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A Call for Tutorials on Alternative Approaches to the Study of Verbal Behavior

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In conversations with Hank Schlinger, the new editor of *The Analysis of Verbal Behavior,* concerning the future of the journal, I suggested that he solicit special tutorial articles on topics in related fields to supplement the empirical and theoretical articles that are the mainstay of the journal. He requested that I submit a written proposal to that effect, to be shared with the readership.

Most of the readers of this journal are professional behavior analysts with a general interest in the extension of fundamental principles to all behavioral phenomena; only a handful, I dare say, are specialists in verbal behavior. Consequently, few of us are well read in the vast literature of closely allied fields, to say nothing of traditional linguistics, psycholinguistics, or developmental psychology. Because our approach differs so fundamentally from that of traditional scholars, much of this literature will be of little use to us. But not all: I have commonly found that, if one sets aside the theoretical interpretation of an article in, say, psycholinguistics or cognitive psychology, what remains is a kernel of useful or at least provocative data. If we, collectively, are to provide a comprehensive interpretation of verbal behavior, we must avail ourselves of all of the relevant data. Of secondary importance, if we are to persuade those outside our field of the validity of our approach, we must at least know the kinds of questions that trouble them. Because it is hopeless for an individual to master all of the relevant literature, I suggest that we encourage the submission of review articles, book reviews, and tutorials by anyone with expertise in both behavior analysis and some portion of the traditional literature. Such reviews should not only present relevant data and identify sources for further reading but should indicate the relevance to a behavioral interpretation of language.

Should such reviews be solicited from experts? Perhaps no behaviorist can write about another discipline with the authority of an expert, but it is quite unlikely that an expert in, say, language development, neuropsychology, or the anatomy of the vocal tract would be able to identify the critical features of his or her field that have special relevance to a behavioral approach. Behaviorists are more likely to identify just those elements of an alternative approach that are useful to us than any expert. A pig is better at sniffing out truffles than a mycologist is.

The journal already welcomes papers of this type. Knapp (1997) and Mabry (1994–1995a, 1994–1995b) have reviewed books in the field of linguistics. The review of the literature of chimpanzee language training by Hixson (1998), the exploitation of signal-detection theory by Robbins, Layng, and Karp (1994–1995), the special section on artificial intelligence in the 1992 volume (Cherpas, 1992; Schlinger, 1992; Stephens & Hutchison, 1992), the article on memory by Delaney and Austin (1998), Schoneberger's (1991) comparison of three philosophical positions, and Cherpas' (1998) introduction to perceptual con-

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trol theory were all written in the spirit of the present proposal. I am suggesting that we actively encourage more papers of this sort, even to the point of actively soliciting papers on particular topics. I am not suggesting that we open the journal to research articles from incompatible paradigms, rather that we merely become better informed about such research.

Several topics suggest themselves to me as candidates for such reviews. My list is entirely idiosyncratic and should not be viewed as prescriptive. In many domains I am too ignorant even to think of examples. The reader will no doubt think of many others.

Priming: Findings and Procedures

I am persuaded that autoclitic frames play a central role in structural regularities in language (grammar). An individual who has heard many examples of *Where did X go?* or *I gave the X to Y* will utter such expressions with novel elements (Place, 1992; Skinner, 1957; Stemmer, 1990). A central challenge for our field is explaining the fluctuations in stimulus control as such expressions unfold. Much of the control is intraverbal, of course, but what is the role of novel elements in an intraverbal string? In the first expression, is the element *go,* preceded, as it is, by a variable, under intraverbal control of the rest of the frame? Can intraverbal control span a variable element? If *go* is under intraverbal control of the preceding elements of the frame, why does it not preempt the element *X,* which, as a variable, cannot be under such intraverbal control? Such questions are not entirely hopeless, but our answers to them are speculative (e.g., Palmer, 1998). However the matter is not entirely beyond empirical inquiry. Cognitive psychologists make much use of reaction times to study a set of phenomena they call *priming.* Some priming phenomena are clearly examples of intraverbal control. In a typical procedure a word is briefly presented to a subject on a screen, followed shortly by a second word. The subject's task is to respond appropriately to the second word in some way (say, by reading it aloud, or identifying it as a word, or confirming that it rhymes with some target word, such as *story*). The measure of interest is the latency to respond in the required way to the second word relative to the latency to respond in a control condition. It is typically found that stimulus words reduce the latency to respond to target words to which they are intraverbally related (e.g., Balota & Lorch, 1986). (In some cases, and at some temporal parameters, interference may be observed.) Thus seeing *Washington* would reduce the latency to read *Monument.* Such findings are commonly used to support structural models of cognition that would be of little or no interest to a behaviorist, but some of the data would stand on their own. A single stimulus word potentiates a variety of target words, some of which are physically similar to the stimulus word, some of which are intraverbally related, and some of which are semantically related (i.e., they evoke common responses in a listener). A single event has a wide variety of behavioral effects that occur in parallel. On any given occasion only the strongest response is actually emitted, obscuring the variety of effects. Findings such as these may support and help to guide our interpretations of verbal behavior, problem solving, and other complex behavior.

Perhaps more important, the experimental procedures themselves might be useful to us. Response latency in such procedures may serve as an index of response strength. As Skinner (1957) noted, strength is difficult to evaluate in verbal behavior because the usual measure, response rate, is of only limited use. Under most conditions, the verbal community tends to punish repetitive verbal behavior. However, on the assumption that a reduced latency is a measure of the strength of intraverbal control (an assumption that should not be accepted uncritically), it might be possible to track intraverbal control

over elements of an intraverbal frame. In the first example above, under what conditions is the element *go* strongest? Is it strongest immediately after *did* or after *did* plus another arbitrary stimulus? Will anything (white noise, a cough, a nonsense syllable) serve as this arbitrary stimulus or does it need to be a "noun phrase"? (If the latter, we would be faced with a new problem: What, in behavioral terms, does it mean to say that something is a noun phrase?) However tentative our interpretation of such research would be, we should recognize that the problem being addressed is one of the most formidable in the field of verbal behavior.

Structure and Function of the Vocal Apparatus

Some properties of verbal behavior are constrained by the structure of the vocal tract and the respiratory system; others are perhaps constrained by the architecture of the nervous system. In the search for generality in our principles of behavior, we typically ignore the constraints imposed by the forearms of rats and the necks of pigeons, but in the present case we are explicitly concerned with the behavior of a particular species; response topography is relevant.

As one example among many, such considerations may cast light on the sentence as a unit of analysis. To linguists, the sentence is a formal unit, defined as such by one's favorite grammar. Skinner (1957) dismissed such formal units out of hand, and his analysis offers an alternative explanation for many strings that would be considered sentences in everyday parlance. Place (1992, 1998) is perhaps unique among behaviorists in arguing that the sentence can not only be defined functionally, but that it is, in some sense, the fundamental unit of verbal behavior, for it is typically the sentence that specifies contingencies to which a listener can profitably respond. (Note that this view of the sentence is entirely incommensurate with the linguists' definition: Linguists are unperturbed if their grammar generates strings a thousand words long or strings that are gibberish to the ears of a listener.)

Lieberman (1984) observed that spoken verbal behavior occurs in phase with respiration and that sentence boundaries commonly correspond to phase changes in respiration (i.e., from exhaling to inhaling). That is, we usually wait until we finish a sentence before inhaling, sometimes to the point of discomfort. This is a curious regularity, quite at odds with a formal analysis of language, and one that calls for a behavioral interpretation. Is it the contingency-specifying property of sentences, of which Place speaks, that regulates respiration when we speak? Why do we hurry to finish a long sentence before inhaling? What are the costs to the listener of pausing for breath in the middle of a sentence? Are there costs for the speaker as well? Perhaps the answers to such questions will lead to a better understanding of the role of prosody in verbal behavior.

The psycholinguists have discovered some curious regularities between response form and meaning. Different vowel sounds are the result, in part, of the size of the resonant cavity in the mouth. When the tongue is low and in the back of the mouth, the resonant cavity is relatively large and low-frequency speech sounds are amplified; when it is high and in the front, the resulting resonant cavity is small, producing vowel sounds in which high frequencies have been amplified. Pinker (1994) points out that in English "and many other languages," the latter sounds are found in words associated with small things, the former with words associated with large things; thus, *teeny, bitty, squeak,* and *tweeter* versus *humongous, thunder, roar,* and *woofer.* Moreover, in that large collection of sing-song nonsense words that differ mainly in the vowel, such as *sing-song, spic and span, hippity-hop, tic-tac-toe, eeny-meeny-miney-moe, tit-for-tat, clickety-clack, fiddle-faddle,* and many other examples (see Pinker,

1994, p. 167), the high-frequency sound always comes first. In addition,

> Words that connote me-here-now tend to have higher and fronter [sic] vowels than verbs that connote distance from "me": *me* versus *you, here* versus *there, this* versus *that.* . . . Words that connote me-here-now tend to come before words that connote literal or metaphorical distance from "me": *here* and *there, this* and *that, now* and *then, father* and *son, man* and *machine, friend* and *foe.* . . . The syllogism seems to be: "me" = high front vowel; me first; therefore, high front vowel first. (Pinker, 1994, pp. 167–168)

Pinker suggests that this tells us something about how the mind works and its rationale for producing vowels in different ways; in contrast, we would search for a behavioral interpretation. Perhaps it is a combination of onomatopoeia, stimulus generalization, and modeling; *squeak* sounds like a mouse, *tweet* like a bird, *roar* like a lion, *thunder* like thunder; when neologisms are invented they follow the pattern of words whose controlling variables share some properties; thus *pitter-patter.* Once a pattern becomes established in a verbal community it gets passed from speaker to speaker and from generation to generation. I have no investment in this particular interpretation, for I do not possess all of the relevant facts. Unfortunately, the relevant facts, to the extent that they are known, are found in the psycholinguistic literature, not the behavioral literature. We should make them our own.

Neuropsychology

Neuropsychological data must necessarily be consistent with behavioral data. Skinner (1957) made effective reference to aphasics to support his functional classification of verbal operants:

> When a man can pronounce a word "after" the physician but cannot use it for practical purposes, or cannot name an object upon demand but soon uses the name in another connection, or cannot *read* but can follow written instructions, or can follow written instructions only after reading them aloud, a functional classification of verbal behavior is dramatically set forth. (p. 218)

In my experience, most people find such examples persuasive—perhaps more persuasive than is justified, for neuropsychological data have been adduced to support wholly incompatible analyses of language (e.g., Harley, 1995; Pinker, 1994). Indeed, the devil can cite scripture to his purpose. Nevertheless, there are independent reasons for preferring a behavioral account, and neuropsychological data can complement that account. A review of relevant examples culled from the last four decades of case studies would be helpful in documenting and extending Skinner's casual allusions to such cases.

Data from brain imaging studies will also complement a behavioral interpretation of language. It has been shown, for example, that nonsense words evoke neural activity only in sensory and sensory association cortices, but "meaningful" words evoke activity in the motor association cortex as well (Peterson, Fox, Snyder, & Raichle, 1990). Meaningful words are discriminative stimuli that evoke behavior, and a word's "meaning" is to be found in the relevant history of discrimination training. As in the case of neuropsychology, such findings are unlikely to inform a behavioral interpretation, but an integrated behavioral and biological account may be more persuasive to the skeptic than a behavioral account alone.

Prosody

What is the role of cadence and stress in verbal behavior? Prosody is both a stimulus property and a response property of verbal behavior, and I believe that it plays an important role in the rapidly shifting stimulus control of verbal operants as we speak. Certainly it helps the listener. Text read in a perfect monotone (such as the automated speech of the telephone exchange) is notoriously hard to follow.

But it may be even more important to the speaker. Linguists make much of the observation that speech is hierarchically organized into structured strings and that, therefore, a linear, moment-to-moment account of language (a behavioristic straw man) is doomed. However distasteful we find such characterizations of a behavioral account, the data must be respected. Consider the following sentence, which appears in a book by Peggy Noonan, one of Ronald Reagan's speechwriters: *A congressman this guy I know knows lives here* (Noonan, 1990, p. 96). In this example, three independent clauses are nested, with the noun phrases clustered at the beginning and the verb phrases at the end. Such constructions are challenging to both speakers and listeners, and they are challenging as well to a stimulus control interpretation of verbal behavior. A skillful reader will inflect the sentence in such a way that some of the clause boundaries are accentuated, pausing slightly after *congressman* and *knows* and stressing *congressman* and *lives.* It is at least a plausible hypothesis that prosody offers crucial stimuli in "spontaneous" speech production as well.

Consider the following utterance, slightly modified from one I overheard on campus the other day: *I invited Diane—even though she still hasn't paid me back for the pizza and beer—to come with us.* The fragment *to come with us* is presumably evoked intraverbally by *invited,* but does not occur until both the direct object (a variable) and a parenthetical intrusion have been completed. It is dammed up, as it were, and only comes forth when other elements have been emitted. To put the problem loosely, how does the speaker "know" when the parenthetical expression is complete and that it is time to conclude the intraverbal frame? It is tempting to solve the problem at a stroke by resorting to the speaker's "communicative intentions" or expressions such as "completing a thought." Even so, we seem to need a homunculus that acts as a traffic cop, waving on some expressions and holding back others. What are the environmental and behavioral events that displace the homunculus? One possible variable is prosodic. If intraverbal frames have a characteristic cadence, the boundaries of an expression would be marked by a pattern of stresses and pauses. Such cues could serve as discriminative stimuli that control subsequent expressions.

When uttering subordinate clauses and parenthetical intrusions, we may impose a cadence just to mark its boundaries. Suppose, in the example above, the parenthetical comment had been any of the following:

—even though she still hasn't paid me back for the pizza and beer—
—even though she still hasn't paid me back for the pizza—
—even though she still hasn't paid me back—
—even though she still hasn't paid—

Note that in each case the last word is stressed and is followed by a pause; the stress on these words is relatively weaker when they occur in the middle of the expression. Thus *paid* is stressed more in the last example than in any of the previous three. Whatever other function this pattern might serve, it marks the boundary of a verbal unit. Thus, prosody might be one of the variables that organize structural regularities in verbal behavior, with this advantage over the corresponding formal units of the linguist: Prosody has physical properties.

Stress serves other functions than marking boundaries of units of verbal behavior. We stress compound nouns differently from adjective-noun sequences; thus *the greenhouse* versus *the green house, the bluebird* versus *the blue bird, the top-hat* versus *the top hat.* Moreover, special emphasis on a word can change the verbal contingency entirely. The following utterances all have a different effect upon a listener:

CAN you hear him?
Can YOU hear him?
Can you HEAR him?
Can you hear HIM?

The study of prosody, unfortunately, has been left entirely to the psycholinguist. I am aware of no behavioral research on these topics. The reader who wishes to explore the role of prosody will find Lieberman (1984) a helpful reference.

Other Topics

I have identified several topics that I believe would nourish a behavioral interpretation of verbal behavior, and I have tried to discuss them in just enough detail to persuade others of their relevance. However, I am perhaps no better informed about the range of suitable topics than other readers. Many other candidates suggest themselves. First, there is the enormous literature on language development. The relevance of the data from this literature can scarcely be questioned, because it is likely that the relationship between verbal behavior and its controlling variables is clearest when it is being first acquired. The text by de Villiers and de Villiers (1978) is a well-regarded reference and is relatively sympathetic to a behavioral viewpoint.

Second, the field of linguistics, the domain of some of behaviorism's sharpest critics, should be sorted carefully for useful nuggets. In my experience, these nuggets will appear in the form of curious regularities in verbal behavior that are easily overlooked by the untrained observer (cf. Palmer, 1998). The theoretical edifice of linguistics, can, I think, be discarded, but some of the data are perplexing and ought either to be put into good order by the reviewer or set out as challenges for others.

Third, the topic of second-language acquisition deserves attention. It is commonly noted that acquiring fluency in a foreign language is much more difficult for adults than children, and it is speculated that there is a "critical period" after which complete mastery is impossible. What are the data that support this claim? Are there more parsimonious interpretations? A closely related literature explores polyglot immigrant communities and the special dialects, argots, and pidgins that frequently develop in such communities. Because many such verbal communities are both relatively new and small, the contingencies that shape verbal behavior may be more conspicuous than with established languages.

Some approaches to the interpretation of language are more compatible to our own than others. Theorists in pragmatics, conversation analysis, and speech acts, for example, typically explore the contingencies of verbal exchanges much as would a behaviorist. To what extent can these positions simply be translated into behavioral terms? If there is a residue, why?

It is perhaps pointless to predict whether a given topic will yield useful data or help to flesh out a behavioral interpretation of verbal phenomena, and it is certain that no individual is in a position to explore all of the relevant literature. Nevertheless, intellectual integrity requires listening to alternative points of view. But more important, our own field will advance most surely if we exploit all available resources, including relevant portions of the traditional literature.

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