In T.R. Dixon & D.L. Horton (Eds.), Verbal Behavior and Genral Behavior Theory. Engelwood Cliffs, NJ: Prentice-Hall, Inc., 1968.

Associations to Stimulus-Response Theories of Language[1]

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I will emphasize four points in the course of this discussion. First, "Linguistic Theory" is a psychological theory: it accounts for a set of regular psychological intuitions about sentences. Second, stimulus-response associationism cannot account for the structure of language or language behavior: various attempts to offer S-R models as theories for *part* of language structure are not only pointless in principle but have been fruitless in fact. Third, we cannot hope to learn to understand verbal behavior by studying experimental manipulation of words: sentences are not special cases of word sequences; on the contrary, words are special cases of sentences. Finally, the reviews of recent psycholinguistic investigations demonstrate how little we understand the principles of the interaction between language structures and behavior.

What is a Linguist?

Today's papers have drawn clearly the distinction between the formal analysis of language and the psychological reflection of that analysis in actual speech behavior. I am reluctant to add even more smoke to an already congested problem, but some discussion is necessary to understand the different aspects of language study.

I would like to show you that there is no methodological conflict between linguistics and psychology since *both* approaches to language utilize two distinct types of facts: the structure which underlies behavior and the actual implementation of that structure. Certainly psychologists must include such structural notions as *word*, *sentence*, and *ambiguity* in their descriptions. On the other hand, it has often seemed as though linguists ignore all psychological facts in favor of so-called "linguistic facts" about language and that linguists are therefore concerned only with "structure" and not with "behavior." But linguistic facts *are* psychological since they are themselves behavioral intuitions. Consider five different "levels" of language which linguistic theories have described separately: sounds, words, phrases, sentences, and meanings. Each of these linguistic levels is differentiated intuitively from the others and has a particular kind of intuitively discovered

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unit. For example, we "know" that *cats*, *stack*, *cast*, *task*, *tacks*, *scat* all make use of the same *sounds*, independent of their differences in meaning and word-class. We "know" that *look out* and *outlook* use the same words, that *the nice boy likes the nice girl* and *the nice girl likes the nice boy* use the same phrases; that *the boy, who is nice, likes the girl* and *the boy is nice and likes the girl* are made up of the same sentences; that *the man is a bachelor* and *the man has never been married* share the same semantic interpretation. Each of these different kinds of "linguistic facts" have been distinct in linguistic theories (although not all theories attempt to find a grammatical explanation for every level). What I wish to emphasize here is that linguists have fragmented language into different kinds of descriptive problems and that each problem is differentiated *intuitively* from the others.

It is not just the descriptive levels which are intuitively differentiated. Recent advances in linguistic analysis have depended on an intuitive limitation of the kinds of facts at each level which are to be explained by a theory of language. Linguists intuitively separate those facts which are pertinent to the grammatical theory of language ("langue," "competence") from those pertinent to the use of language ("parole," "performance") by actual speakers[2]. In this way they mercifully protect themselves from many of the caprices imposed on language structure by human performance.

The basic intuitive distinction formally represented in modern linguistic research is the separation of sequences into sentences and "nonsentences": an adequate grammar of a language is *by definition* the grammar which produces the sentences of that language and does not produce any nonsentences. Linguistic theory must claim that the sentences of the natural language and those produced by the linguistic grammar are identical. *By assumption* the natural domain of linguistic theory is the set of all *intuitively* well-formed sentences: any systematic facts about speech which are outside that domain are *by assumption* part of the "performance" of the language.

The natural domain of linguistic theory might have been defined to include only more restricted kinds of facts. For instance, the natural unit to be explained by linguistic theory might be defined as the *phrase*. In this case any systematic analysis outside the phrase would not be included within the theory itself. The natural domain of linguistic theory has sometimes been even more restricted. Indeed, de Saussure, the first modern proponent of the distinction between the structure of language and the use of language, held that the essential linguistic unit is the *word* and that the formation of phrases and sentences is outside of the theory of the language.

There is, of course, no formal way of deciding which decision is most appropriate. In general, as we discover new types of theories for the formal analysis of language, our goals for the theory can expand. Thus one could view the "langue" theory of the *Cours*[3] as an explication of the phonological and semantic nature of words and the precise character of similarities and differences among words. One could further view the structuralist taxonomic phrase-structure approach of recent American theory (see Bloomfield, 1933) as a way of explaining the nature of

[2] For discussion of this distinction in the study of syntax and semantics see de Saussure (1916); Chomsky (1957, 1965); Fodor and Katz (1962); Bever, Fodor, and Weksel (1965).

[3] de Saussure (1916)

word-types and relations among them in phrases, as well as offering a precise characterization of similarities and differences among phrases. Similarly, the recent transformational developments can be viewed as a way of explaining the nature of phrases and relations among them in sentences. as well as formally explicating the characteristics of the similarities and differences among sentences themselves.

I have given you this brief review of the course of the domain of linguistic description to illustrate that there is no clear point where linguistic theory stops and psychological theory begins. There are, however, many arbitrary lines which have been drawn by linguists in order to partition off the kinds of facts about language which they felt prepared to describe. In each case there has been nothing formal *or* mysterious about the division of facts about language; the transformational grammarian appeals to an intuition shared by most of us about our language when he claims that he will consider only facts which pertain to complete sentences. We all agree roughly on what a sentence is, and no doubt we could define psychological tests which will distill most sentences most of the time. However, the fact that there are no generally used experimental procedures for isolating sentences does not mean that the distinction between sentences and nonsentences is psychologically irrelevant or invalid. On the contrary, in many cases the agreement on what is (or is not) a sentence is larger than for most behavioral distinctions. Even if the agreement were much weaker than it is, the point would remain the same—the linguist uses an introspective behavioral criterion to choose among his intuitions about the language he is studying.

Before discussing further the nature of this criterion, consider some of the intuitions which modern grammarians use to decide what data about sentences are relevant for description. The most important is *sentencehood* itself. Any sequence which meets the intuitive criterion of sentencehood must be included in the linguistic description. For example, the linguist assumes that (a) is a sentence which he must describe and that (b) is not:

(a) George is a nice boy.
(b) Boy is a nice George.

Another intuition is that of multiple interpretation (grammatical ambiguity). A grammar must give the sentences in (c) alternate grammatical structures.

(c) (1) The patient will inherit everything.
(2) They gave her dog candies.
(3) The duck is ready to eat.

Finally, the several levels of structural relations among words in sentences must be described. For instance, at the surface phrase structure level in (d) "dog" is more closely bound to "candies" than in (e).

(d) They gave him dog candies.
(e) They gave his dog candies.

Similarly at the deep phrase structure level the relation between "duck" and "eat" is quite different in (f) and (g).

(f) The duck is anxious to eat.
(g) The duck is tasty to eat.

I have gone through these familiar kinds of examples because I want to re-emphasize that there is nothing formal in the decisions which linguists make about their data. In fact, from a formal point of view the whole process is tremendously tenuous and fortuitous. The linguist must make various assumptions about the way in which his own intuitions reflect the knowledge of his language: his intuitions must be dependable, and they must be qualitatively invariant over different kinds of sentences. These assumptions are simple and certainly appear to be well founded, but they should not be considered inviolate. It is quite conceivable that the psychological reflection of grammar is inconsistent with respect to the sentencehood of individual sequences.

Such an inconsistency has been found in the case of sentences with center-embedded clauses. *Insecticides exterminators professors recommend manufacture fumigate apartments* is phenomenologically absurd as an example of English. General linguistic constraints require, however, that a grammar generate it (i.e., in order to be able to generate sentences with *one* embedding, e.g., *Insecticides exterminators manufacture fumigate apartments*, the grammar must allow *any* number of center-embeddings). Thus the linguist claims that sentences with two embeddings may *seem* ungrammatical, but only as a psychological illusion; it is a case for which the usual intuitions about sentencehood cannot be trusted. Ultimately, the linguist is making a very strong psychological claim: the phenomenological unacceptability of two-center-embeddings is not related to the structure of the language while the unacceptability of sentences like (b) is dependent on the language structure.[4]

[4] Another example of this occurs with verb-particle separation. Surely we can say (1) *He called the girl up*, but what about (2) *He called the not very nice or attractive young girl whom you like anyway up*? There seems to be some kind of length restriction on the object noun-phrase which can occur between the verb and particle. At the moment there is no satisfactory structural explanation for this; that is, in order to be able to generate acceptable sequences like (1), the theory must also generate sequences like (2).

Often the linguist uses extremely subtle criteria in his decisions about intuitions which he himself cannot make explicit. Who can say that (h) is grammatical but (i) is not?

(h) Personally, I don't disagree with a word he didn't ask you not to say.
(i) The whole group are here.

or which of the sentences (j)—(m) is ambiguous and which is not?

(j) John will make a wonderful storyteller.
(k) John will make a wonderful statue.
(l) John will make a wonderful rug.
(m) John will make a wonderful microscope.

or whether *the troops* phrase is more closely related to the preceding verb in (n) than in (o)?

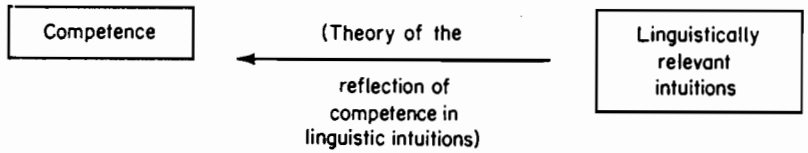
(n) The general defied *the troops* to fight.
(o) The general desired *the troops* to fight.

In each of these cases it is possible to make fairly consistent decisions, and this is what linguists in fact do. I do not doubt the self-consistency that a person can achieve in these decisions, but I doubt very much that he can tell me what he's doing.

In brief, a linguist utilizes the strategy diagramed below—he assumes that a speaker has a knowledge of his grammar and that some of the structural distinctions in the grammar are *consistently* reflected in his intuition about sentencehood, structural relations, ambiguities, and so on. He uses these consistent reflections in behavior to decide what data about the language he must describe (Diagram 1).

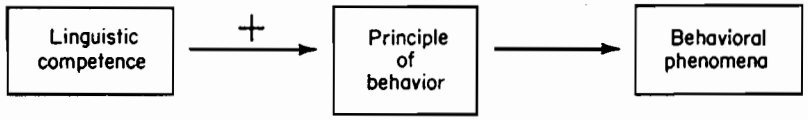
The data are themselves enumerated by a *grammar* which meets the general requirements of all grammars.

(1)



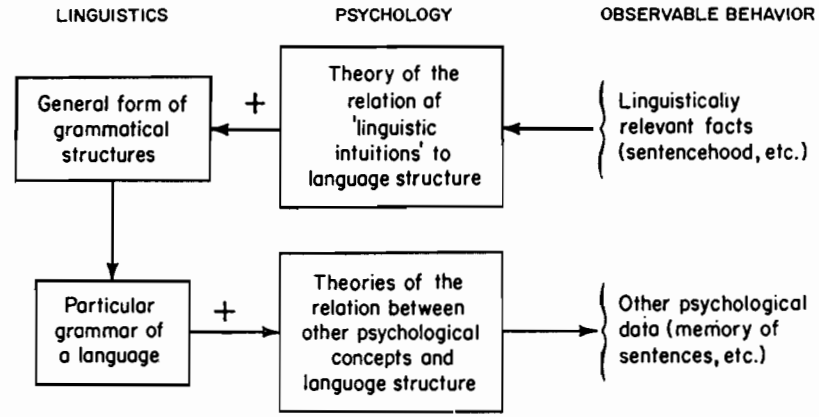
In contrast a "psycholinguist" who is concerned with generative grammar is the complement of the corresponding linguist. The linguist assumes that the psychological reflection of structure is stable and investigates the formal structure. The psycholinguist assumes that the formal structure is correct, and he studies its psychological ramifications by combining the grammar of a language with some general principle of behavior to predict new facts about language behavior (Diagram 2).

(2)



This strategy uses the same assumptions about the relation between grammar and psychology as the linguist, but in the reverse direction. The linguist assumes that his intuitions are consistent with respect to the grammar, and he combines them with his assumptions about the general form of grammars to produce a particular analysis. The psychologist takes this analysis in conjunction with a particular behavioral theory to predict new data. In combination the entire scientific process has the form of Diagram 3.

(3)



I perform this exercise in taxonomy not because it is explanatory but because it is instructive. In both linguistics and psychology we can observe that the most uncertain link is the particular psychological theory of how structure is implemented in actual behavior. There are many psychological concepts which we would like to integrate with grammatical structure: long-term memory, short-term memory, forgetting, attention, perceptual constancy, recognition, comprehension, the learning of structure, the neurological reflection of structure. The problem is that our general psychological theories of how structure is implemented in behavior are miserable. We do not even really understand the psychological nature of the procedures that linguists use to isolate facts relevant for linguistic description. Thus I hardly think that we are in any position to do crucial experimentation on the psychological "reality of transformational grammar" any more than the "physical reality of the parallelogram of forces" could be tested without an understanding of how to avoid (or compensate for) friction effects. In fact, the grammar itself seems to be the most secure element in our research strategy since it does account for a large set of consistent intuitions. This implies that psycholinguistic experimentation should tell us at least as much about general rules for implementing structure in behavior as it does about the structure of languages itself.

I should emphasize that despite the difficulties with certain data, the transformational theory of language is supported just by the clear psychological data which the theory does account for. Intuitions about sentencehood, ambiguity, and structural relations are psychological data of the most diverse kind. A theory which accounts for them is thus a *psychological theory* of the first order.

This bipartite strategy is common in the theoretical treatment of natural phenomena. To quote an example we have given before[5]:

> It is not at all surprising that the analysis of speech behavior should proceed from two empirical and theoretical sources. Indeed, distinguishing among the different kinds of data that constitute superficially homogeneous phenomena is absolutely universal in scientific explanations; it occurs whenever considerations of simplicity and explanatory power require that the observations be represented as interaction effects. Consider, for example, the analysis of a block sliding down an inclined plane. There are two kinds of variables that interact to determine the block's behavior—first, the forces acting downward on the body and determining the acceleration for an ideal system; second, the reactive forces (e.g., friction) due to the character of the particular body and plane under study. The observed behavior is susceptible of systematic explanation only on the view that it is the product of interactions between these distinct systems.

I think that it is a universal scientific strategy to approach scientific description in this manner. The value in it is quite obvious—it allows us to describe natural behavior in terms of structure which exists as a constant component of many superficially distinct behaviors. What else is scientific study for, except to extract general constant principles from heterogeneous phenomena?

In the above example the points of contact between the theory (parallelogram of forces) and regular phenomena is sufficiently rich to make unreasonable a rejection of the theory. We can use the same essential concepts for the description of bodies on inclined planes, falling bodies, ballistic paths, strain, and so on. In each case the essential concepts are systematically reflected in slightly differing ways but always remain intact. Unfortunately, the number of different points of contact between modern linguistic theory and natural speech phenomena has not been much richer than the basic intuitions of linguists which I reviewed above. This is largely because we have no general theory of how any structural theory of behavior should interact with psychological phenomena. Later in this paper I shall summarize the different empirical loci of interactions between transformational grammar (TG) and natural phenomena. I shall try to con-

[5] See Bever, Fodor, and Weksel (1965).

vince you that there is a sufficient number of them to recommend acceptance of TG and to expand our understanding of how behavior in general reflects its own structure.

S-R Theory and This Strategy

It is clear that the usual formulations of S-R theory cannot possibly interact with the bipartite strategy of research which I have outlined. The reason for this is quite simple: S-R theory would attempt to account for both components of language behavior without distinguishing them. With or without mediating concepts (as Fodor has shown), S-R models appear to cover the whole of language behavior. They are proposed simultaneously as accounts of the structure of language and the reflection of that structure in actual behavior (notably in learning). The various reasons why this global approach cannot work are summarized in McNeill's paper and elsewhere, and I do not wish to repeat them here.

We can ask next if S-R theory is compatible with either the structure of grammatical competence or the psychological theory of performance. It takes only a moment to see that S-R principles cannot adequately enumerate the structure of sentences. This issue has also been discussed in detail, and I shall restrict myself to a recent attempt at this conference and elsewhere to resuscitate S-R models.

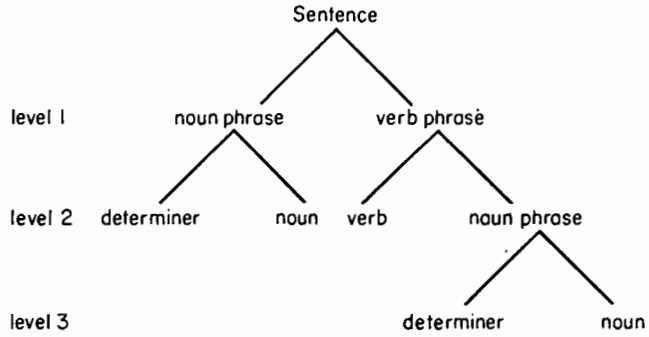
In summarizing our interchange with Braine, McNeill correctly analyzes Braine's work as an attempt to integrate the theory of linguistic competence with associationistic principles, by ignoring all aspects of linguistic theory that associationism cannot handle. Braine and McNeill agree that simple associationism cannot handle the learning of transformations; Braine concludes that there is something wrong with transformations, while we and McNeill conclude that there is something wrong with simple associationism. Braine, however, disagrees with our claim that associationism cannot even account for the structure of simple context-free grammar (CFG). In addition, Osgood has recently proposed a similar way of viewing the structure of language so that associationistic principles can allegedly account for it. The goal of these enterprises seems to be to save at least some part of linguistic theory for S-R explanation. Even if transformational grammar as a whole cannot be accomodated by S-R approaches, the implication of these efforts is that something will be gained if part of it can be.

The claim that associationistic theory can account for the structure of CFG appears important since, if true, it would offer a stronghold for the S-R treatment of limited parts of language behavior. Braine, for example, is perfectly happy to restrict his theory to an account of the learning of a nontransformational grammar in young children and let others explain the learning of transformations. The essential mechanism is that of "word class and substitution"; by learning position classes for individual words and type of allowable orders of classes, a child can develop a small "phrase structure" system which could provide the basis for his later language development.

The problem with this idea is that the only kind of CFG that could be learned in this way is one with a finite number of allowable class orders, that is, a grammar *entirely without recursion* and therefore inadequate to natural language. A general CFG is one in which there are ordered phrase structure expansion rules of the type:

(1) Sentence ⟶ noun phrase, verb phrase

(4)



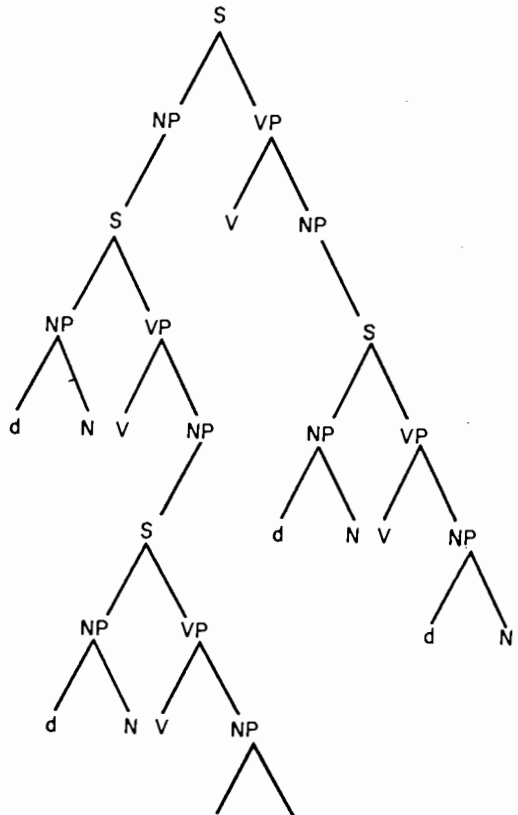
(2) Verb phrase ⟶ verb, noun phrase
(3) Noun phrase ⟶ determiner, noun

which enumerate tree structures with definite levels (Diagram 4).

A phrase structure grammar with recursion is one in which a given constituent (e.g., "sentence") can dominate itself in the tree and the rules do not apply in any particular order, e.g., a grammar like the above with this additional rule:

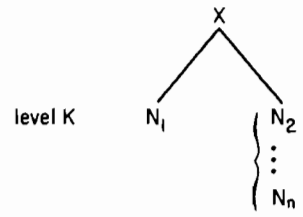
(4) Noun phrase ⟶ sentence

This grammar (rules 1–4) enumerates phrase structure trees like:



It is inherent to recursive CFG that the sentences it produces are of arbitrary length. Yet it is just this that Braine cannot allow, since substitution and word-class grammars require an upper bound on the allowable word-class sequences.

In a related attempt to integrate CFG and the descriptive principle of transitional probability, Osgood has proposed that we can measure not only the transitional probabilities between words but also those between constituents and that this complex of hierarchical probabilities can represent the structure of sentences. Thus in the first tree above we can determine at level 1 the probability that "nounphrase" precedes "verbphrase" in a corpus of sentences. In this way behavioral effects due to the sequences of groupings of words can be treated in the same way as the effects of individual word sequences. Again recursion in phrase structure is incompatible with this proposal. If the grammar generates structures like those in the second tree above, coherent phrase structure levels are not available. Regular statements about the sequential probabilities of tree structure nodes *presuppose a way of deciding whether or not any pair of nodes are at the same phrase structure level.* In phrase structure without recursion, levels are specifiable, and at a particular level "K" a node, "N_1", is followed by a restricted number of nodes "$N_{2....n}$" each with a particular probability.



In this system a grammar consists in a hierarchically ordered set of Markov sources, each set at a particular level.[6] Suppose now that the phrase structure system were recursive. It is impossible to define a concept of phrase structure level which relates to the grammar: there is an arbitrary number of levels, and *any* sequence can now appear at any level (see Chomsky, 1957, p. 35ff). Of course, many arbitrary relations between pairs of nodes can be defined, and then the probability of any particular pair satisfying that relation can be calculated. This might in some cases be an interesting set of statistical statements about a particular corpus. But in no case could it be used as a basis for a system which could *generate* tree structures. Such a system could generate all the existing tree structures only if the notion of phrase structure level is characterizable and there is a finite number of them.

Of course, the fact that Braine's and Osgood's formulations are not integrated with transformations and the kinds of facts which transformations describe is sufficient for their rejection as viable theories of language structure. But these formulations are not even able to represent the weak phrase structure system that could account for a language with an arbitrary number of dependent clauses in a sentence (e.g., I like the girl who hit the man who left the store . . .). In any case, it has been shown that S-R theory and related associationistic descriptive techniques are inadequate to account for the structure of language or of transformational grammar as a whole. I think that the redoubt of trying to show that it can account for

[6] I am not sure that anybody knows what the formal properties of this system would be, but I am trying to capture the systematic nature of Osgood's proposals. For a discussion of the incompatibility of associationism and recursion see Bever, Fodor, and Garrett in this volume.

part of the grammar is extremely uninteresting. The grammatical theory itself is not divisible; transformations are defined to operate on particular trees, and phrase structure rules are formulated to provide just the necessary tree structures for transformations: the form of one depends on the form of the other. Any isolated component of transformational grammar cannot account for the structure of natural language, and any theory of language structure which proposes to account for one component as though the others did not exist is futile.

S-R theory is severely limited as a basis for a linguistic theory. We now can ask whether it could represent a possible account for the performance or "psychology" of the structure of language.

There are two ways of answering this question. In the first place (as Fodor and Garrett emphasize), S-R principles cannot account for the "linguistic" structure of language as expressed in the formalisms of transformational grammar. I have already argued that linguistic analysis is itself an explanation for *psychological* data, so S-R theories fail on psychological grounds if they fail on linguistic grounds.

The second answer is that S-R theories have no bearing on most psychological research into the manipulation of *sentences*. I can see no form of legerdemain that reconciles the findings of Miller and his students with the principles of associationism. Perhaps a connection can be made for particular experiments. S-R theories are primarily learning theories, yet they cannot even account for the learning of language (as McNeill reviews); I see little point in arguments on their behalf for other kinds of language behavior.

What Is the Role of Probability in Language Behavior?

I have reviewed the facts which show that S-R theory is neither a general theory of language behavior nor a general theory of language structure nor a general theory of the performance of that structure. Clearly the past few days have demonstrated phenomena dependent on contextual probability of occurrence. What then is the theoretical status of the many phenomena correlated with probability of occurrence in the behavioral manipulation of words and sentences?

In brief, probability effects in language behavior are due to constraints inherent to the performances of language structure. Thus probability is an effect, not a cause: if a speaker follows certain statistical regularities, we cannot infer that those statistics directly reflect the structure of his language *or* the structure of his performance of the language. Suppose it is the case that nouns occur with a probability of k and verbs of n in natural discourse. Can we infer that it is a rule of language or speech behavior to produce these probabilities? No. Can we assume that this is a part of the story? No. Language structure is not an array of nouns and verbs, and certainly the principles for the performance of that structure are not expressed in terms of nouns and verbs. According to the strategy for language study which I have outlined, every probabilistic fact is an empirical fact, not an answer or explanation.

Suppose passive sentences are said more frequently than active sentences. No doubt this may reflect the relative psychological complexity of performing

the passive rules.[7] We take this as a *problem* in need of an explanation, not a theoretical claim about the language. For example, we might suggest that the basis for this fact is that the grammatical derivation of passive sentences involves more transformations than for active sentences. In effect, experimental and natural probabilities are the only measures of psychological work on units which we can observe. I do not see this as a scientific virtue, and I find wholly irrational the assumption that since that's all we can see directly, that's all there is.

Yet it is true that words in sentences seem to constrain each other. In (p) the word "hill" fills in the blank more often than the word "water."

(p) John will run up the _____.

We might then postulate that certain features which affect sequential probabilities in sentences are the left-right probabilities of adjacent words. Johnson has effectively argued that it is the left-right probabilities of constituents. I think, however, that if these constraints are part of the story at all, they are needlessly superficial. Consider the constraint between "duck" and "eat" in the two sentences below:

the duck is tasty to eat
the duck is eager to eat

Clearly the probability of occurrence of *duck* is dependent on the logical relation which it bears to *eat* (and vice versa). These relations are, of course, specified in TG in the base structure of sentences. Thus statements about the probability of any two words appearing in the same sentence are simplified by reference to the deep structure. Consider the associative probability of *John* and *hill* in the sentence above; I think it is the same as in these sentences:

> It is up the hill that John will run.
> The hill will be run up by John.
> John will be the one to run up the hill.

There are, in fact, an enormous variety of sentences, all stemming from the same deep structure and, I believe, all with the same basic associative probability between *John* and *run*. With this kind of example in mind, I propose the following principle: *the associative probability of words in actual sentences is a direct function of the associative probability in their underlying structures.*

This principle is not surprising if one remembers that underlying structures are related to the meanings of sentences by a simple function, while the structure of the actual form of sentences is much more removed from the sentence meaning. Johnson's point in his paper that word sequences of more than 5th order probability are treated psychologically like real language supports this principle. If the relevant unit of associative probability were actually the surface structure constituent (as Johnson seems to believe), then sequences with 3rd order probability should be language-like since they contain many well-formed phrases. Fifth-order approximations to English may contain a number of whole clauses and sentences, that is, units which are coherently related to real sentences with associated deep structures.

I do not think that anyone should be startled by the proposal that associative probabilities are more easily related to deep structures. I mention it because these structures are themselves abstract. If we count probabilistic statements in terms of them, then we must reject any claim that the probability effects in language behavior are *directly* observable in the properties of actual language sequences. Since S-R approaches are *in principle* dependent on the observable sequences, S-R theory is even incompatible with the language phenomenon it ought to handle best—associative probabilities of words.

Psycholinguistics in the Child and Adult

Language learning and the psycholinguistic structure of the child

McNeill has pointed out how amazing it is that the child masters his language by the age of four and yet has not mastered many other cognitive functions. In fact, it is so amazing that I do not believe it, in its simplest form (just consider the well-known fact that the child shifts from syntagmatic to paradigmatic word associations much later, at age 6 or 7). It is one thing to say that a child's competence is characterized by a transformational grammar; it is quite another to assume that *ipso facto* the child has the same psychological implementation of transformational grammar as the adult. Obviously the child must be endowed with the capacity to develop deep and surface sentence structure, but how he does it and in what order are open questions.

I have no direct counterevidence to the assumption that the child has the *full* adult psychological structure underlying language by the time he goes to school. But there are indications in the ways children use language that they initially are more sensitive to the superficial structure. For example, if we examine the progression of how children play with language and the jokes they understand, we can see that at first the child uses a superficial level. At the age of five, the child is delighted with the riddle, "Why can't you starve on the desert?" (because of the sandwiches there) and at the age of ten, with the riddle, "Why can you jump higher than the Empire State Building?" (because it can't jump). Thus, as the child grows older, his language reveals increasing sensitivity to deep structure.

Superficially the child appears to deal with the world as adults do, but it can be shown that his cognitive approach is actually quite different. It seems reasonable that he could also appear to talk like an adult and actually have quite a different psychological basis for the same grammar. This suggestion bears *not* on the child's eventual competence (which I assume to be TG) but on the structure of his performance, and therefore the suggestion is quite testable. If I am right, a child of four appears to have mastered his language but will react differently from adults in certain experimental situations. Experiments with comprehension and memory for transformations in children have been inconclusive because the transformations used also lead to increased surface structure complexity or increased length. However, other experiments may bring out differential effects and show that the child is psychologically dealing with surface structure in situations where adults use deep structure. I have in mind a series of experiments which I have run

[7] Of course, the way in which psychological complexity is reflected in frequency of occurrence is itself a general phenomenon. In experimental studies the usual technique has been to assume that the probability of experimental success with a particular stimulus (e.g., on a long-term memory task) is inversely related to the number of psychological units which the stimulus evokes (the so-called null-hypothesis). Indeed the demonstration that a random interaction of hypothetical units is matched by the actual probability of experimental success is often taken as support for the psychological reality of those units (see Mehler, 1963).

with Donald MacKay and Jacques Mehler on the way various different types of ambiguous sentences are treated by adults. In certain tasks lexical ambiguities (The sailors like the port) and surface structure ambiguities (They fed her dog biscuits) are more difficult than deep structure ambiguities (The duck is ready to eat); in other tasks they are easier. With Mehler I plan to use children in some of the same experiments. If I am right, the behavioral ordering of these different kinds of ambiguities should yield different results in children than in adults.

Modern psycholinguistics in the adult

For the past six to seven years, psychologists have made various kinds of attempts to integrate the developments in linguistic theory with other kinds of behavior. The problem with most of these experiments is that they were in large part devoted to the confirmation of the "psychological reality of TG." This, of course, was due to the force of the original artificial distinction between "competence" and "performance," in which it was an issue to "prove" that the grammar is "real." Our problem now is somewhat different —to determine by what mechanisms sentences are understood, remembered, recognized, and so on.

When we look at the problem in this way, we discover that we don't know anything at all. It is easy to show that phonological, surface, and deep structures of sentences have psychological reality (other than that inherent to the linguistic intuitions themselves). But the problem of exactly how these structures interact in actual psychological processes such as perception, short-term memory, and so on, is bewildering. Let me now review some of the relevant findings discussed today and from our own research. In each case I have inklings of some general principles that are operating; but even if they are true, they are depressingly far from the correct story.

PHONOLOGY. Issues in the perceptual segmentation of the speech signal have paralleled recent theoretical linguistic controversies. Within the linguistic analysis of phonology it has been shown that the relation between the abstract and phonetic speech segments do not maintain the behaviorist principles that a distinction of the abstract level is *directly* reflected phonetically in the same segment (see Chomsky, 1966). For instance, in the words "medal, mettle" the only phonetic difference is in the length of the vowel for many speakers, although the "t" and "d" are kept distinct at the abstract level. Garrett and Fodor point out that in phonetic perception itself it now appears that the principle of linearity between psychological levels is also not maintained between the phonetic interpretation of a segment of sound and the actual *acoustic* properties of that segment. For example, the acoustic structure of the "t" in "stew" is identical with that of the "k" in "ski," yet they are perceived as phonetically distinct. This kind of finding (largely due to Haskins Laboratory) is particularly important since the phonetic-acoustic level appeared to be the last reasonable hope for the application of behaviorist principles to the structure of language.

SURFACE STRUCTURE. Within linguistic theory certain syntactic and phonological rules must apply recursively; that is, they reoperate on the output of their own previous operations.[8] Each time the rules reapply, it is to a larger constituent in the phrase structure of sentences. Several

[8] See Chomsky and Halle, forthcoming.

psychological phenomena reflect this linguistic cycle. In his experiments on transitional error probability with prelearning of isolated sequences, Johnson found that the more deeply embedded a word pair is, the more it profits from pretraining. This is to be expected if the memory unit is the linguistically defined constituent and the memory process is defined recursively over successively larger constituents—the more deeply embedded the constituent, the more times the memory process applies to it. Recently Jacques Mehler, Peter Carey, and I have found a striking example of this psychological cycle in the eye-movements of subjects reading sentences. More deeply embedded structures received more eye fixations. Garrett and Fodor review other effects of this sort in the perception of pauses and the placement of clicks in sentences; the more deeply embedded a structure is, the more perceptual coherence it has.

From these data I suggest that there is a general *psychological cycle* defined over surface phrase structures; in linguistic analyses these cycles acquire rules peculiar to a given language; in other psychological phenomena the kind of rules depends on the kind of behavior. But the principle is the same —the psychological rules reapply to themselves at each successively larger constituent.

DEEP STRUCTURE AND TRANSFORMATIONS. Garrett and Fodor review the recent work of Miller, *et al.*, which shows that for certain transformations (Passive, Negative, Question) the number of transformations correlates with experimentally derived measures of psychological complexity. These results are important because they indicate that there may be a direct mapping of TG onto more kinds of behavior than producing linguistic intuitions: that is, for memory we would like to be able to claim "one linguistic operation equals one psychological operation in memory." It is understandable therefore that Garrett and Fodor then lament the lack of a simple correlation of the number of grammatical rules with difficulty in the perception and memory of sentences for all types of transformations. After all, they reason, if grammar is reflected in behavior, why not assume that more rules always mean more complexity? Their claim that the matter is all very confused is easily supported by many experiments, some of which seem very confused. However, some of the experiments are artifactual, and some are misunderstood. I agree with them that if all the rules and their application to word sequences are undifferentiated, then no coherent relation between the grammar and behavior appears. But if the different formal characteristics of the rules are taken into account, the simple relation between the grammar and psychological complexity can be maintained for the cases which have been studied. In particular, their strongest "negative" fact is that sentences with auxiliaries, like "John *will have been going* to town," are no more difficult to remember and understand than simple sentences, like "John goes to town" (found by both Mehler and Savin). Yet sentences with auxiliaries ought to be more complex if TG interacts straightforwardly with memory and perception, since auxiliary sentences involve more rules.

In fact, in this case there may be no dilemma at all. These sentences are "kernels" (sentences with no optional markers in the deep structure and no optional transformations in the derivation). The only transformation which applies to them is the affix-movement transformation. This transformation *also* applies to the very simplest declarative sentences. Thus in transfor-

mational terms, these sentences are just like simple declarative sentences and should be no more complex (assuming that within sentence lengths from two to seven words, memory is not differentially affected).

COMPLEXITY AND KERNELIZATION. The search for a *general* theory of psychological complexity is quite frustrating, and in many other cases the relation between rules and behaviors is obscure. For example, differences in experimental paradigm can greatly affect the way the grammar is reflected in orderings among types of sentences. Mehler and I have recently found that in a short-term memory paradigm, sentences with adverbs and particles are recalled with distortions which do *not* relate to their deep syntactic structure. These errors characteristically include placing the adverb at the beginning and the particle at the end of the sentence. (For example, if subjects hear the sentence "The salesman carelessly sent over a large check," they repeat it as "Carelessly *the salesman sent a large check* over." Errors in the reverse direction rarely occur.) On the other hand, in a long-term memory paradigm the results follow predictions based on the deep structure of the sentence. This latter result supports the "coding" hypothesis found for the original optionals, which claims that sentences are remembered in their deep structure form with indications as to what transformations will apply.

These results show that in different psychological tasks behavior can be related to the grammar in different ways. In short-term memory the psychologically less complex form is the one with *more* transformations. In long-term memory it is the one with fewer transformations. I interpret these results as the consequence of a general "kernelization" routine in the psychological manipulation of sentences. As a strategy it is stated: *Reduce sentential material to simple declarative sentences.* This routine applies to the surface structure of the sentences *or* to the deep structure, depending on the psychological task. In short-term memory this routine applies to the surface phrase structure of the sentence and tends to reduce the structure to a *simple sentence* (italicized in the example above) preceded by an adverb and followed by a preposition. In long-term memory this routine applies to the deep structure and causes errors in which the transformations are forgotten, but the deep structure kernel remains.

Kernelization appears as a perceptual phenomenon in our research on the location of clicks, shocks, and flashes presented during the course of speech. Using materials in which the fine phrase structure was carefully controlled, we have found that the *only* phrase structure break which has an effect on the location of the extraneous stimulus is the boundary between clauses. That is, it is *not* the case that the "constituent" is the unit of speech perception (as we have claimed). Rather the ongoing perceptual processing of the speech signal segments it into sentence groups. Thus even as we hear sentences, we attempt to kernelize their surface structure.[9]

The Lowly Word

During the previous days of this conference, we have considered various behavioral effects which pertain to words and their mutual associations. I should like to submit now that these papers implicitly (some explicitly) distinguish "verbal behavior" from "language behavior." During this conference I have come to realize the comfort of the study of words if they are stripped of their relevance to language. But I am concerned with the difficulty of integrating the results from these experiments with the study of language behavior in general. There are both theoretical and empirical demonstrations that a sentence is greater than the sum of its words.

[9] See T. Bever, J. Fodor, and M. Garrett (1966); T. Bever, R. Kirk, J. Lackner, and L. Squire (1967); and T. Bever and J. Mehler (1967).

In linguistic analysis the syntactic and phonological role that a word can play in sentences determines its internal structure: the internal analysis of words depends on a prior general analysis of sentences. Consequently, study of individual words or groups of words can be understood only as a special case of the study of sentences. Various results support the related psychological fact that randomized sequences of words are manipulated in entirely different ways from words organized into sentences.

First, Johnson's analysis of the literature has highlighted the behavioral discontinuity of graduated increases in approximations to English. Roughly, a sequence is either treated as language or it is not. In a series of recent experiments, Mehler and I have found that the apparent duration of sentences follows a power function lower than that for randomized sequences. This is also true of the actual durations of sentences and random word sequences read aloud by subjects who think they are reading at a constant speed. In a finding which may be related I have discovered that the subjective centrality of speech presented alternately to the two ears is significantly closer to a step function for speech than for random word sequences.

Several experiments indicate a physiological difference in the processing of these two kinds of material. In the click experiments, in which subjects locate clicks presented during sentences, loud clicks in the right ear are perceived as occurring later than clicks presented in the left ear. This asymmetry does not appear for clicks presented during random word sequences. Second, in a short-term memory paradigm I have found that more order mistakes are made in the left ear than in the right for sentences, but this also does not appear in random word sequences.

These experiments only provide some more empirical support for the obvious. Insofar as an experiment with words is concerned with them as part of language behavior, it is concerned with them as units determined by their role in sentences. Insofar as these properties are ignored, the relevance of the research for language behavior is diminished.

Conclusion

I have reviewed these confusing results because I want everyone to be aware that we do not think we have the answers. For reasons that have appeared in the discussions, we are convinced that sequential probability statements cannot account for the structure of the performance of intuitions about language. Nor can probability statements account for the interaction of linguistic structures with many other kinds of behavioral phenomena. So we are sure that that is *not* the answer. We think we see some of the questions. Our problem is that we need a whole new set of theories and facts to start answering those questions. Obviously, we need help.

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Toward a Wedding of Insufficiencies

19

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The thrust of my commentary on the papers of Garrett and Fodor, McNeill, and Johnson will be that neither the models of the language *user* proposed by contemporary behavior theory nor, by inference, from contemporary linguistic theory are sufficient. Each needs supplementation from the other. Although these papers differ in substance, they are similar in other respects which concern me as a psycholinguist. First, they provide evidence for the psychological relevance of transformational grammars (i.e., such grammars are not mere logical exercises, as some psychologists would like to believe); second, they stress the "competence" of native speakers as the primary criterion toward which psycholinguistic explanations must aspire ("competence" remaining a concept to be conjured with); and third, McNeill and Garrett and Fodor, at least, repeatedly state that S-R theory (as they call it) is incapable of incorporating the phenomena described by a transformational grammar—*in principle*.

I agree completely with the concluding remarks of Garrett and Fodor: "The problem for general S-R theory is thus clear. Either it must be demonstrated how an appeal to the sorts of learning mechanisms S-R theory allows can account for the assimilation and application of the kinds of linguistic information incorporated in the more powerful competence models, or associationistic accounts of language learning and of speech must be abandoned." But this particular shoe fits on their feet as well as mine. The implication given in the Garrett-Fodor paper, as well as in the McNeill paper, is that any kind of S-R theory has *already* been ruled out of the running, as far as incorporating language behavior is concerned. It is their responsibility to demonstrate, rigorously and conclusively, that this is the case. I will try to show that this has not been done and, further, that a sophisticated behavior theory, utilizing S and R constructs along with principles of association, is becoming increasingly compatible with theories of linguistic competence, as both mature. Since I want my comments on theory to apply to my discussion of experiments, I shall take up theory first and experiments second.

On The Psycholinguistic Status Of Contemporary Behavior Theory

Varieties of behavior theory

None of the papers under discussion makes a clear distinction among the