Do Priming Studies Reflect Fundamental Behavioral Processes?

"Priming" refers to the effect of one stimulus on the responses to a second. As this chapter makes clear, a variety of procedures have been used to investigate both potentiation and interference. In ***LCB***, *footnote 23, p 265* we briefly describe the "lexical decision task," one of the most widely used priming procedures in normative cognitive science. As noted, in this procedure the subject must respond to the second of two visual stimuli by pressing one key if the stimulus is a common English word and a second key if it is not. On average, subjects respond more quickly to the second stimulus if it has been primed by a word that is related to it in any of several ways.

The relationship of greatest interest, for our purposes, is the intraverbal relationship. Words are intraverbally related if they have commonly occurred together in one's history, either in the speech of others, oneself, or between speakers. (See Chapters 10 and 11.) For example, "You're welcome" is intraverbally related to "Thank you," and "black" is intraverbally related to "pitch." Intraverbal relations appear to play an important role in vocal speech, for speakers hear themselves as they speak. One's utterances exert intraverbal control over subsequent utterances. Such effects may play an important role in the systematic ordering of verbal operants conventionally called "grammar."

To the extent that priming studies measure intraverbal control, they may be an important tool in the behavioral interpretation of grammar. However, as the study reported below suggests, not all priming procedures reflect fundamental behavioral processes. In particular, the lexical decision task appears to be too complex to do so.

Consider the procedure moment-to-moment in time: A priming stimulus is presented in the middle of a blank screen. The subject orients to it and presumably emits a covert discriminative response. (That is, he "reads" it.) The ambient stimuli, quickly receding in time, are the visual pattern on the screen and any stimulus properties of the subject's response. Then a second stimulus appears. The subject orients again and responds a second time. Of primary interest is the latency to make this second response. However, in the lexical decision task, the response that is measured is a later response, namely, a key-stroke to one key or another. What controls the key-stroke? If the second stimulus is a familiar word, it will evoke a strong pattern of discriminative responses, whereas if it is a non-word, it will evoke discriminative responses only weakly. The key-stroke is a differential response to the two kinds of responses. However, this final discrimination might be difficult, and in any case we would expect some variability from trial to trial. The difference between RABBIT and XBQKPJ might be more salient than the difference between GAMBIT and JANDOR. That is, some words and non-words are more quickly discriminated as such than others. Thus the lexical decision task confounds the measure of the intraverbal relationship between two words with that of a highly variable third task.

The following is an abbreviated version of a research report that appeared in the 2005 edition of VB-News, the newsletter of the Verbal Behavior Special Interest Group of the Association for Behavior Analysis. The paper was written for an audience assumed to be ignorant of the content of Chapter 9 but familiar with some technical terms introduced in Chapter 11. Although the study was a pilot study designed to test the equipment and the experimental procedure, and therefore should be interpreted cautiously, it revealed so much variability in performance that it discouraged further use of the lexical decision task. As noted, the "naming procedure" (text, page 244) appears to offer a better measure of intraverbal control. The subject simply reads aloud the second stimulus. However, this procedure requires more sophisticated apparatus for gathering and analyzing data.

The paper briefly alludes to some haziness in the concept of response strength. Notice that this haziness diminishes when one takes the biobehavioral perspective of the text. Response strength is a summary term for the behavioral effects of the physiological events that accompany behavioral contingencies. That is, changes in synaptic efficacies, among other things, underlie fluctuations in response amplitude, frequency, latency, and so on.

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The Intraverbal Effects of Briefly Presented Verbal Stimuli

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In *The Behavior of Organisms*, Skinner proposes the use of *response strength* as a summary term reflecting a variety of response measures that tend to covary, such as latency, intensity, frequency, and resistance to extinction (1938, p. 15). Presumably there is nothing "left over" after we have specified these other measures, but we are seldom in a position to specify them all. The summary term implies a generality not commonly found in a set of experimental measures. However, the term is sometimes used as a hypothetical construct. We are apt to say, for example, that rate is a *measure* of response strength, as though response strength had an independent status. The term connotes a property or dimension of a response rather than an index of the control exerted by a set of antecedent variables. Terms often evolve insidiously, as Skinner reminds us in *About Behaviorism:*

> Turning from observed behavior to a fanciful inner world continues unabated. Sometimes it is little more than a linguistic practice. We tend to make nouns of adjectives and verbs and must then find a place for the things the nouns are said to represent. We say that a rope is strong, and before long we are speaking of its strength. We call a particular kind of strength tensile, and then explain that the rope is strong because it possesses tensile strength. The mistake is less obvious but more troublesome when matters are more complex. There is no harm in saying that a fluid possesses viscosity, or in measuring and comparing different fluids or the same fluid at different temperatures on some convenient scale. But what does viscosity mean? A sticky stuff prepared to trap birds was once made from *viscus,* Latin for mistletoe. The term came to mean "having a ropy or glutinous consistency," and viscosity "the state or quality of being ropy or glutinous." The term is useful in referring to a characteristic of a fluid, but it is nevertheless a mistake to say that a fluid flows slowly because it is viscous or possesses a high viscosity. A state or quality inferred from the behavior of a fluid begins to be taken as a cause. (Skinner, 1974, p. 161)

We must be equally careful about our use of the term response strength. We are perhaps less apt to be careless with the comparable term *response probability*. Probability connotes the interplay of many variables, whereas strength suggests an inherent property.

Neither term is entirely satisfactory. The term probability is associated with models of random events, which are presumably of limited relevance to predicting and controlling behavior. If one is in leg irons, the probability of running away from a firing squad is zero, but there is a sense in which running is a strong behavior. Similarly, we may be silent while another is speaking but nevertheless be "bursting" to interrupt with our own trenchant or witty observations.

Skinner wrestled with the problem of an appropriate term in a 1958 letter to Percival Symonds:

> I am currently not happy about the notion of "probability of a response." It would be nice to adopt a pure frequency theory of probability, but this obviously won't work in talking about multiple contributions to the control of a single instance. I don't know of a better term, however, and I believe that we are dealing essentially with a probability function and that it will do no harm to reiterate this by adopting the term. This is one point where a further systematization will be most valuable. For that matter, I am not happy with the present status of the response as a unit of behavior, and in our animal work we are, I think, moving slowly toward a more flexible formulation—one which will reflect the continuous flow of behavior. The notion of a verbal "atom" is a simple step in that direction."

Nevertheless, the concept of response strength—or response probability—lies at the heart of a science of behavior. We say that reinforcement alters the probability of a response in the presence of a discriminative stimulus, that establishing operations alter the probability of a response, that the evoking or suppression of competing behavior alters the probability of a response. The terms may be troublesome, but they are not merely explanatory fictions. In the laboratory they are operationalized easily enough.

The terms are perhaps most useful when interpreting behavior outside the laboratory. In such cases changes in response measures are often inferred rather than observed, and it is convenient to have a term to capture a set of correlated changes. Outside the laboratory, behavior is commonly determined by a confluence of variables, with many concurrent behaviors in competition. Under such conditions the addition of a discriminative stimulus may have no observable effect at all, but we can reveal an effect by removing other discriminative stimuli: Weak stimulus control may be sufficient in the absence of competing contingencies. Similarly, each member of a set of discriminative stimuli may be unable to evoke a response alone, but in concert they are able to do so. A conception of response strength incrementing with the addition of each stimulus until it becomes the dominant response accommodates the facts smoothly.

Specific measures are sometimes inappropriate to the interpretive task at hand. For example, rate of response has proven a sensitive experimental variable in free operant procedures, but it is seldom useful in the interpretation of verbal behavior. (Our listeners rarely delight in hearing the same utterance again and again.) However, by appealing to the concept of response strength as a set of correlated measures we may use another such measure, e.g. resistance to extinction or response latency, in its stead.

In this paper we describe a pilot experiment designed to evaluate the use of response latency as a measure of intraverbal stimulus control. We assume that latency permits the same foundation for interpretation as rate, resistance to extinction, and so on, although, of course, its relationship to those measures is inverse. That is, we take it that latency is encompassed by the concepts of response strength and response probability.

We employed a "semantic priming" procedure, a procedure widely used in the literature of cognitive psychology (e.g. Balota & Lorch, 1986; see Neely, 1991, for a review) but only rarely used by behavior analysts (Hayes & Bissett, 1998, is the only exception we are aware of). In the semantic priming procedure, stimuli are briefly flashed on a screen, and the subject has to press keys indicating that the stimulus is, or is not, a common English word. The typical finding is that subjects respond more quickly when an English word is preceded by another word with which it is intraverbally related. For example, if presented with the sequences, WAR-PEACE, and LION-TABLE, subjects usually respond more quickly than to PEACE than to TABLE. The preceding stimulus is said to "prime" the response to the second stimulus.

In the literature of cognitive psychology such findings serve as the grist for models of mental architecture, but the procedure is of course a behavioral procedure, and the dependent variable is response latency, one of the correlated measures embraced by the term response strength. Any decrement in response latency is apparently an indirect index of the strength of intraverbal control.

Intraverbal control is presumably pervasive in verbal behavior, since verbal operants typically occur in the context of other verbal behavior. Most verbal operants are multiply determined. When we say, "Pass the salt," the response "salt" is in part a mand, under control of salt-deprivation, the taste of food, the context, etc.; it may be in part a tact, under control of the shaker across the table; and it may be in part an intraverbal under control of the prior stimuli (produced by the speaker) "pass the ...", for the speaker has presumably encountered the expression or uttered the expression many times in the past. It is a plausible assumption that most utterances contain verbal operants that are related intraverbally, however weakly. Moreover, it follows that most examples of intraverbal "strengthening" are unobservable: Most verbal operants have occurred contiguously with many other verbal operants. "Pass the ..." has presumably been paired with many things other than "salt." Hearing someone say it, or hearing ourselves say it, must have a slight strengthening effect upon a wide variety of mutually incompatible responses. Skinner has made the same points: "The intraverbal relations in any adult repertoire are the result of hundreds of thousands of reinforcements under a great variety of inconsistent and often conflicting contingencies. Many different responses are brought under the control of a given stimulus word, and many different stimulus words are placed in control of a single response." (1957, p. 74)

It is beyond our ability to measure such slight effects in any given instance. Commonly, we do not overtly respond intraverbally at all: When we hear someone begin to say, "Pass the ...", we do not blurt out "sugar, salt, mayonnaise, Cheerios, time of day, pitcher of beer" and so on, in a kind of verbal salad. Nevertheless, there is presumably some tendency to do so.

The priming procedure might be able to provide an experimental foundation to such tenuous interpretations. The procedure appears to be a measure of intraverbal control. Since the typical procedure uses textual stimuli and keystroke responses, the measure is indirect, but any observed decrease in latency to a target stimulus is presumably due to the intraverbal relationship between the priming stimulus and the target response. A modification of the procedure, in which subjects simply read the second word, would provide a more direct measure, since it would omit the judgment of whether the string was an English word, but such a procedure would require specialized apparatus to time the onset of the textual response.

Our purpose was simply to replicate the procedure as it is commonly studied, to confirm that we would observe a priming effect and to evaluate its reliability and its suitability as an experimental preparation for the study of intraverbal behavior. The procedure has been widely used, but results are invariably reported as averages. One gets no sense of the texture of the data from the extant literature.

Method

We recruited 15 Smith College students, all women. We did not evaluate them in any way, nor did we gather demographic data. One woman confessed to being "dyslexic," but we ran her anyway. We ran subjects one at a time on a single Mackintosh computer in an open computer lab in which other people were occasionally working or milling about. Subjects were given brief instructions to respond to the second of two stimuli as fast and accurately as possible, with a Y or N on the keyboard, to indicate that the second stimulus was or was not a well-formed, common, English word. Stimuli were presented and responses recorded by a commercial program called Mindlab.

We presented each subject with 60 pairs of stimuli in randomized order. The first member of each pair was a common English word. Following two "get ready" signals, the word would appear alone in the center of the screen. After 250 ms, the second stimulus would replace it and would remain on the screen until the subject responded. The second stimulus was either a nonsense word (20 examples), an unrelated word (20 examples), or a related word (20 examples). A 5 s intertrial interval (ITI) followed each response. The sequence of events was as follows:

ITI (screen blank) (5 s)
Get ready stimulus #1 (a row of stars) 367 ms
Screen blank (367 ms)
Get ready stimulus #2 (a row of slashes) 150 ms
Screen blank (367 ms)
Priming stimulus (250 ms)
Target stimulus (on until response occurred)

Thus each trial lasted less only a couple of seconds, not including the ITI.

Our stimulus words were a subset of those used by Balota and Lorch (1986). We chose those stimuli because of their demonstrated effectiveness in producing a priming effect and because they had been validated as "related" or "unrelated" by prior word-association tests in other studies. We do not subscribe to the assumption that "relatedness" is constant across the verbal community, of course, but our purpose was replication not experimental analysis.

Note that the measure of interest was the difference in response latency to related words and unrelated words. The nonsense words served only to provide a pretext for responding: Y if the string was a word, N if it was a nonsense word. They played no role in the analysis.

Results

We confirmed the typical results of such studies. On average, subjects responded faster to related words than to unrelated words. The average latency to respond to related stimuli, across all subjects and across all stimuli, was 645 ms. Average latency to respond to unrelated stimuli was 687 ms, or 42 ms longer. The effect is statistically significant ($p < .01$). However, those figures mask considerable individual differences. Four of the 15 subjects responded more quickly to the unrelated stimuli than to the related stimuli, and one subject responded at the same rate to both. The following table shows the differences in latency (average latency to unrelated stimuli minus average latency to related stimuli in milliseconds):

Subject	Difference
1	116
2	66
3	46
4	70
5	-24
6	34
7	120
8	48
9	-31
10	-18
11	0
12	101
13	46
14	-34
15	73

The differences for subjects 5, 9, and10 were distorted by one or more anomalously long latencies. If those latencies were dropped from the analysis, the difference in latencies was positive for all three subjects. (There were no such anomalies for subject 14.) Recognizing that the subject is always "right," we present the data as we found them.

Trial-by-trial data also reveal considerable differences. The standard deviation of latencies to respond to related stimuli was 189 ms and ranged from 416 ms to 2150 ms. The corresponding standard deviation for unrelated stimuli was 228 ms, ranging from 183 ms to 2250 ms. Across both conditions, there were seven trials in which response latency exceeded 1.5 seconds.

Discussion

If our data are representative, the typical priming effects reported in the literature, at least for this procedure, are misleading. Only four of fifteen subjects showed an average difference in latencies in the ballpark of 40 milliseconds, and individual trials differed by as much as two seconds. (The shortest latency was 183 ms, the longest 2250ms.) One would expect considerably less variability in the data if they reflected only the effect of an intraverbal relation. That there is a difference in responding to related and unrelated stimuli requires an explanation, but that there is so much variability in the effect also requires an explanation.

It is a plausible assumption that the difference in latencies reflects intraverbal control. Presenting the stimulus WAR potentiates the response PEACE, and this potentiation is reflected in a reduced latency to respond to the word when it appears as a textual stimulus. That is, the "topography" of saying *peace* is controlled by two supplementary stimuli. "Topography" is in quotes because only rarely does a subject utter the response overtly. Nevertheless, if we assume that covert behavior is still behavior, it must have physical dimensions, however obscure and diffusely distributed.

However, the latencies found in priming studies cannot be taken as direct measures of intraverbal control. That latencies range over two seconds indicate that other variables are at play. It is possible that the variability arises directly from the stimuli used and differences in the subjects' histories with respect to different stimuli. For example, the intraverbal relationship between *war* and *peace* no doubt varies from one subject to the next. The arbitrary parameters of the procedure might also play a role. For example, we used a 250 ms interstimulus interval (between priming and target stimulus) because that interval has been used by others, and we used a 5 s ITI because we ourselves found it annoying to wait longer. Such parameters can be fine-tuned, but it is unlikely that the observed variability is due only to such problems.

The typical priming procedure terminates in a "lexical decision task," that is, a response that reflects whether a stimulus is or is not a familiar word. This task is quite complex and only obliquely reflects intraverbal control. It is highly unlikely that the latency to complete such a complex task is a direct reflection of any single basic process. The procedure might be improved by requiring subjects simply to read aloud the second of two stimuli, timing the onset of the response with a voice-activated switch (the "naming task"). The latency of the response would presumably reflect both textual and intraverbal control and would be relatively uncontaminated by other variables.

One might ask for what purpose one might want to measure intraverbal control with any precision. Is not intraverbal behavior of interest only when analyzing recitations of poetry, memorized facts, and so on? Certainly not. The holy grail in the interpretation of verbal behavior is accounting for regularities in the sequencing of verbal operants, that is to say, grammar. A behavioral account must explain the sequencing of behavior moment to moment in time. An invariant property of sequences of operants is that they occur contiguously. Intraverbal control presumably arises from contiguous usage (Skinner, 1957, p. 75), though the role of frequency and reliability is unknown. Behavior analysts face the daunting puzzle of how structural regularities in verbal behavior generalize from one example to another in new constructions, as they undoubtedly do. (If you are told that Jack Michael flobbered his toe on a brick in the hall, you can use the word in novel ways, even though it is nonsense: "How can I avoid flobbering *my* toe?") Intraverbal control is one variable that may play a role in controlling the sequencing of responding or of triggering shifts in stimulus control in such sequences. We need to exploit all of the variables at our disposal.

In particular, it would be worthwhile evaluating how quickly intraverbal behavior can be established. Does hearing a passage in which two distinctive terms appear contiguously establish some intraverbal control in a subsequent priming task? If not, how many replications are typically required? If so, how long does the effect endure? Does silent reading have the same effect, or any effect? Reading aloud? Must the terms be familiar, i.e., already in the speaking repertoire of the listener? Such questions should be easy to evaluate once suitable apparatus has been set up, for a priming procedure is easy to run. If, as we suspect, intraverbal control is quickly established, it may play a pervasive role in the sequencing of verbal operants.

Response latency may be a productive alternative to more common dependent measures encompassed by the concept of response strength, such as response rate. Verbal behavior is so tightly controlled by context that free operant procedures are inappropriate. Some priming procedures (e.g. the naming task) may enable us to put an empirical foundation under inferences about fluctuations in response strength in verbal behavior.

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