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INSIDE THE BLACK BOX, WITH APOLOGIES TO PANDORA. A REVIEW OF ULRIC NEISSER'S COGNITIVE PSYCHOLOGY[1]

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It behooves us, as good citizens of the science of psychology, to shirk no area of psychology as long as we can apply scientific method to it. The research in cognitive psychology is certainly interesting, on the whole well executed, and very challenging. It is well within the scope of a behavioristic approach. It merely awaits more attention from behaviorists.

Is it true what they say about us behaviorists? Do we really believe that looking inside the organism is like opening Pandora's box? According to the myth, Pandora received a gift from Zeus. The gift was a box containing all the ills of humankind. When Pandora opened it, she released all the ills and in doing so supplied us with an early explanation of human behavior. I think it would be well to point out right at the start that even if John B. Watson suffered from a Pandora's-box complex, contemporary behaviorists have long ago peered inside with no ill effects at all. Even Watson himself was very much interested in proprioception, a form of stimulation that is certainly inside the organism. Behavioral research has not only theorized about stimuli inside the organism (*e.g.,* Schoenfeld and Cumming, 1963; Skinner, 1945, 1953, 1957) but has produced a good deal of exciting experimental work (Jacobs and Sachs, 1971; Kendler, 1971; McGuigan, 1966; N. E. Miller, 1969; Razran, 1971).

I have two objectives in writing this paper. The first is to acquaint those of us of a behavioristic persuasion with some of the theories, concepts, and data of cognitive psychology; the second is to show how radical behaviorism's concepts can address themselves to the problems that our cognitive psychologist colleagues have (temporarily) sequestered unto themselves.

THE COGNITIVE POINT OF VIEW

Cognitive psychologists believe that a behavioristic approach to psychology is grossly inadequate and too confining. Even Neisser (1967), who largely ignored the behavioristic literature, made some negative remarks about the approach. A mere generation ago, he tells us, "a book like this one would have needed at least a chapter of self-defense against the behaviorist position" (p. 5); now, apparently, it suffices to point out that radical behaviorists eschew categories, images, and ideas. Segal and Lachman (1972), in agreement with Neisser, spoke of the "demise" of behaviorism, its weakening, or its metamorphosis into neo- or non-behaviorisms. In an introduction to a symposium on cognition and affect, Antrobus (1970) claimed that radical behaviorists have given up the study of cognition. He went on to say that while some psychologists were content with the achievements of radical behaviorism (and he admits these achievements), there are "others whose curiosity can be contained neither by epistemological constraints nor by the achievements of radical behaviorism . . . they wish to know what internal events produce 'spontaneous' behavior in the absence of any particular external stimulus. What are the internal processes that make it possible for an individual to perceive, speak, think, recall, and dream?" (Antrobus, 1970, p. 2).

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Cognitive psychologists talk about two kinds of "internal" events when they reach into Pandora's box. The first is an internal *stimulus.* No behaviorist, contrary to what some cognitivists think, objects to the search for a real stimulus, no matter where it is. The second type of internal event, however, is quite different. It constitutes an answer to such questions as: What happens to the stimulus after it comes to the subject? How is it changed? How is it stored? Both the external initiating stimuli and the external responses are events that cognitivists tolerate because they are the ones that they measure; nevertheless, they would just as soon do without them. It is the second kind of internal event to which behaviorists object. Neisser (1967) called our attention to this kind of internal event in his definition of cognition: it "refers to all the processes by which the sensory input is transformed, reduced, elaborated, stored, recovered, and used. It is concerned with these processes even when they operate in the absence of relevant stimulation, as in images and hallucinations. Such terms as *sensation, perception, imagery, retention, recall, problem-solving,* and *thinking,* among many others, refer to hypothetical stages or aspects of cognition" (p. 4, author's italics). The confrontation between cognitive psychology and behaviorism is clear, for the italicized words are the ones that Skinner has for years avoided as hypothetical constructs. The question to which we will address ourselves is whether behaviorism can, by shunning these terms, still study the substantive area that is of interest to cognitivists. Even though the cognitivist professes an interest in structure and the behaviorist in functional relationships, both approaches try to account for all of behavior.

One final point by way of introduction. Why are we talking about the myths of yesteryear? Did we not show the bankruptcy of the Gestalt approach to psychology many years ago? Is there really anyone who would like to revive the confusion of introspectionism? The answer is that the cognitivist approach is no more the same as the Gestalt approach or introspectionism than the radical behaviorist approach is the same as Watson's. In both cases we are discussing only the predecessors of current thinking. In the same way that Neisser feels uncomfortable with the Gestalt concepts of the template and the memory trace, which suggest a copying process of the stimulus, behaviorists reject Watson's idea of the verbal report because it invests speech with a superior status, derived from the special entree it has to information that is somehow barred to nonverbal behavior. The present paper will not only point out those current aspects of cognition and behaviorism that are in direct conflict with one another, but it will also list some of the views that are no longer held.

Neisser speaks of construction as the central concept of his cognitive approach. "Perception is not a passive taking-in of stimuli, but an active process of synthesizing or constructing a visual figure" (Neisser, 1967, p. 16). He compares perception to the activity of the paleontologist who "extracts a few fragments of what might be bones from a mass of irrelevant rubble and 'reconstructs' the dinosaur" (p. 94). Essentially, Neisser proposes a theory of stimulus input → (analysis-by-synthesis) → response output, and although he admits that it is "little more than a metaphor", he justifies this approach by insisting that "a man who sees things that are not present must be constructing them for himself" (p. 95). In explanation, interestingly enough, Neisser rejects as quite naive the notion that a precise replica of the stimulus is stored inside the subject, but insists, nevertheless, that the information of the stimulus is stored and then constructed or reconstructed to give rise to perception, hallucinations, or remembering. The problem, however, stems not from the way the stimulus is followed inside the organism (although the proof or disproof of its travels is not immediately obvious) but rather from an incomplete analysis of the term "see". Skinner (*e.g.,* 1945, 1953) has often stated that such responses must be analyzed in terms of the stimuli (and there are many) that determine them. To take the term "see" as an example, an observer will require a relatively long time to recognize an individual who is transposed into a new context consisting of different clothing or of an environment in which he had not previously been encountered. This is a case in which some of the usual supporting stimuli, or parts of the original compound stimulus, are absent, resulting in a reduction of the response strength of "seeing". On the other hand, an observer will also "see" a person he has been waiting for, at some appointed time and place, who has not actually arrived. This is a case where a few strong stim-

uli (the fact of the appointment, experience with the person in the past coming on time, *etc.*) in conjunction with other parts of the compound (general appearance of the individual) produces a response we would not ordinarily call "seeing".

Neisser discusses this term with respect to Goldiamond and Hawkins's (1958) experiment. They produced "seeing" by presenting a number of strong discriminative associated but not principal stimuli. Before the experiment, subjects were given a number of nonsense syllables to read and it was explained that they would be presented again later for very brief exposure times; during the experiment only smudges were presented at high speed, to which subjects responded as if they were nonsense syllables. Goldiamond and Hawkins concluded that the typical perception experiments contained response biases reflecting the observer's tendency of *saying* rather than *seeing,* since in their own "non-stimulus" procedure "seeing" responses occurred quite regularly. I agree with Neisser in so far as he objects to separating saying from seeing. I prefer to consider saying as controlled by a continuum of stimuli. Although the retinal image is an important stimulus in determining what we "see", we often "see" in its absence. On the other hand, I disagree with Neisser's calling all "seeing" responses perception; "seeing" is simply a response under the control of *various* stimuli. If given a choice, I would prefer the term "saying" under varying conditions of stimulus control to "seeing" with varying degrees of justification.

Neisser explains visual imagery as follows: "My own usage will be as follows: 'visual image' is a partly undefined term for something *seen* somewhat in the way real objects are seen, when little or nothing in the immediate or very recent sensory input appears to justify it" (Neisser, 1967, p. 146). You have an image when you count the number of windows in your apartment or house without looking at it, or, for that matter, when you remember on what part of a page a certain bit of information can be located. Neisser cites the work of Antrobus, Antrobus, and Singer (1964) to show the importance of various motor components of "seeing" to explain images. In that experiment, more eye movements accompanied a subject's imagining an active (*e.g.,* tennis) than a passive (*e.g.,* an illuminated face in a darkened room) scene. With respect to dream imagery, Roffwarg, Dement, Muzio, and Fisher (1962) were able to match the dream content to the rapid eye movements measured while the dream took place. Thus, we have a relationship between the subject's motor activity component (response-produced stimuli) of seeing and what he "sees". Neisser summarizes (p. 153): "Visual synthesis of an image without eye motion may be possible, but the better the image the more likely it is to involve some sort of scanning." Then, having said that, he goes on to explain that the image is not merely motoric; it is also visual.

Neisser also rejects the notion that the brain stores a permanent record (a replica) of what the brain owner perceives; unlike Penfield, he views the electrical stimulation of the cortex as a way of producing what he calls perceptual synthesis. For Neisser, there are operations of synthesis that make use of the *information,* which is what is stored in the brain—always construction or synthesis.

In one very important sense the behaviorist agrees with this view: a stimulus does not become incorporated into the organism. It controls the organism's behavior. The organism neither copies nor categorizes the stimulus; it merely responds. Stimuli vary from time to time and because of the various effects of repeated stimulation (habituation, learning, extinction, *etc.*), even when the same stimulus occurs, early stimulation differs from stimulation that follows it. This implies that the same response may be evoked in different strengths or that altogether different responses may be evoked by stimuli that the organism experienced a different number of times.

The idea of construction is also forcefully brought to our attention in the context of verbal behavior. Psycholinguists, a large group of whom form a significant subset of cognitivists, invoke some sort of construction process to explain how one individual understands the speech of another. The most important adherents to an analysis-by-synthesis theory in speech are members of the Haskins laboratory. This group of investigators (*e.g.,* Liberman, Harris, Hoffman, and Griffith, 1957) found that, unlike the judgments of other physical stimuli, which vary gradually over the entire sensed continuum, the judgments of phonemes change abruptly from one to another in a categorical manner. The Haskins group con-

cluded that since the acoustic cues are inadequate to explain how an individual perceives speech, and since its perception is categorical (that is, various stimuli either are or are not members of a particular category), the perception of speech must take place with reference to articulation. In other words, when a subject hears a sound, he discriminates it from another one by matching it to the way he would produce such a sound. Since there are many obvious situations where a hearer could not do this fast enough, the Haskins group modified their theory to say that the matching takes place with respect to neuromotor commands rather than with reference to actual movements. They further bolster this theory by pointing to the greater efficiency (and parsimony) of having one mechanism to explain both the production and the perception of speech.

A number of investigators have criticized this theory. Lane (1965, 1967) suggested that the categorical perception phenomenon is not necessarily restricted to speech but relates to the type of conditioning paradigm involved in the learning of speech. When two stimuli are selected from a physical continuum and are made discriminative stimuli for two different responses, the same kind of abrupt change occurs from one stimulus to another, as is found in speech sounds. Studdert-Kennedy, Liberman, Harris, and Cooper (1970), however, were unable to produce categorical perception by a conditioning paradigm. In contrast, Kopp and Udin (1969) were successful in replicating Lane's procedure for the labelling of pure tones varying in frequency, while Pisoni (1971) concluded that the effectiveness of such training appears to vary from subject to subject.

Another interesting criticism of the motor theory of perception is contained in a paper by MacNeilage (1970). He rejected the theory because it would take at least 17,000 different motor commands to emit the phonemes, since their utterance differs not only with respect to identity but also as a function of which phoneme precedes and which follows it; and that omits such further common variations in speaking as stress, speaking rate, and segmenting. Instead of motor commands to explain speech production, he suggests an "internal specification of certain spatial targets". The metaphor is tennis: an individual's response depends on where he tries to direct the ball. Of interest to behaviorists is the fact that this kind of response specification is the way in which responses are commonly grouped, that is, in terms of their common effect on the environment (Salzinger, 1967). It suggests that perception of speech precedes its production; otherwise the effects would not control the responses. In any case, it contradicts Neisser's analysis-by-synthesis idea for speech.

But Neisser bases his construction interpretation of speech perception on other evidence as well. That evidence is based on Chomsky's (1957) transformation grammar, which made its first bow in linguistics and subsequently took over the fledgling field of psycholinguistics. Neisser begins his discussion of psycholinguistics by speaking of the "irrepressible novelty" of verbal behavior. Like Chomsky, he maintains that the uniqueness of sentences is characteristic of verbal behavior.

To explain the regularity of speech, he relies on structural rules for the formation of sentences. Such structural rules relate to the problem of ambiguity as well. In order to interpret an ambiguous sentence such as, "They are eating apples", the generative grammarian psycholinguist insists that you must know its structure. In one structure the word "eating" is an adjective; in another, it is a verb. Diagrammatic analysis makes clear that these two structures are possible. It is known as phrase structure analysis and it describes the surface structure of utterances. However, there are sentences that cannot be understood without delving into the "deeper structure". Thus, they also posit a common deep structure, such as the one underlying all of these sentences: "He appreciated Chomsky's theory." "Chomsky's theory was appreciated by him." "He did not appreciate Chomsky's theory." "Did he appreciate Chomsky's theory?" "Didn't he appreciate Chomsky's theory?" "Wasn't Chomsky's theory appreciated by him?"

Miller (1962) summarized data in support of the notion that a listener recorded the basic kernel (best approximated by the simple declarative sentence of these various sentences, with a footnote as to how to transform it into the particular sentence that he heard. Those sentences that took a larger number of transformations, such as the passive negative question, resulted in the greatest distortion in memory because they ostensibly required the

largest number of footnotes. Some psycholinguists have even suggested that such structures are built into the brain of the speaker/hearer so that the simpler structures always result in easier comprehension and better recall than the more complex structures. Following Chomsky, they have maintained that the grammatical structure has nothing to do with the meaning of sentences; the two aspects of verbal behavior were said to be independent of one another.

The behaviorist takes a different approach to the problem of ambiguity. To begin with, ambiguity in normal conversation is probably quite rare. Note, we are not saying that misunderstanding is rare; it is relatively frequent. But under normal circumstances the stimuli surrounding the emission of speech generally ensure that any sentence will lead to a particular response on the part of a hearer; the response itself is made without uncertainty, even though the hearer's interpretation may well be wrong. Exceptions occur when ambiguity is purposefully employed to befuddle or amuse the audience, which is probably why ambiguity is maintained in the language rather than becoming extinguished. It sometimes pays to be ambiguous; thus, one can insult an individual without incurring the consequences of his wrath, since such sentences are weak discriminative stimuli for responses that are aversive to the speaker.

The two explanations for ambiguity bring up the controversy over Chomsky's contention that knowledge of grammatical structure is sufficient, without meaning, for understanding sentences. To test Chomsky's idea of the independence of grammar and meaning, Salzinger and Eckerman (1967) constructed the simplest kind of sentences (simple, active declarative sentences) and much more complicated ones (passive negative questions) out of nonsense syllables and function words and tested for differences in recall. An example of the simple declarative sentence in the active mood was: "And the piqy kews were beboving the nazer zumaps dygly." An example of the same sentence in the passive negative question form was: "Weren't the nazer zumaps dygly beboved by the piqy kews?" Each of the forms was also represented in a random order; various groups of comparable subjects were given the opportunity to learn two different sentences in the same form.

The results showed a number of significant differences. The function words (*e.g.,* by, the) were more easily recalled than were the nonsense words; sentences were more easily recalled than random arrays of items; and the second sentence that a subject learned was more easily recalled than the first. In contrast, differences due to grammatical structure, that is, differences between the simple declarative sentence and the more complicated one, failed to emerge. Furthermore, the small, statistically nonsignificant difference due to sentence types, that existed for the first sentence, vanished entirely for the second sentence of the same type. Finally, the small difference that existed for the first sentence expressed itself only in the *randomly* ordered strings, rather than in the "correctly" ordered sentences.

The results are rather clear. Subjects recalled better those kinds of strings of verbal items to which they had more frequently been exposed. Thus, although there was a tendency for the simple declarative sentence in the active mood to be more easily recalled than the passive negative question (a difference probably reflecting relative frequency of exposure and/or emission), this difference quickly disappeared after a few trials of learning (in which frequency of exposure of the initially infrequent sentence type was increased). Surely this cannot reflect an *innate* structure that forces a subject to recall material in terms of a kernel sentence plus some notation on the number of transformations needed to produce the sentence form required. The more parsimonious interpretation, that differences among sentences varying in "structure" merely reflect a difference in frequency of exposure, is much more cogent. The question as to why some sentence structures do in the first place occur less frequently than others can also be answered in terms of known psychological principles from behavior theory, without having to turn back to innate ideas.

Cognitivists believe that language is "structured". Neisser presents it as an obvious case of analysis-by-synthesis. How else, he asks, can we understand so-called embedded structures? "The man who came to dinner is a friend of mine." Here the words "is a friend of mine" must not be uttered until the intervening verbal behavior "who came to dinner" has been emitted. The generative grammarians tell us that in language one can increase the amount

of embedded material indefinitely (if only it weren't for man's limited memory). This kind of approach neglects the importance of the consequences of behavior. If what we said made us forget how we began to say it, the consequence would be an absence of positive reinforcement (such as getting what we were asking for, or, for that matter, merely retaining the hearer's attention) and extinction of such behavior.

Neisser also treats the complex problems of memory and thought. He begins by rejecting what he calls the Reappearance Hypothesis. It states that a memory can be locked up inside the organism and produced at a later time as needed. As a substitute for it he suggests the Utilization Hypothesis, according to which remembering takes place "after an elaborate process of *re*construction" (Neisser, 1967, p. 285, author's italics). The person who recalls does so by using the "traces of *prior processes of construction*" (p. 285). This, it should be noted again, is an active process that Neisser places inside the organism.

The criticism is obvious. Where is the locus of control for such an internal process? In fact, the question of control is not answered by such models, merely postponed—or, if the theoretician is clever enough, quite obscured. Let me say immediately that Neisser is aware of the problems involved. He admits that a *homunculus* is "unpalatable", but then he adds that it cannot be avoided. "If we do *not* postulate some agent who selects and uses the stored information, we must think of every thought and every response as just the momentary resultant of an interacting system, governed essentially by laissez-faire economics" (p. 293).

Neisser then posits two stages of remembering: one he calls "primary process", a sort of crude process, and one he calls "secondary process", which includes an active agent within the organism that manipulates the information toward some end. This active agent is analogous to the executive routines of the computer. The executive decides which subroutines to use, but it is not itself used by any other higher executive. Apparently what Neisser means by this secondary process is the way the built-in capacities of the organism respond to the environment. Finally, at the end of his book, Neisser expresses regret that cognitive psychology is after all incomplete. It cannot take into account motivation. "However, the course of thinking or of 'inner-directed' activity is determined at every moment by what the subject is trying to do. Although we cannot always see only what we want to see, we can generally think what we like" (Neisser, 1967, p. 305). Free will is simply taken for granted.

THE BEHAVIORIST ANSWER

So much for the first objective of my paper. I have tried to present some of the leading ideas in cognitive psychology, at least as viewed by a leader in that field. Along the way I could not entirely refrain from commenting or even arguing against some of the concepts. Now I will try to deal with the cognitive *versus* behavioristic approaches in a more general way.

First, it is important to understand what behaviorism is willing to include and what it insists on excluding in its approach. Cognitivists have made much of feeling confined by behavioristic principles. Lest some of us take these criticisms as merely the statements of those who do not understand behaviorism, let us hear them again from an unimpeachable source. In an article on "The experimental analysis of behavior (TEAB)", Kantor (1970), a foremost behaviorist himself, urges us "to include the free investigation of human organisms". Although the implication that no human work had been done is not entirely just, it is true that most behaviorists have failed to work with human subjects, thus avoiding the kinds of problems regularly met by cognitivists, who in turn have confined their work to human beings. Kantor states (p. 103) that "TEAB methods and postulates" must be applied to the "investigation of all types of adjustments including perceiving, remembering, thinking, and feeling behavior among other classes as performed by organisms of all genera and species."

Let us not, however, conclude that the study of human behavior has been entirely avoided. Skinner (1953, 1957, 1968, to take but a few of the most outstanding examples) has for a long time had quite a bit to say about human behavior. What we need now is more experimental work. I say this without in the least trying to slight the very interesting work that many behaviorists have in fact done in the areas of abnormal psychology, in programmed

instruction, in perception, in verbal behavior, *etc.,* and I say this despite the fact that almost all of my work has been with human beings.

Among the concepts that cognitivists accuse behaviorists of excluding, perhaps the most important is the image. Aside from Skinner's own interest in private events, there exists a compilation of studies in this area by McGuigan (1966) and a recently edited volume by Jacobs and Sachs (1971), which focussed on a variety of private events that are susceptible to experimental analysis. Experiments in this area have shown that one can condition images, hallucinations, or whatever name one gives them (*e.g.,* Ellson, 1941; Leuba, 1940; Leuba and Dunlap, 1951; Hefferline and Perera, 1963). In the last-cited study, the subject reported the existence of two tones when one was produced by conditioning and the other was presented a fraction of a second later. Furthermore, Staats (1967; 1968) made use of the conditioning of images as a central aspect of his theory of the acquisition of meaning. Even Paivio (1971), who rejects the conditioning model as a complete explanation for the image, admits that no explanation would be complete without it.

Behaviorists are quite comfortable with the concept of image—as long as it is viewed as a stimulus to be measured and manipulated rather than as a *post hoc* attempt to explain a result, without even the potential of measurement. This is not to say that the problems of measurement of images (or, for that matter of other private events) have by any means been completely solved. Behavior is multiply determined and the subject's verbal response, "I see an image" may simply be determined by reinforcement contingencies unrelated to an image as a discriminative stimulus (S^D); the critical S^D may be the instructions of the experimenter, especially if the reinforcer is large enough in magnitude. Paivio's (1971) experimental work is certainly intriguing but his operational definition of imagery in terms of a rating scale is not entirely satisfying. Nevertheless, the large amount of supporting data amassed by Paivio and his students for the role of imagery in learning and comprehension cannot be ignored any more than the reality of private events.

One final point about images. It is a logical extension of the concept of stimulus control, to move from public events in the external environment to private events that are response-produced. The highly sophisticated methods of operant and respondent conditioning should certainly be useful in shedding some light on the acquisition of images, their evocation, their maintenance, and their loss. Research on animals would do a great deal to aid us in understanding the image, since no amount of verbal fudging (whether intentional or not) that sometimes creeps into experiments with human beings through instructions or biased analysis of subjects' verbal responses can enter an animal experiment.

Cognitivists pose as an additional problem for the behaviorist the phenomenon of spontaneous behavior—the behavior that is not, Neisser tells us, controlled by "relevant" stimulation. The solution is that behaviorists no more believe in responses without stimuli than they believe in free will. Behaviorists assume that responses are controlled by stimuli. The control may not always be obvious; it may not always be strong; in terms of today's technology, it may even be impossible to specify the particular stimulus that is controlling a particular response. But stimuli do control responses. It must be added that behaviorists are not interested in predicting the precise member of a class of stimuli that controls the precise member of a class of responses; their interest is in predicting what stimulus classes control what response classes. Contrary to Neisser's contention, there is no such thing as an irrelevant stimulus controlling a response. If a stimulus controls a response, then it is by definition relevant. To describe an hallucination as controlled by irrelevant stimuli is to make the value judgment that it is better to be influenced by one's retinal image than by a conditioned stimulus in seeing. This approach leads away from the experimental analysis of the phenomenon. We must never discard stimuli that we think ought not to control responses; we must find out how they, like the supposedly more appropriate stimuli, come to control the responses, how they maintain control, and how their control can be reduced.

Cognitivists believe that behaviorists confine their interest to atomistic responses and stimuli. Contrary to this belief, behaviorists define stimulus and response as classes (Skinner, 1935). It is this concept of response *class* that makes some of the criticisms of the behavioral approach quite beside the point. G. A. Miller

(1962) has set up the straw man that there are too many combinations of words for a child to learn to emit all the sentences he will eventually say, without benefit of generative rules of grammar. But behaviorists (Skinner, 1957) never did discuss single responses only. Language is acquired in the form of response classes and their interrelationships. In 1967, I devoted a paper to a discussion of verbal response classes. In 1968, Chomsky (1968, p. 87) responded: "Unfortunately, this is empty verbiage until the condition that defines membership in this class is established." My article did go about the job of defining some of the many conditions that define membership. Single words as well as combinations are members of a vast variety of different response classes. The word "table", for example, is a member of response classes defined by the following controlling operations: physical discriminative stimuli—What is this (point to a table)?; verbal discriminative stimuli—Complete this sentence: He put the chairs around the _____; List as many words as you can beginning with the letters ta_____; On what do you place a plate of food?; List some examples of furniture; Read some material containing the word "table" in it; Describe the furniture you have in your dining room; *etc.* Obviously a verbal response can be a member of many different classes; and so, incidentally, can nonverbal responses. Neither verbal nor nonverbal responses require structural analysis. The hand movement you employ to squash a mosquito may well be the same topographically as the one used to call a dog, applaud a famous singer, or express joy, *etc.*

As to the problem of units of verbal behavior, behaviorists know that verbal response units are indeed quite variable from time to time and from occasion to occasion (see Salzinger, 1973). Like the definition of response class, the definition of response unit is functional. Under certain circumstances the response, "Fire!" may have the same function as (*i.e.,* be considered a single integral unit as much as) "Hey, I think there's a fire there!" On the other hand, it is equally clear that the word "fire" in the sentence above is not a single integral unit. We do not need a generative grammar to provide us with definitions of response classes even when we talk of sentences, as Chomsky (1968) implies. Some of the differences attributed to "structure" are better explained in terms of differences in image (Paivio, 1971). We must add to this the important fact that people emit verbal behavior in nonsentences, thus making the entire generative grammar effort incomplete, at best, for the analysis of speech. For additional criticisms of the generative grammatical approach the reader is directed to Goodman (1967), Quine (1970), Salzinger (1967; 1970), Staats (1971) and Verhave (1972). Critics of generative grammar have recently been joined by linguists, particularly with respect to the problem of semantics (see, for example, Steinberg and Jakobovits, 1971).

The final question is: is the trip inside the organism really necessary? Note that the trips inside are not charted by physiology or biochemistry. Neisser is no more interested in those topics than are most behaviorists. As an experimentalist, he collects data in such a way that one can replicate his experiments and interpret his results because his data are reliable and available. Trips inside may be viewed as small excursions not critical with respect to evaluation of the data, *i.e.,* not critical, to complete the metaphor, to determine where the trip ends.

There is another way to ask the question about the necessity of delving inside the organism: what function do the "inside" concepts have? They are obviously useful to at least some people in organizing the next experiment or the next theoretical question. In that way, such concepts may stimulate experimenters to think in novel ways. Novelty in science should always be fostered.

There is, however, one way in which some of the cognitive concepts are deleterious: in supplying answers by naming problems rather than by investigating them. In so doing they fail to promote further needed research. As an example of this, consider the fact that the concept of competence, which is so dear to generative grammarians and has done so much to keep alive their theory of grammar, has actually proposed that the speaking behavior of the individual is not to be trusted for information as to how well the individual speaks!

In reply to my criticism (Salzinger, 1967) that the notion "grammatical is equivocal as a scientific concept because there is no agreement among subjects in classifying sentences in this manner", Chomsky (1968) rejected the empirical evidence rather than the concept.

"Obviously, the failure indicates nothing more than that the tests were ineffective" (Chomsky, 1968, p. 88). However, a concept should be capable of empirical refutation if it is to be retained in a science. The concept of competence presents a similar problem. It also denigrates the value of data without providing anything better than the investigator's opinion in opposition.

We must, instead, deal with the important observation that organisms emit different behaviors under different circumstances. Thus, when we say that an individual emits ungrammatical sentences but "recognizes" that they are ungrammatical, thereby revealing his competence in the language, we are merely singling out one class of responses to one set of discriminative stimuli and asserting that it has priority over another for exhibiting his "underlying" ability. The arbitrary character of this procedure emerges clearly when we consider an individual who emits grammatical sentences but fails to "recognize" them, for he would be considered less competent than the individual mentioned above.

Finally, a word about the concept of construction, since it is central to Neisser's book. Despite Neisser's protestations to the contrary, he requires the concept only because he accepts a theory very much akin to the Reappearance Hypothesis. If he gave up the notion that any part of the stimulus has to be incorporated into the organism, he would not have to talk about constructing a stimulus that is not there, not to speak of reconstructing such stimuli for purposes of recall. Memory, or more concretely, recall, must be evoked by stimuli, some of which are external and some internal. Stimuli evoke responses that vary in strength. The way in which an organism is modified after learning is in its responding to a stimulus it did not formerly react to.

The difference between perception and memory does not reside in the concepts of construction and reconstruction. Rather it more typically inheres in the strength with which the evoking stimuli control the responses that interest the experimenter. For in perception, a subject responds to a stimulus manipulated by the experimenter at the time he is required to respond, whereas in memory, a subject is required to respond to a related (part of the) stimulus some time after the earlier perceived stimulus has been presented.

I don't quite know how one proves that it is less elegant to describe subjects as "constructing" stimuli to which they respond than to think of them as varying in sensitivity in responding to various stimuli with various responses. I do believe that my speculation is more parsimonious in that it requires only a change in how responses are evoked rather than a change that consists essentially of having organisms incorporate ever larger and larger and more intricately organized chunks of the external environment as they grow older. On the other hand, I am not certain that Neisser's experiments would be any different if he had used my model instead of his. Are theories of cognition necessary?

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