Pick up almost any book on language and immediately you are confronted by the term "competence" followed by the injunction: you will make no progress in the study of language without taking competence into account. What is the meaning of this all-important term? According to Chomsky (1965, p. 4), competence refers to the "speaker-hearer's knowledge of his language," and performance (the other side of the coin) refers to "the actual use of language in concrete situations." Furthermore, he tells us: "Linguistic theory is concerned primarily with an ideal speaker-listener, in a completely homogeneous speech-community, who knows its language perfectly and is unaffected by such grammatically irrelevant conditions as memory limitations, distractions, shifts of attention and interest, and errors (random or characteristic) in applying his knowledge of the language in actual performance" (p. 3). While actual language use may provide some evidence as to the mental reality underlying language behavior, he continues, it "surely cannot constitute the actual subject matter of linguistics, if this is to be a serious discipline" (p. 4).

A first reading of this enumeration of topics to be avoided, namely memory, motivation, attention, and assorted social psychological, personality, and other psychological variables, suggests that this pronouncement is the preamble of some kind of nonaggression pact between the fields of linguistics and psychology: Linguistics promises not to infringe on the territory of psychology by studiously avoiding any attempt to explain anything that a psychologist might find of even remote interest, in exchange for which psychology promises to return the favor. However, as everybody knows, this is not true, for Chomsky (1965, p. 9) declares, "No doubt, a reasonable model of language use will incorporate, as a basic component, the generative grammar that expresses the speaker-hearer's knowledge of the language." 

Then, having stated that he is interested in a mentalistic linguistics, Chomsky (1965, p. 193) explains that "Mentalistic linguistics is simply theoretical linguistics that uses performance as data (along with other data, for example, the data
provided by introspection) for the determination of competence, the latter being taken as the primary object of its investigation. In other words, although he is not interested in actual performance, with all its ugly obtrusive variables, he will use it to investigate competence. Elsewhere, we are warned that we must restrict our use of performance data to those situations in which we may do so in "devious and clever ways". Although admitting behavioral data of some sort into his area of study, he nevertheless disposes of the behaviorists' opposition to mentalism by saying (Chomsky, 1965, p. 193): "The behaviorist position is not an arguable matter. It is simply an expression of lack of interest in theory and explanation."

It might be well to make clear at the outset that behaviorists are indeed interested in theory ... I shall assume nobody believes that behaviorists lack interest in explaining their data, and will therefore not comment on that any further. Perhaps the point can be made clearer by explaining that the behaviorist objection is only to theories that place the significant problems beyond the reach of empirical investigation. Skinner's (1950) objection in his paper "Are theories of learning necessary?" (from which the title for my paper stems) was to the use of another dimensional system, such as physiology or mental events, to explain something that was happening on a behavioral level. Whether a theory using physiological dimensions will become acceptable is a matter of obtaining the requisite data both in physiology and in behavior. A theory employing mental terms, however, gives rise to no data and therefore cannot become acceptable in the future either.

But let us return to the problem of using some carefully selected segment of performance data to shed light on the competence of the speaker-hearer. Essentially such a procedure consists of the linguist deciding what part of the performance data to explain. It reminds me of Vonnegut's (1974) recent explanation of his published interview. He describes it in the following way: "It is what I should have said, not what I really said... I went to work on the transcript with pen and pencil and
scissors and paste, to make it appear that speaking my native tongue and thinking about important matters came very easily to me." (p. xxii).

I assume that in these introductory remarks I have made clear to those unacquainted with my views and work that my answer to the question posed in the title is a resounding "NO". I hope, in this paper, to catalogue the reasons for the elimination of the concept of competence, at least for the psychologist, if not for the linguist. But even in linguistics, it is fair to say that the concept of competence is getting a bit frayed. Hymes (1974), to take one outstanding linguist, has created the concept of communicative competence. He rejects the idea that one first does an analysis of the structure of a language and then adds the knowledge of sociology to arrive at the area of sociolinguistics. Hymes (1974, pp. 132-133) tells us: "There is no internal linguistic makeup that would lead one to group together 'See you later, alligator,' 'Ta Ta,' 'Au revoir,' 'Don't take any wooden nickels,' 'Glad you could come' ... as leave-takings." Hymes gives further reasons for the necessity of broadening what he calls the restricted concept of grammatical competence, by including such social factors as would explain utterances by a secretary on the telephone, e.g., "May I say who's calling?" For here the appropriate answer as given by the generative grammar approach would be just "Yes" when the linguistic competence called for is the translation into the question "Who's calling?", with "May I say..." to be responded to as a polite form.

Hymes also comments on Chomsky's concept of creative language use. A sentence called novel solely by internal criteria may be inappropriate or bizarre when taking the context into account, while a sentence not regarded as novel by internal criteria may indeed be so because it occurs in a novel context. Finally, in a demand for the "liberation" of the concept of competence from being simply a synonym of the word, "grammar," Hymes (1974, p. 205) asks that the concept of competence refer to "abilities actually held by persons." Those abilities are not distributed equally among all people, nor are they so distributed among situations. Hymes wants
to restore the old meaning to the word competence, that is, a summary statement about the behavior that the individual is likely to emit at some future time. Other sociolinguists make the same point that Hymes makes; they maintain that there is no such thing as linguistics without the social part: There is only sociolinguistics.

Generative grammar has also been criticized by those linguists not yet convinced of the wisdom of sociolinguistics. Derwing (1973) makes a detailed study of Chomsky's system. His book should be read by linguists and psycholinguists alike, for he has carefully analyzed Chomsky's writings and those of his students, pointing out both the contradictions in their work and the ultimate lack of testability of the theory. Derwing's (1973, p. 321) summary statement is worth quoting: "The chief argument of this book has been that contemporary linguistics has gone fundamentally astray, both conceptually and methodologically."

For those who feel that linguistics must have something to contribute to the psychology of language, there is some solace to be had. One needs to look for those linguists who view the empirical examination of real language behavior as the cornerstone of their work. One such linguistic approach is called form-content analysis. Originated by William Diver at Columbia University, it has been applied to Spanish by Garcia (in press) and to German by Zubin (1974), to mention but two examples. The approach is diametrically opposed to Chomsky's basic assumption that one should exclude human factors from attempts to understand language. Garcia points out the difference between the two systems by showing that Chomsky postulates a nonrandomness of signals to be explained by a formal system of rules regarding the signals in terms of the criteria of simplicity of description, while

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Many of the points that I will make in this paper are also made in Derwing's book. Even though I came across this book late in the writing of this paper, it was a delight to find it. I thank David Zubin for pointing it out to me.
Diver speaks of nonrandomness of vocal movements in people who are trying to communicate with signals that have meaning. Thus for Diver and his students, the dichotomy of competence and performance is simply not possible. They use such human functions as memory, laziness (cf. Zipf's law of least effort), and avoidance of complexity as factors to explain the use of certain words in particular grammatical classes.

Zubin (1974) tested the hypothesis, in the German language, that the greater the causality of a noun, the greater the probability that it will be in the dative rather than in the accusative case after verbs of avoidance.\(^{75}\) He verified this hypothesis by constructing different situations for the native German speakers who were asked to construct sentences describing what happened. To give but one example, when Zubin described a person as avoiding a car because it stands in his way, the subjects summarized the situation using the dative 14% of the time, while when he described the same person as avoiding a car that is driving towards him, the subjects used the dative 100% of the time. Clearly the dative morphological signal connotes more activity in the noun it is applied to than does the accusative morphology. This is only an example, but it shows both the usefulness of including nonlinguistic factors in considering the reason for grammatical structure, and the empirical nature of the validation procedure that can be applied to grammatical problems.

Before presenting my catalogue of reasons for the elimination of the concept of competence, allow me to refer you to some papers in which I deal with the generative model as a whole (Salzinger, 1967; 1970; 1973a; Salzinger & Feldman -- introduction, 1973) so that I will be able here to concentrate on the concept of competence.\(^{46,50,51,55}\)

**Competence as a long, complicated promissory note.** The time is long overdue for that performance model that Miller and Chomsky (1963) promised us will contain the competence model as an essential part.\(^{37}\) There are a number of reasons for the
nonfulfillment of the promise. To begin with, there is the abyss that exists between competence and performance. Repeatedly we are warned that competence merely contains the rules of the system that accounts for the sentences that English speaker-hearers could construct -- if only they were perfect -- and that what we need is a performance model that would account for the failings in the human being that prevent him or her from manifesting the perfection of the competence model. The performance model is a device that shows how we forget and are distracted and thus are unable to embed forever as the competence model tells us we must. The competence model is simply a logical system. As a consequence, it is very difficult indeed to know where to begin to connect it to something real, such as speech.

In addition to ruling out all those things that psychologists study in human behavior, the competence model begins with a unit that is quite arbitrary, namely, the sentence; there are after all a great many other possible units$^{52}$ (Salzinger, 1973b), and there are a great many things that people actually say that are not sentences (cf. Hymes, above).$^{25}$ Perhaps we would be better off having an incompetence model in addition to the competence model to take care of all the nonsentences and the nongrammatical utterances that serve to cause trouble, since the competence model is only about those grammatical sentences that a generative grammarian considers so. In a commentary on a paper by Chomsky$^{10}$ (1970a), Black (1970) spoke of the sentence as a "grammarians artifact," pointing out that speaking an indefinitely long sentence is about as far removed from actually talking as walking an indefinitely long distance is from actually walking.$^{2}$ Black also objected to the procedure of verification of the grammaticality of the sentences. Comparing the verification of Chomsky's system with that of Kepler's laws of the planetary motions, Black (1970, p. 454) says: "We cannot ask a planet how it thinks it ought to behave in situations that don't arise: yet this seems to be what we are doing when we consult a native 'informant'."$^{2}$ In response, Chomsky (1970b) maintains that in Kepler's case a telescope has the same role as an informant has in Chomsky's.}$^{11}$ He
insists that there is no difference in principle between the two verification procedures.

However, when I criticized Chomsky for being unable to show that grammaticalness can be reliably judged by native speakers of English, as his system requires (Salzinger, 1967), he responded (Chomsky, 1972, p. 93): "Obviously, the failure [to find a reliable division between grammatical and nongrammatical strings] indicates nothing more than that the tests were ineffective. One can invent innumerable tests that would fail to provide some given classification. Surely the classification itself is not in question. Thus, Salzinger would agree, quite apart from any experimental test that might be devised, that the sentences of this footnote share an important property that does not hold of the set of strings of words formed by reading each of these sentences, word by word, from right to left." This statement makes clear that Chomsky does indeed wish to impose a different principle for the verification of grammar than for the verification of the laws of planetary motion, for it is hard to conceive of asking for the validation of Kepler's laws by appealing to judgments of certain extreme values only. If we find it difficult to agree on some sentences but easy on others, then Chomsky wishes to appeal only to the simple ones to verify the theory. In sum, the promissory note has not been redeemed because the basic model does not lend itself to the explication of behavior which Chomsky calls by the derogatory term "performance."

Competence as an idealization. One argument that Chomsky gives for the great discrepancy between the competence model and behavior is that the competence model is an idealization. But we must ask, An idealization of what? If it is an idealization of language behavior, why does it exclude from consideration so many sentences and so many nonsentences that we know to occur? On the other hand, if it is an idealization of an underlying neurophysiological system to explain how language is produced and understood -- something often implied by the generative grammarians -- why are all the terms basically behavioral and the verification
procedure one of intuition? The answer to both questions appears to be that the idealization is restricted to that of a generative grammarian as speaker-hearer when at work on the model.

The idealization of competence fails as an attempt at reductionism for the following reasons: For reductionism to succeed, it must formalize both the reducing and the reduced sciences. The competence model that we are presented seems on the contrary to do neither, for it deals with a level that is neither behavioral nor neurophysiological, although it is closer to the behavioral level, and the terms are of such a nature as to make translation into the neurophysiological science no easier. In addition, not only is it true, even by Chomsky's own admission, that we have not yet formalized language behavior (performance, if you must call it that) but we do not yet have the knowledge, never mind the formalization, in biology, the reducing science, for the reduction. Luria (1974), one of our foremost biologists, in an article eager to show how biology can help to explain language behavior, says, after stating that the justification for treating language as a biological phenomenon comes from Chomsky's "observation" that all human language is based on innate grammatical and syntactic structures (Luria, 1974, p. 28): "How to approach the biology of human language and thereby also the biology of the human mind is not yet easy to see." In other words, reductionism as an approach to the study of language is beset by only two problems: 1. We do not know enough to formalize language behavior, and 2. We do not have enough data in biology to begin the formalization there. Turner (1967, p. 334) says: "...reductionism as an article of faith in the absence of a well-structured secondary science (psychology) is a trivial thesis." The idealization cum reduction that Chomsky presents us then fails on the grounds that reductionism is not possible under the present circumstances.

In his book Psychological Explanation, Fodor (1968, p. 134) states: "A theory that predicts the observational data but fails to predict correctly the
relevant counterfactuals is a false theory, whatever the area of science in which it may be entertained. The model or theory or idealization must predict not only behavior that is observed but it must also predict behavior that does not occur. The interesting fact about the competence model is that it predicts grammatical sentences unlimited in length (let us for the sake of argument agree that we know what they are) and it shows quite clearly that it cannot generate ungrammatical strings. The model then fails on both counts. First it fails to generate the nongrammatical strings which we can observe to occur, and then it predicts the generation of infinitely long sentences with infinite embeddings that we know cannot occur. It is interesting to quote Miller (1967, p. 88) in this context: "Chomsky's achievement was to prove rigorously that any language that does not restrict this kind of recursive embedding contains sentences that cannot be spoken or understood by devices, human or mechanical, with finite memories." Miller uses that as an argument for the distinction between competence and performance, that is, between the idealization and the real behavior. But should we not conclude that any model predicting behavior so obviously absurd both logically (as proved by Chomsky) and empirically (as shown by many experiments, if not by common sense) ought to be abandoned? I have not yet had the opportunity to study the recent book by Fodor, Bever, and Garrett (1974), but in their discussion of sentence perception, I find that they are in agreement with my view. They say (p. 279): "Any model of the ideal speaker-hearer which is incompatible with whatever is known about the rest of human psychology is ipso facto disconfirmed." Let us turn to another aspect of the competence model. It assumes that one model is sufficient for both the speaker and the hearer. The question that I would like to raise about this turns on both the logical and the empirical grounds for the assumption. Fraser, Bellugi, and Brown (1963) performed an experiment in which they thought they had shown that children understand more than they can speak, and that therefore the acquisition of language could not take place through the reinforcement
of speech. Of course, behavior theory does not assume that language is acquired through one mechanism only. Nevertheless, Fernald (1972) showed that their experiment did not prove the precedence of comprehension to production, since the comprehension measure had had a contaminating statistical advantage not present in their production measure.

Shipley, Smith, and Gleitman (1967), also interested in language as stimulus, performed an experiment in which children were given certain verbal directions. The children did better in following directions presented in well-formed structures than in telegraphic form, even though they could not at that age produce such well-formed structures. Again it looked as if the children's comprehension, which is assumed in these studies to better reflect competence, exceeded their production. Brown (1973), however, interpreted the findings to mean that the children responded in a puzzled way to the telegraphic speech of their mothers because their mothers did not ordinarily speak that way. What looked like superior performance in response to the well-formed sentences was better explained by poorer performance in response to telegraphic speech.

Luckily, we have another study bearing directly on this question. Kramer (1973) repeated the study with some improvements. She presented anomalous sentences to children, that is, sentences which asked them to do things which are peculiar or at least unusual, such as "Sit on the telephone". The children responded to these kinds of structures by essentially correcting them semantically. They spoke on the telephone rather than sitting on it. Furthermore, when addressed in telegraphic speech, that is, with commands minus function words, they did as well in following the instructions as when they were given the same directions in well-formed sentences. Again we find that comprehension does not exceed performance. But perhaps more important, the study shows that the child, like the adult, makes use of whatever information he or she is given or has available in responding. The concept of response is as seems to explain a great deal here. Having learned earlier that one speaks on
the telephone, the child on hearing the word "telephone," does what he or she has been reinforced to do in the past. Thus while the child appears to follow some complicated, well-structured sentence-form requiring the knowledge of an arcane system of rules, the child is in fact merely following the surrounding stimuli in the only way he or she has learned. The issue then is not whether the child comprehends before speaking, or vice versa, or does both at the same time; the issue concerns what stimuli control the behavior of the child. The concept of the functional stimulus cannot be ignored in this area any more than in any other. What the speaker-hearer model gains in simplicity by proposing one system for both, it loses in real life where organisms respond in the most direct way possible. The normal redundancy of an utterance is usually strengthened by its redundancy with the environment, the past history of communication, the facial expression, other gestures, etc. Thus conversation always involves a multiplicity of stimuli only some of which are verbal, and only a subset of those are grammatical.

Competence as a flight from simplicity. It sounds simpler to have one system for both comprehension and production, but the step left out is the required analysis-by-synthesis to achieve understanding. I have commented on such models in detail elsewhere (Salzinger, 1973a). The behavioral model, on the other hand, says that the organism responds to the stimulus which has the greatest evoking power at that time and place. It is for that reason that when we read or hear, we often make a mistake in interpretation, or we understand something correctly even though it is stated in a misleading way when analyzed carefully but in the absence of the environment in which it was emitted. The above studies with children simply show the same tendency in them, namely, to interpret puzzling utterances in a familiar way rather than to spend time worrying about the ambiguity of the utterance.

While generative grammarians have always emphasized ambiguity, it has been a concept of importance only in their system and not in actual speech. Real people always take into account stimuli around them: stimuli that precede the utterance, the
speaker, the hearers, etc. The compound stimulus then functions to evoke a response that allows the conversation to continue. Furthermore, unlike the generative grammarian who can only reread the same isolated puzzling sentence, the real speaker or hearer has the option of asking 'What?' or 'What do you mean?' etc. The fact that ambiguity is not to be tolerated in actual social intercourse is at least partially demonstrated by some of our studies in which we have found that hospitalized schizophrenic patients speak in an ambiguous way and that the more ambiguous or the less understandable they are, the longer their stay in the hospital. 61-63 (Salzinger, Portnoy, and Feldman, 1964, 1966; Salzinger, Portnoy, Pisoni, and Feldman, 1970).

Another case where we are faced with the choice of a simpler vs. a more complex model is with respect to the perception of clicks and language stimuli simultaneously presented. The idea was that the subject would be able to perceive clicks more readily between than within units of the sentence and would therefore report the occurrence of the click at the boundaries, thus supplying us with information concerning the existence of the units. Ingenious as this technique is, it fails to exclude those basic variables that enter all experiments in perception. There are what Reber (1973) calls response biases and the subject's general attentional priorities that also control his "placement" of the click. 43 To be sure, Bever (1973) has responded to these criticisms, 1 but it is not yet entirely clear what kind of variable or what particular level of unit 72 (Toppino, 1974) the phenomenon of click migration reflects. It is one thing to make experiments "clever and devious," as Chomsky suggests; it is another altogether to show exactly which variable controls the outcome. The fact that speech is not ordinarily devoid of accompanying stimuli directing the hearer, suggests that the study restricted to the utterances only may well be an impossible task.

The notion of responding to simpler rather than more complex stimuli is fortified by what we know about the behavior of animals. Such seemingly complex responses as movement towards an environment of high humidity, as in the woodlouse, can be
explained by what ethologists call kinesis, an undirected movement varying with the intensity of stimulation. Thus although the end product of the movement in the wood- louse that moves more in dry than in wet environments is to bring it into wet environments, it is clear from empirical study that this is not movement toward or away from environments of particular humidity. The mechanisms organisms have that allow them to achieve certain ends are very often quite different from what a superficial inspection might suggest. The mechanism underlying a particular act need not contain within it the end which is in fact achieved. From what we know about the evolution of animals, the idea that human beings should have been given a particular device somewhere in the brain for recognizing sentences or for separating grammars from one another seems rather far-fetched when one considers the kinds of slow and small changes that take place over time and yet which have rather profound effects on the interaction of the organism with its environment.

Although it has been argued for some time, and most persuasively by Lenneberg (1967), that speech in human beings is unique among the animals now extant and that only a discontinuity theory of language evolution could explain human speech, recent research (Gardner & Gardner, 1969, 1971; Premack, 1970, 1971; and Rumbaugh, Gill, & von Glasersfeld, 1973) has shown that the nonhuman primates are capable of learning language as long as we do not try to make them vocalize, a response that is difficult for them to execute for peripheral-musculature and vocal-cavity reasons rather than because of some deficit in the brain, as Lenneberg would have us think. And so while Chomsky and his students tell us that language is species-specific and everybody dutifully repeats it, some psychologists proceeded to examine the question empirically despite its logical absurdity. At this point, those who were surprised that nonhuman primates could learn to communicate in ways quite similar to ours have retreated by quickly adjusting their definition of what constitutes language; the final point of retreat, I imagine, will have to be the ability to say what Noam Chomsky says about language. If one of the non-
human primates can be trained to say that, then perhaps we will have to accept the notion that the human being is not the one and only possessor of true language.

**Competence as a modifier in noun's clothing.** It is my contention that when we make a noun of an adjective, we run the danger of reification. We all know what happened when the adjective "unconscious" was turned into a noun: People began to speak of the unconscious and to look for things inside it; they began to speak of taking things out and they even paid specialists to remove things. Now with respect to competence, we seem to have the same problem. It is one thing to say that an individual is competent or even that his or her behavior appears to be competent, but when one begins to speak of a person's competence, one comes just a little closer to the notion of asking for its location and its content. After all, if you have a competence, you must have it somewhere, and if you have it somewhere, you must have something in it. It is for this reason that Chomsky did not content himself with the word "grammar" to describe what he means by competence. For grammar, in our experience, already has an external referent, as in grammar books, and so would not have done for Chomsky's model. The fact that this competence now lies inside the body also means that one need not examine real verbal interchange to study it, since all the necessary information is internalized. And finally, this change of part of speech allows one to hide in the competence attic all the problems that one would ordinarily have to examine downstairs in the livingroom where the performance takes place.

**Competence as a concept in fact's clothing.** Once the competence was located inside the organism, it was not long before people started to speak about explaining the facts of competence. Even at this conference the speakers of this session were assigned the task of examining how, not if, the notion of competence could be used to account for linguistic performance. Since when does the investigator holding to one theory have to account for the concepts which the holder of another theory uses to explain his or her data? But this is how far we have come. It is my hope that
the phoneme, while under others it may be the word, phrase, clause, sentence, paragraph, story, essay, argument, or description. The importance of the flexibility of such unit size definitions is that they reflect the actual functional relationship of the stimuli surrounding the responses in question.

To review, language behavior is considered by behavior theorists to consist of varying unit sizes. The responses are also always considered as members of various response classes. Thus, to give an example, members of the same response class may differ in length but may be evoked by the same stimulus, e.g., a fire. That kind of event may evoke the responses, "Oh!" "Help!" "There's a fire here!," and the making of a telephone call, running away, an increase in heart rate, etc. The problems involved in the definition of response classes are discussed in detail in Salzinger (1967).46

**Competence as a roadblock to language modification.** This brings us to the problem of whether language behavior is assumed to have been acquired by a process of learning or whether it is viewed primarily as the result of an unfolding process. If it is primarily the latter, then we must face every deficit in language behavior as something built in by competence; on the other hand, if we assume that language behavior is learned and maintained through the variables of behavior theory, then we are at least in a position to try to improve the behavior. I am not trying to say that the basic assumption that language behavior is learned makes it true — although I shall contend that we have much data to defend such a view. I am saying that by making the opposite assumption, that language is almost completely a matter of maturation, we forestall investigation into the possibility of modification.

The facts of history are that operant conditioners simply ignored Chomsky's (1959) so-called devastating review of Skinner's (1957) Verbal Behavior.70 Beginning with Greenspoon's experiment (1955), based on previous work by Skinner, on the conditioning of plural nouns, an onslaught of verbal conditioning studies was unleashed (Salzinger, 1959; 1969a; Krasner, 1958; Holz and Azrin, 1966)
which showed the possibilities of this paradigm in many fields of psychology, including social, developmental, and especially abnormal psychology. Response classes of various kinds, and units of different lengths, were used in the experiments. Some of these studies were diverted by an uninformed determination to be precise, with the unfortunate result of reducing the operant conditioning of verbal behavior to a rote memory experiment consisting of the pairing of one of three pronouns with a single past tense verb, all printed on little 3-by-5 index cards. Such experiments, set up essentially as problem-solving tasks for sophomore students, deteriorated into controversy over whether or not there can be any conditioning without awareness. Much of this material is well summarized by Krasner (1967), so I shall comment only that there are indeed situations in which verbal conditioning is possible without the subject being aware of it.

In our experiments on self-referred affect statements in normal and schizophrenic patients (Salzinger & Pisoni, 1958, 1960, 1961; Salzinger, Portnoy & Feldman, 1964), we conditioned subjects while they were even unaware that an experiment was going on, never mind that they were being conditioned (1969). In another experiment we demonstrated response generalization from positive to negative affect and vice versa (Portnoy & Salzinger, 1964). We (Salzinger, Portnoy, Zlotogura, & Keisner, 1963) found that the conditioning of plural nouns was possible in the context of continuous speech, with plural nouns ending in the /z/ sound being more conditionable than those ending in the /s/ sound. The subjects' reinforcement history clearly interacted with their current conditioning. While response class membership is thus a matter for empirical investigation, conditioning does take place. These basic experiments made clear that operant conditioning can be applied to verbal behavior.

In an experiment in which a papier maché clown served as the source of reinforcement by turning on his light-bulb nose and delivering candy, S. Salzinger and others in our laboratory (1962) conditioned young children to speak at various
rates by using different schedules of reinforcement. Of interest with respect to the concept of response class was the fact that children reinforced for the emission of general self-references showed an increase, not only in the self-references, but also in their total amount of speech, because most of their speech dealt with themselves. Again we find an interaction between the reinforcement procedure and reinforcement history.

The application of reinforcement procedures to children with little or no speech was attempted in our laboratory in 1965 (Salzinger, Feldman, Cowan, and Salzinger, 1965; Salzinger, 1968) as well as in others about the same time (e.g., Lovaas, 1973). Many children without speech can, by means of well planned schedules of reinforcement and by programming various situations appropriate to the emission of verbal responses desired at each stage of learning, be trained to speak. This kind of conditioning situation provides not only the possibility of helping children to acquire language who more often than not are left unhelped because of the implication that they are suffering from brain damage whose effect cannot be altered, but also provides the observer with the opportunity to watch the acquisition of language in slow motion, so to speak. In our observations of one four-year old boy whom we conditioned, we noticed, after he had learned to name objects in his environment, that the introduction, by imitation training, of an utterance of more than one word gave him a very useful tool for the further utilization of language. In one session he was reinforced for saying "Give me candy" instead of, as before, simply for saying "candy". Having learned that the emission of the full utterance "Gimme (we accepted this approximation to "give me") candy" resulted in his getting candy, he generalized the use of the frame "Gimme ______" to anything else that he wanted. This use resembled the pivot-open construction that is now in some disrepute, but obviously it was a construction strengthened by its specific reinforcing property, the kind of verbal response that Skinner (1957) called a mand. The critical variable was the reinforcing consequence.
In contrast to the competence model, the behavior theory model would also try to improve the language behavior of those children who do not get the appropriate kind of training to use their language in such a way as to be reinforced by the establishment in society. Staats (1968) treats this problem particularly with respect to reading, but many additional studies have been done and are to be found in the *Journal of Applied Behavior Analysis*. Studies of this kind are also reviewed in Salzinger (in press).

The general point to be made with respect to competence is that by assuming that it is innate without specific evidence for it, except in terms of such generalities as the common time of language acquisition -- which could after all be explained by common types of environments, especially early in life when all human infants depend to a large extent on an adult -- an obstacle is set up in the path of helping people. The history of operant conditioning is replete with instances in which we were told that a particular behavioral deficit is physiologically determined and therefore cannot be modified, when a judicious application of behavior modification found this to be untrue. It is important that we not blame the faults in our environment on the children through the making of unwarranted assumptions.

**Competence as a roadblock to a general theory of behavior.** Just how special is the human being? The evolution of science is punctuated by periods of controversy about the centrality of homo sapiens. First we had to give up the centrality of our world, then we were forced to admit our relationship to other primates, at least with respect to our anatomy and physiology, then we ungraciously admitted that at least some of our responses resembled those of the animals, then we granted that we did not always know what we were doing, and finally we allowed that at least some if not all of our behavior was determined by the environment. We are still trying to sequester unto ourselves the notion that only human beings think, and we are fighting at our last bastion to claim that only human beings have language. The data are nevertheless assailing us here also. I have already alluded to them. The fact is
that chimpanzees have already constructed utterances with plastic forms, by means of sign language, or by pressing buttons on a computer. Furthermore, these responses have the productive or creative aspect that linguists have for so long attributed solely to human beings. At least Washoe, the signing chimpanzee of the Gardners, was found signing to herself while "reading" a picture book, although to be sure she was doing it while up in a tree.

What are the advantages in considering a human being as an animal? There are many. We have much data about animals under a variety of circumstances, with and without invasion of their neurophysiological and biochemical systems. We find a great deal of order in these data. In the course of our research we have found that it is fruitful to extrapolate to human beings, in terms of both the data and the generalizations drawn from them. The experiments applying behavior theory to human beings have also been successful [49] (see Salzinger, 1969b). Thus when we speak of stimulus control, we have a concept that has proved itself useful not only in describing the behavior of animals but also the behavior of people in a variety of situations. Behavior theory has been successfully utilized in education whether through programmed instruction or more effective teaching techniques, in abnormal psychology in the more humane control of psychotic behavior and in the more effective treatment of neurotic behavior, in social psychology in the more precise description of social interaction, in psychophysics in the elucidation of the concept of threshold, with signal detection theory to give it a mathematical foundation, in psychopharmacology in the precise and effective pretesting of drugs and their effects on behavior, in developmental psychology in the better handling of children and the gaining of a surer understanding of their behavior, in physiological psychology by providing the precision of behavior control against which physiological theories can be effectively tested, and in the better understanding of the inter...

Given a theory of behavior as powerful as that, it seems foolish to avoid using
it to shed light on the language behavior of human beings. To insist that human beings must be different and that they cannot be traced through the history of evolution seems like a willful attempt to leave unused the most powerful tool we have in psychology for uncovering the sources of our language behavior. Embracing a competence model based on the premise of exclusivity seems foregone to be a failure on that count alone.

The caricatures that pass for descriptions of behavior theory in the area of psycholinguistics accuse behavior theorists of failing to believe in the organism. This is not justified. Behavior theorists view psychology as a biological science, and therefore view the neurophysiology and the biochemistry determined by the genes and the somatoplasm of the organism as variables. The competence model, on the other hand, views the organism as a constant. Current findings suggest that both the inside and the outside of organisms are conditionable by the same general paradigms.

In contrast to the competence model, behavior theory looks for those properties in the organism that make it sufficiently flexible to acquire language. A few years ago (Salzinger, 1973c), I made up a list of five such properties for language as stimulus and language as response. If we need a concept like competence, then these are the properties we should examine.

The first property is language-learning potential. Essentially, this property suggests the investigation of the various response systems of an organism to determine if any of these systems could be efficiently accommodated to serving the function of language. Assuming that something needs to be learned in order to have a language such as human beings have, we would turn our attention to the modifiability of the response systems -- whether they can be modified by imprinting, by respondent conditioning, or by operant conditioning.

The second property is general learning capacity. Any animal that can in general be only minimally influenced by its environment would not be expected to develop a language dependent on the kinds of modification which must be mediated by the environ-
ment. We have a large literature on the ability of various animals to learn.

The third critical property for language development is verbal response avail-
ability. Recent investigators of language in chimpanzees made a breakthrough because
they looked for a response system that had a high enough rate. Essentially, we are
saying that the response allowing the organism to communicate must have a high enough
operant level for the process of reinforcement to act on it. An individual incapable
of producing sounds cannot be expected to develop language by vocalizing, but may
acquire it through signs.

The fourth property necessary for language is the relative verbal response con-
ditionability. In human beings, verbal behavior is eminently conditionable. In
selecting an animal's response for language, one must select a system which has al-
ready been shown to respond to conditioning procedures.

The fifth property is the regulating function. Generative grammarians and moder-
psycholinguists have shown a singular lack of interest in this very important func-
tion. An outstanding theoretical treatment has come from Luria \(^{32,33}\) (1967; 1969).
He was able to show a regular progression over age of verbal control over nonverbal
responses. It seems to me that no matter how convoluted your grammatical system, if
it does not reveal how language guides nonverbal and verbal behavior, then the model
is incomplete.

Before leaving the issue of the blocking effect of the competence model on a
general theory, I would like to describe the inadequacies of the attempts by psycho-
linguists to discredit the behavior theory model in the area of language behavior.
In 1966, McNeill presented the following anecdote to demonstrate the futility of em-
ploying behavior theory to explain the acquisition of language: The mother is tryi
to get the child to stop using the double negative in "Nobody don't like me." De-
spite the mother saying "No" and repeating to the child "Nobody likes me" some ten
times, the child finally says "Oh, Nobody don't like me."\(^{35}\) (p. 69). It is peculi-
ary that the mother should have chosen to correct the grammar of this very self-
abnegating statement. One possibility is that in this interchange the mother's repetitive statements are positive reinforcers that keep the child going. Mother and child may be playing a game.

If the mother was being serious, however, then it might be well to point out that a change in this behavior could be brought about by using other methods. One would use shaping procedures along with repetitions of parts of the desired phrase or sentence. In addition to using an inefficient modification procedure, the mother was also hampered by a lack of supporting environmental stimuli. There is no obvious discriminative stimulus for the statement involved, and the constant repetition, in the absence of an appropriate stimulus, appears to be an insufficient condition for learning even when language learning is viewed as a learning of rules. Moeser and Bregman (1972, 1973) showed that the rules for a miniature language system were not learned in the absence of pictures corresponding to each of the sentences, even when 3200 trials were presented, although class membership of new words could be learned through verbal context alone, once the rules based on the sentences with pictures had been acquired.\textsuperscript{38,39} Thus the fact that the child seemed unable to dispense with the double negative did not provide evidence for a "stage" in language maturation. Rather it reflected inadequate learning conditions.

In another attempt to show that behavior theory does not explain the acquisition of language, Brown and Hanlon (1970) investigated what they call "communication pressure" at a time when the children are supposedly acquiring new grammatical structures.\textsuperscript{5} They classified the child's questions into well-formed ones and into non-well-formed ones, and the interlocutors' responses into "sequiturs" and "nonsequiturs". By their definition, the answer "We ate them all" to the question "Where Christmas cookies?" was a sequitur (an answer that showed that the question was understood) to a question not well formed; the answer "Your spoon?" to the question "Where my spoon?", on the other hand, was considered to be a nonsequitur since it consisted of a question, not an answer. They found no significant
differences in the number of sequiturs or nonsequiturs to the child's well-formed or non-well-formed questions, and concluded that there was no selective communication pressure.

This study fails to show a lack of communication pressure in children, not because there is none, but because of a number of methodological errors. The sampling of the consequences evoked by the questions is unduly restricted. Presumably a sequitur ought to have some positively reinforcing properties — but how can it when, while showing comprehension of the question, it informs the child that he may not have the cookies? Furthermore, it is not at all clear that the partial repetition of the child's statement (the nonsequitur in the analysis) might not be functioning as a positive reinforcer even though it does not show full comprehension of the question. At times merely giving the child attention by making a verbal response may suffice as a reinforcer. At other times, presenting a discriminative stimulus — that is, a stimulus in the presence of which a response is positively reinforced — may evoke another utterance of the same kind. At some times, the critical reinforcer may be nonverbal — a smile, a look, a cookie, a toy; at others, it may be verbal — repetition of what the child said, agreement, praise, or the telling of a story. Only observation of a good deal of behavior and its consequences can make clear what responses are being reinforced and what reinforcers are in each case. The classification system that Brown and Hanlon used to decide that no communication pressure toward more grammatical speech existed, partially confounded its classification system with the classification system of positive reinforcer, negative reinforcer, and punisher.

Brown and Hanlon tested the relevance of behavior theory in another way as well. They related syntactically correct and incorrect speech to contingent approval and disapproval. Within the former category, they included statements such as, "That's right," "Correct," "Very good," and "Yes:" in the latter category, they included statements such as, "That is wrong," "That's not right," and "No." They found rela-
tively few occurrences of these kinds of approval and disapproval. Among those few occurrences that they did find, the parents did not approve more often of the syntactically correct structures than of the others. Here are some examples of the data they found (p. 49): 'Child: 'Draw a boot paper.' Mother: 'That's right. Draw a boot on paper.' Child: 'Mama isn't boy, he a girl.' Mother: 'That's right.'" 5

What can we make of this?

First, we must remember that these conversations by no means include all the reinforcers or even the significant ones in these situations. Food, cookies, attention, hugging, smiling, opportunity to draw are all positive reinforcers of which we have no record from the tapes that were studied by Brown and Hanlon. Second, it is quite true that parents mostly reinforce the content, not the form of verbal behavior, and that is because language is communication, not a protracted lesson in grammar or syntax. Third, the mother was very likely reinforcing the child for drawing, not for speaking, at the time of the child's remark, 'Draw a boot paper.' In addition, however, the mother was clearly correcting the child's grammar as well by the repetition in altered form. All in all Brown and Hanlon do not present impressive data to test the relevance of behavior theory in the acquisition of language.

Typically, generative grammarians accuse behavior theorists of oversimplifying. In this case, it is the grammarians and grammarophiles who are doing the oversimplifying. To find out whether an acquired response is reinforced, you must observe its occurrence and its consequences, not by preselecting some series of events thought to act as positive or negative reinforcers, but by observing all the consequences — verbal and nonverbal, conditioned and primary. The Brown and Hanlon study simply does not test the hypothesis it sets out to test. For an example of a much better effort at observing the contingent relationship between events in the environment and the child's speech, we recommend Horner and Gussow (1972), who analyzed the complete tape recording of two full days' worth of verbal behavior. 24 The contingencies
in that case were quite clear.

We come to the conclusion that it is better to make use of a general theory of behavior than one specially constructed for human beings and that on that count alone we should eliminate the concept of competence.

**Competence in the guise of a theory of language.** It is clear that what Chomsky talks about is grammar but what he wishes to generalize about is language. I think the time has come to say that the emperor has no clothes on. We have already talked about the social variables that Chomsky is determined to exclude, and we have already shown how misguided that approach is.

Now I would like to point out that an increasing number of investigators have rightfully become discontented with a theory of language that is restricted to grammar. There are theoretical questions, such as what the deep structure is filled with, being raised by linguists who want to fill it with semantics, and there are the more psychological questions being raised by such investigators as Schlesinger (1971), who tries to derive a structure of meaning. He puts into the deep structure the intentions of the speaker — what he or she wishes to say. While this is still putting inside the organism what resides outside, it does show a progression from grammar to meaning.

Also of interest in recent years is the greater appeal that the external environment has had for developmental psychologists. This is what Brown (1973) has called "rich interpretation" and has been used by Bloom (1970, 1973) and Bowerman (1973). Bowerman (1973, p. 11) argues: "...many additional clues to competence can be found in the nonlinguistic contexts of speech events, the linguistic interaction between parent and child, the relationship between successive utterances of the child, and the comparison of the child's speech to that of his parents." She states that Chomsky's model of transformational grammar is not equipped to provide "an explicit account of the semantic functions of sentence elements" and therefore "may be basically unable to reflect children's linguistic knowledge adequately." (pp. 14-15).
We were able to show in our laboratory that the extent to which an individual understands another depends on psychopathological and social variables. We have already mentioned that schizophrenic patients are more difficult to understand than are normal individuals. We have also investigated how social contact patterns affect the extent to which people understand one another (Salzinger, Hammer, Portnoy, and Polgar, 1970; Hammer, Polgar, and Salzinger, 1969). The degree to which one person knows another and the centrality of the individual in the group are very important indeed in explaining how well people understand each other.

Krauss and Glucksberg (1969) tested a number of children for communicative competence. Children were required to name unique graphic figures so that adults could correctly apply the names to those same figures. The results showed that even at age 14, children still had not yet acquired the same competence that adults had in communicating in this way. The authors argued that one should make a distinction between Chomsky's grammatical competence and the communicative competence that interests psychologists.

Finally, it might be worthwhile to mention one more study done in our laboratory. Taking Chomsky at his word that one can have grammar without meaning, we (Salzinger & Eckerman, 1967) constructed two types of sentences out of function words and nonsense syllables. One type was the simple declarative sentence in the active mood; the other, the complex sentence which, according to Chomsky, ought to require transformations for proper processing, was the passive negative question. We also presented the elements of every sentence in random order. We had a number of interesting findings, but most germane to this discussion is the fact that it was no more difficult to memorize the more complicated structure than the simpler one. In a clear test of material devoid of meaning, the grammatical structure of sentences has no effect at all. The role of grammar, it seems, is not to be independent, but rather to be the overly dependent appendage our common sense has always told us it must be.

I wish to end my paper with a puzzle. The puzzle is this: When is behavior not
performance? The answer that Chomsky and his colleagues give us is, "When it is competence." I believe that behavior theory can more meaningfully deal with the problem that underlies the distinction between competence and performance. The distinction came about because people behave differently on different occasions; they speak differently and they respond to speech differently. My suggestion is that we study the conditions that provide us with differences in behavior, without worrying about which one reveals the real underlying ability of the individual. This means that we must define in precise ways the reinforcement contingencies that apply to language as behavior and to language as stimulus. It simply does not follow that a child who "knows" something in one situation will know it in another (Sigel, 1974). This is a familiar problem in all psychological tests where one must generalize from the test situation to school or job conditions. If we can solve the problem with respect to language, we might be making a contribution to testing as well. Only after studying language in relation to conditions of emission will we be on the way to gaining an understanding of the psychology of language.

It is my belief that we are ready for this approach, for we have come a long way from the Sturm und Drang of competence to the Ach und Weh of its nontestability.

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