to say in the following paragraph.

4.5. CONCEPTUAL FACTORS IN LANGUAGE ACQUISITION

With this subject, we definitively leave the domain of the LAD schema. Reasoning on the basis of the schema itself, we showed, in paragraph 2 of this chapter, that syntactic input alone is not sufficient for a realistic model of language acquisition. One can, of course, still try to stay as close to the schema as possible while adding a handful of semantic aspects to LAD's input, but such an approach would be harmful to the actual development of the study of language acquisition. Around 1970 a basic reorientation began to be observed in this field of research. A semantic point of view came to replace the common syntactic approach to the study of early language.

One of the first studies to appear from this view point was that of Bloom (1970). In the preceding paragraph we showed that McNeill's syntactic analysis of the two-word stage is essentially distributional, and we mentioned that Bloom proved that a given word order can go together with different semantic or functional relations. In that connection we cited her example of the ambiguous string *Mommy sock*. If the grammatical functions of subject, object, etc., are still to be defined configurationally as in *Aspects*, it will be necessary to develop a transformational grammar for the two-word stage, in which an ambiguous sentence such as *Mommy sock* can have two deep structures, both of which result transformationally in the same surface structure. This is essentially the method followed by Bloom. On the ground of semantic considerations, she categorizes the functional relations which are expressed in the two-word sentences. To account for this she writes a context-free base grammar in which those functions are defined configurationally. Terminal strings generated by the base grammar can contain more than two elements. A system of transformational rules, however, reduces the length to two words at most (for the two-word stage), and this results in the observed ambiguities. This is fine, but it remains a somewhat hesitant beginning for the semantic approach. What is new to Bloom's approach is her nondistributional semantic point of departure, which, if care is taken, can yield far better insight into the first sentences of children. But there is no need whatsoever to use a system of description for the functional relations thus found which was developed for the language of adults (and even then without being all too convincing); we are referring to the definition of functional relations in terms of the hierarchic relations among syntactic categories. For purely formal reasons, we would then have to suppose that at an early stage the child disposes of a differentiated system of grammatical categories (for Kathryn, one of Bloom's subjects, the two-word stage demanded ten such categories, as well as four nonterminal categories), and we would also have to assume that the sentences have abstract parsings, not expressed in the surface form. No independent empirical evidence is available for either of these suppositions; they are only artifacts of the grammar's formalization.

Schaerlaekens (1973) avoids these complications. She starts with the same semantic point of view, and tries, like Bloom, to discover which functional relations the child expresses in his two-word sentences. But she does not impose a transformational model which would raise more problems than it would solve. She only shows that many of those functional relations are accompanied by a characteristic word order, but that word order is defined in terms of the functional role which the two words play in the relation, and not in terms of abstract grammatical categories. If the functional roles were reversed, the words would change places correspondingly. Thus the bothersome problem of the multiple categorization of words is avoided. The most important relations which Schaerlaekens found in the two-word sentences of the six children she studied are the following:

(1) The RELATION OF FIXED ALLOCATION *(car Piet, book Arnold)*; in general this is a genitive relation *(the car of Piet, the book of Arnold)* with a fixed word order: possession-possessor.

(2) The RELATION OF COINCIDENCE, which is usually a locative relation *(Joost bed, mister bicycle)* has a fixed word order: object-place *(Joost is in bed, mister is on the bicycle)*.

(3) The SUBJECT-VERB RELATION is an actor-action or agentive relation *(Karel cries, father shaves)*; there is no fixed word order, but the *S-V* order appears to be dominant.

(4) The OBJECT-VERB RELATION is an objective relation *(airplane take, watch trashcan)*; there is a fixed word order for some children, *O-V*, but not for all.

(5) QUALIFICATION RELATIONS, such as DEIXIS *(look car, hears bim-bam)*, NEGATION *(no bed, no milk)*, PLACE RELATION *(there tower, here bus)*. Not all of these relations were observed for all the subjects, nor were they always clearly differentiated. Word order aspects are likewise precarious and complex.

Other infrequent and idiosyncratic relations were also observed, but we shall not discuss them here.

The point is that in all cases we are dealing with functional relations among concepts which may be expressed in a characteristic word order, but even this is not necessarily the case. This situation is analogous to that of the conceptual models of language users. In those models attempts were made to understand linguistic behavior as the communication of essentially nonlinguistic conceptual relations. In the present case, language development is to be explained in terms of the development of the conceptual basis. Slobin (1971b) writes:

> Is it possible, then, to trace out a universal course of linguistic development on the basis of what we know about the universal course of cognitive development? (Can one take Piaget as a handbook of psycholinguistic development?)

The analysis of the first words and sentences of the child must therefore be primarily oriented to the question of the intentions which the child wishes to express. The first words stand for objects and actions in the child's world, but there is no reason to suppose that this first subjective lexicon also contains information on categories. Grammatical categories will rather develop slowly but surely from the discovery that certain conceptual relations ordinarily accompany certain relative word positions in the language of adults.[1] The child will try to imitate this, and will eventually have the actor precede the action *(Karel cries)* regardless of the grammatical categories of the words in the language of adults (the same relation is expressed in a sentence such as *airplane by*, where *by* indicates the action, although it is not a verb). Only later will the child learn that not all words which can stand for actions may be placed in the second position in this relation. The words for which this does hold according to the language of adults will be specially marked as the "regular" ones. This is only an early stage of syntax acquisition. But it has already been preceded by rather extensive conceptual and verbal experience, according to this conceptual point of view. Language acquisition would then follow the sequence conceptual (concepts and relations) — semantic (words and their conceptual references) — syntactic (syntactic categories of words and rules of syntax).

As we wish also in this last paragraph to concentrate our attention on relations with formal grammars, we shall not discuss the actual content of this conceptual approach (we refer the reader especially to Campbell and Wales 1970; Donaldson 1970; and Luria 1961). In closing, we shall treat the only formalization known to us of the cognitive-semantic approach, to be found in Schlesinger (1971). According to Schlesinger, also for the child, the input of the speech mechanism is the intention. He calls its conceptual representation the *I*-MARKER. The output of the system is the sentence — at any rate the author limits himself to that. The *I*-marker is transformed into a sentence by means of REALIZATION RULES.

The *I*-marker is a relational network of objects and relations. Just as in Schank's model (cf. Chapter 3, section 3.6.2.), the number

[1] It is quite possible that at this first stage also inflection and accent will be used to mark conceptual relations.

of conceptual relations is limited; they principally include the conceptual case relations of agent (*Ag*), object (*Obj*), locative (*Loc*). There are also attributive relations (*Att*), determiner relations (*Det*), and operations such as negation and ostension.

Suppose that the speaker wishes to say that John catches the red ball. For the formal treatment we shall write concepts in upper case letters and words in lower case letters. The following relations exist among JOHN, CATCHES, THE, RED, and BALL (abstraction made of tense and number relations):

(i) *Att* (RED, BALL)
(ii) *Det* (THE, RED BALL)
(iii) *Obj* (THE, RED BALL CATCHES)
(iv) *Ag* (JOHN, CATCHES THE RED BALL)

According to Schlesinger, the formal structure is then

Ag(JOHN, [*Obj*([*Det*(THE, [*Att*(RED, BALL)])], CATCHES)])

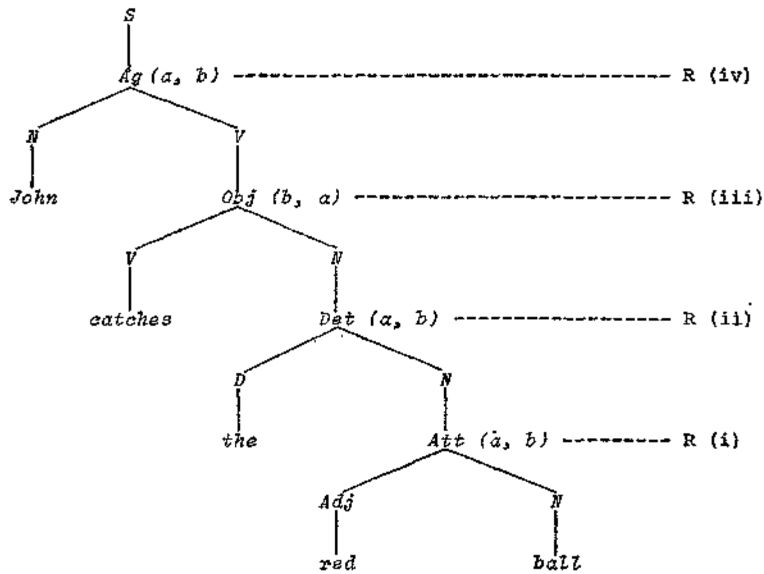
where the elements are not ordered from left to right, but only have a hierarchic organization.

The realization rules assign a place and a syntactic category to each element in the *I*-marker. This means that the *I*-marker is realized verbally by finding words or phrases of the category indicated and putting them into the correct order. As an example, Schlesinger gives the following realization rules for the relations shown above in (i) to (iv):

R(i) $Att(a, b) \rightarrow N(Adj_a + N_b)$
R(ii) $Det(a, b) \rightarrow N(D_a + N_b)$
R(iii) $Obj(a, b) \rightarrow V(V_b + N_a)$
R(iv) $Ag(a, b) \rightarrow S(N_a + V_b)$

R(i) means that the attributive relation between concepts a and b is expressed syntactically by finding an adjective word for a, followed by a substantive word for b. In other possible realization rules this sequence will then behave as a substantive or noun phrase. Suppose that the speech mechanism finds the word *red* for Adj

RED, the word *ball* for *N* BALL, and so forth. The resulting linguistic structure will then be:



The conceptual relations are thus marked in the surface (more or less as in Halliday's systemic grammar). Some of the realization rules bring about that which in *Aspects* is done by transformations, for example, the positioning of the adjective. But additional position rules will be needed for the realization of paraphrastic transformations. Schlesinger, however, does not work this out, because he wishes by way of the schema to focus attention on language development. Before going into that, we will make two remarks on the formalization up to this point. The *I*-markers show that which Chomsky calls a SET SYSTEM (cf. Volume II, Chapter 4, section 4.2.). They define the hierarchic relations among elements, but not their arrangement. Their formal structure was studied by Curry (1961), among others. Chomsky's objection to this is that such a system must be complemented by a set of ordering rules (Schlesinger's realization rules) and, in his opinion, one could quite as well begin directly with an ordered structure. But in Schlesinger's case the matter is obviously somewhat different. The *I*-marker is not a phrase marker, but a network of conceptual relations which is also based on arguments which are not linguistic in character. The same networks must serve as the input for nonlinguistic motor actions (the "hand" in Winograd's model), and they are the output of nonverbal input (the "eye" of that model). It would not be easy to find strong arguments for an ordering of concepts. It is, moreover, an old, but completely unproven proposition that word order reflects the sequence of thoughts (cf. Levelt 1967b). A set system can have its attractive sides as a conceptual representation, but the problem here is finally no different from the one discussed in Chapter 3, section 3.7., namely that of finding a suitable formal system for the representation of knowledge. In that regard, Schlesinger's proposals are significantly more limited than the procedural approach presented by Winograd. Schlesinger is also more limited insofar as he considers the sentence to be the maximum framework of expression for the *I*-marker; for Winograd, an intention can be expressed in a whole paragraph.

Our second remark concerns the aspects of the intention which are expressed linguistically. Schlesinger defines the *I*-marker as "the formalized representation of those of the speaker's intentions which are expressed in the linguistic output". This not only disregards everything which precedes the immediate input in the realization rules, especially the whole intelligent processing in the conceptual base, but it also excludes from further investigation all factors, such as limitations of memory or vocabulary, which cause some intentions which do constitute input for the speech mechanism not to result in linguistic output. Such factors, however, are important for the study of language acquisition.

Concerning the development of language in the child, Schlesinger states that the earliest realization rules which appear in the child's language are POSITION RULES, that is, rules which project these conceptual relations on a particular word order, just as was seen in Schaerlaekens' study. Syntactic categories play no role at this stage. Schlesinger mentions the following position rules among others. (The two concepts are given in the order to be realized. The examples are taken from naturalistic observations in which the context unambiguously revealed the intention.)

(1) actor + action	ex. *Bambi go, airplane by*
(2) action + object	ex. *pick glove*
(3) actor + object	ex. *Eve lunch*
(4) modifier + head	ex. *big boat*
(5) negation + X	ex. *no wash*
(6) ostension + X	ex. *here bed, it ball, that blue*
(7) X + locative	ex. *sat wall, baby room*

Although these rules are based on the linguistic corpora of English-speaking children, the same or similar position rules hold for the Dutch corpora studied by Schaerlaekens; differences in terminology should not lead us astray. It is indeed the case that, according to research on the two-word stage, not all position rules are functional. Even in later stages, some children exhibit a free word order. Gruber (1965) mentions such a case, and it is worth mentioning that Washoe, the Gardners' chimpanzee, never felt bound by position rules, although she seems to have expressed the same kind of relations as those involved in position rules (1) to (7). The child learns to express conceptual relations not only by means of position rules, but also by the use of special morphemes, such as inflections and prepositions (take, for example, the plural, possessive, and tense morphemes which appear quite early).

According to Schlesinger, only after having learned various position rules does the child learn the syntactic categories, the category rules. How does this happen? Little is known about this, but two principles follow directly from the conceptual theory of language acquisition:

(1) If a syntactic category has a conceptual corollary, that category is not acquired before the child is able to make the conceptual distinction. The category of possessive pronouns is not learned before the child can understand the genitive relation "belongs to". Tense categories appear after the development of the capacity

to distinguish time, and so forth. Although this all seems to be quite obvious, the principle is not a trivial one. In the first place, it does not follow from the LAD schema, according to which syntactic categories can first develop and only later be used for semantic expression. In the second place, it is not easy to prove in a nonlinguistic way that a given conceptual relation lies within the reach of the small child. How, for example, could one test the distinction between "count" and "mass" concepts? In the third place, it is seldom clear which conceptual distinctions are in question. Thus it is sometimes argued that substantives and verbs are distinguished conceptually as things and actions. Braine (1972) proves convincingly that this is a misinterpretation.

(2) A category is more rapidly acquired the better it is marked. Slobin (1971b) gives many interesting examples of this principle. One of them concerns a child who was raised bilingually to speak Hungarian and Serbian. In Hungarian, locatives are expressed in an orderly way, with a suffix added to the noun. In Serbian, on the other hand, locatives are expressed in the form of a relation between preposition and an inflection of the noun, and that inflection depends on the gender of the noun. The child learned the Hungarian locative categories long before he used the Serbian correctly. More generally, we can expect that the simpler the syntactic means is, the earlier the children will begin to express relations and concepts in correct syntax. The rather late acquisition of the future tense in Dutch and English can be better explained on the basis of the relatively complicated syntactic form than on the proposition that the notion of the future is acquired late in development. Before the child begins to speak, he already follows with his eyes the disappearance of an object behind a screen, and directs his attention to the point at which the object is expected to reappear; a primitive notion of the future is thus present already.

How far this conceptual point of view will lead remains an open question. For the present, we can expect important findings in research on early language. The more the language and the thought of the child become entwined, however, the more feedback will occur from syntax to conceptualization. Later the child will also become able to draw conclusions from the syntactic category of a word on the concept to which the word refers. Brown (1957) gives experimental examples of this. Apparently, the acquisition of concepts can in its turn be the consequence of syntactic categorization.

However this may be, the study of language acquisition, like that of the language user, will certainly be served by the development of suitable formal systems for the representation of knowledge. Here, too, the theory of formal languages is becoming increasingly important to the nonlinguistic aspects of theory.

HISTORICAL AND BIBLIOGRAPHICAL REMARKS

For a historical survey of psycholinguistics, see Blumenthal (1970). Some of Whorf's articles have been collected in Whorf (1956). Steinthal's linguistic psychology is published in his book *Grammatik, Logik, und Psychologie* (1855). Paul's principal work appeared in 1886. Wundt's linguistic theory may be found in the two volumes of his *Völkerpsychologie* (1900).

Intuitive linguistic judgment has been neglected as an object of research by both linguists and psychologists. Beside the literature mentioned in Chapter 2, the following sources on ungrammaticality should be noted: Coleman (1965), Marks (1965), and Seuren (1969). Bever (1970a; 1970b) is the only linguist to give thorough attention to the empirical aspects of the problem of intuition.

The psychology of grammar discussed in Chapter 3 is largely the work of George Miller and his collaborators. Both his fundamental contributions, such as the articles written in collaboration with Chomsky and published in the *Handbook of Mathematical Psychology* (1963), and his excellent popular writings made this type of psycholinguistics dominant in the 1960's. A complete survey of the developments in this field until 1966 may be found in Levelt (1966); the developments from 1966 to 1971 are covered in Fillenbaum (1971). It would be incorrect to continue to identify Miller with this tradition; since 1965 he has been almost exclusively concerned with semantic problems. Much information on the early conceptual or semantic models can be found in Minsky (1968). For our chapter we have borrowed much from Frijda (1972), Schank (1972), and Winograd (1972). We refer the reader also to Tulving and Donaldson (1972). More information on pattern grammars can be found in Rosenfeld (1969), Grasselli (1969), Lipkin and Rosenfeld (1970), and the two special editions of *Pattern Recognition* — 3: 4 (1971); and 4: 1 (1972).

It is no accident that the LAD schema corresponds well to the theory of grammatical inference. Both originated in Chomsky's work. But it is surprising that the two lines of reasearch lost contact with each other at such an early stage of development. This was neither to the advantage of the study of language acquisition, where mathematical precision tended to be replaced by dogmatism, nor to the advantage of the study of artificial intelligence, where the possibility of application of the findings to psychology was considerably limited. The terms "empiricist" and "rationalistic" have a much wider meaning than the technically limited use in Chapter 4. Chomsky's broader use of the term may be found, for example, in Chomsky (1968). The chapter is model oriented, and limited to the formal grammar problem. Consequently it should not be consulted as a general survey of the literature on the subject. A rather complete bibliography was composed by Appel (1971). Likewise Chapter 4 does not give a complete treatment of the learning of artificial languages. The first studies on the subject were those of Esper (1925). Miller's *Grammarama* project was a heroic but not very successful undertaking in this field (cf. Miller 1967). An evidently more fruitful approach is that of Smith and Braine; a good survey of literature may be found in Smith and Braine (1972).

Since the writing of this text much progress has been made in the study of mother/child conversation, cf. Broen (1972), Holzman (1972), Phillips (1973), Sachs et al (1972), Snow (1972). Van der Geest (1974), and other papers. They all give considerable support to the "intelligent text presentation" model, and add importantly to our knowledge about the role semantic input plays in the acquisition of language.

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