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Dialogue on Private Events

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In the fall of 2003, the authors corresponded on the topic of private events on the listserv of the Verbal Behavior Special Interest Group. Extracts from that correspondence raised questions about the role of response amplitude in determining units of analysis, whether private events can be investigated directly, and whether covert behavior differs from other behavior except in amplitude. Most participants took a cautious stance, noting not only conceptual pitfalls and empirical difficulties in the study of private events, but doubting the value of interpretive exercises about them. Others argued that despite such obstacles, in domains where experimental analyses cannot be done, interpretation of private events in the light of laboratory principles is the best that science can offer. One participant suggested that the notion that private events can be behavioral in nature be abandoned entirely; as an alternative, the phenomena should be reinterpreted only as physiological events.

For 19 days in the fall of 2003, the Verbal Behavior Special Interest Group (VBSIG) listserv was the forum for an impromptu discussion about private events among the authors. This paper has been excerpted from that discussion and lightly edited by the first author, who also composed the introductory and concluding passages. The remaining authors are listed according to their chronological entry into the discussion. Since all entries were posted with no thought to publication, they were typically written hastily and informally. No doubt the authors could articulate their views more clearly, given the opportunity, and upon reflection, they might prefer to have made different points entirely. Therefore, the reader should recall that this was a spontaneous dialogue, not a set of position papers. The discussion revealed that, even among such an intellectually homogeneous group, there is no consensus about the place of private events in the science of behavior. Nevertheless, it is noteworthy that the tone of the exchange was courteous and positive throughout; opinions differed, but no points were scored.

The informality of the dialogue has been retained. Occasional changes have been made to the text to accommodate the standards of the journal, and some transitional passages, presumed to be helpful to the reader, have been inserted and attributed to the relevant author, although these passages did not appear in the original entries. It would have been possible, of course, to mark all such editorial intrusions orthographically, but the distraction to the reader would not have been repaid by the added precision of attribution. The reader should note that, in many cases, entries were written in parallel, a circumstance that necessarily has led to some incongruities in the dialogue: Entries are sometimes separated from their target by several other entries to which they make no reference.

The "VB" in VBSIG has two meanings. Broadly speaking, the special interest group is devoted to the scientific study of verbal behavior as behavior (that is, as distinct from approaches devoted to language as a capacity of the mind, or as a set of grammatical rules, or as a hypothetical module in the brain). But the group was inspired by Skinner's book *Verbal Behavior* (1957), which has provided a foun-

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dation for much of the empirical and conceptual work in the field. Consequently, Skinner's views on private events are familiar to all of the participants and foreshadow many of the points made in the discussion. It is perhaps useful to review what Skinner has said.

In *Science and Human Behavior* (1953), Skinner opened his chapter on private events with these lines:

> When we say that behavior is a function of the environment, the term "environment" presumably means any event in the universe capable of affecting the organism. But part of the universe is enclosed within the organism's own skin.... With respect to each individual, in other words, a small part of the universe is *private.* We need not suppose that events which take place within an organism's skin have special properties for that reason. A private event may be distinguished by its limited accessibility but not, so far as we know, by any special structure or nature. We have no reason to suppose that the stimulating effect of an inflamed tooth is essentially different from that of, say, a hot stove.... But if some of the independent variables of which behavior is a function are not directly accessible, what becomes of a functional analysis? (pp. 257–258)

Skinner then reviewed ways in which verbal behavior under the control of private events might be shaped, but cautioned that some verbal responses that appear to be under control of private events might actually be responses to public variables (a point emphasized by Layng in the discussion):

> Another possibility is that when an individual appears to describe unemitted behavior, he is actually describing a history of variables which would enable an independent observer to describe the behavior in the same way if a knowledge of the variables were available to him. The question, "*Why* did you do that?" is often important to the community, which establishes a repertoire of responses based upon the external events of which behavior is a function. (p. 263)

He distinguished between covert behavior and other private events:

> One important sort of stimulus to which the individual may possibly be responding when he describes unemitted behavior has no parallel among other forms of private stimulation. It arises from the fact that the behavior may actually occur but on such a reduced scale that it cannot be observed by others—at least without instrumentation. This is often expressed by saying that the behavior is "covert." (p. 263)

Skinner anticipated other points made in the dialogue. For example, he cautioned that "the appeal to covert or incipient behavior is easily misused" (p. 264), but he suggested that advances in instrumentation might come to our aid by moving the boundary of what is publicly observable, since "the line between public and private is not fixed." (p. 282) Regarding the matter of parsimony, he remarked,

> One is still free, of course, to assume that there are events of a nonphysical nature accessible only to the experiencing organism, therefore wholly private. Science does not always follow the principle of Occam's razor, because the simplest explanation is in the long run not always the most expedient. (pp. 279–280)

Skinner distinguished between interpretation and experimental analysis in *Verbal Behavior* (p. 11), but his most extensive discussion of the topic is found in his reply to commentators in a collection of his canonical papers (Skinner, 1988):

> [The commentators] have assumed that beyond science and technology there lies only philosophy. I have found something else: interpretation. I would define it as the use of scientific terms and principles in talking about facts about which too little is known to make prediction and control possible. The theory of evolution is an example. It is not philosophy; it is an interpretation of a vast number of facts about species using terms and principles taken from a science of biology based upon much more accessible material and upon experimental analyses and their technical applications. The basic principle, reproduction with variation, can be studied under controlled conditions, but its role in the evolution of existing species is a mere interpretation.

> Plate tectonics is another example. It is

> not philosophy but an interpretation of the present state of the crust of the earth, using physical principles governing the behavior of material under high temperatures and pressures established under the conditions of the laboratory, where prediction and control are possible.
>
> Laboratory analyses of the behavior of organisms have yielded a good deal of successful prediction and control, and to extend the terms and principles found effective under such circumstances to the interpretation of behavior where laboratory conditions are impossible is feasible and useful. I do not think it is properly called philosophy. The human behavior we observe from day to day is unfortunately too complex, occurs too sporadically, and is a function of variables too far out of reach to permit a rigorous analysis. It is nevertheless useful to talk about it in the light of instances in which prediction and control have proved to be possible. (pp. 207–208)

Skinner remarked that private events are of little practical importance in the control of behavior. Nevertheless he claimed that the radical behaviorist's view of private events is one of its most important achievements, for it permits a monistic science. In that light, his interpretation of private events is a landmark in the history of science. It is not surprising then that the topic should have generated so much discussion on the VBSIG listserv.

On October 27 John Eshleman opened the thread by mentioning five of his "simmering interests," one of which was summarized by the question, "Is amplitude a dimension of verbal behavior worthy of further study?"

Paul Brandon (PB): Is amplitude a subcategory of topography, in which case a change in amplitude might define a new response, or is amplitude a measure of a defined behavior like frequency? In the first case, we would speak of a verbal response of given amplitude, with a change in amplitude defining a new response. In the second we could speak of changes in the amplitude of a given response.

John Eshleman (JE): Both may be correct. It may be the case that a change in amplitude defines a new response, but even if this were true, amplitude would remain a dimension of behavior like frequency. Vocalized verbal behavior exhibits a wide range of variability in amplitude, only some instances of which make functional differences. Amplitude is a dimension of any behavior. In the case of subvocal behavior, it is currently difficult to measure; one would presumably have to resort to instrumentation that detected subtle variability of the vocal neuromuscular system.

But, your question is important: If someone asks, "When was the Battle of Hastings fought?" is a hear/shout "1066!" the same response as a hear/subaudibly-vocalized "1066"? Presumably both are intraverbals. The content or "topography" seems to be the same. The difference is in amplitude, but this may well vary with the context or other circumstances. Also consider amplitude changes within a response. A vocalization might start out at one amplitude and change midstream to a higher amplitude: "HellOOO!" Is what seems to be one response really two responses?

PB: There is no question that amplitude is a dimension on which behavior can be measured. The question is whether a given response can vary in amplitude, or whether a change in amplitude (beyond some defined range of variability) always defines a new response as indicated by a change in function. Talk to an actor about the functional differences in vocalizations of different amplitude! Note the way we attempt to incorporate an amplitude dimension in written verbal behavior (capitals = SHOUTING online). Another area for potential study might be music, in which response amplitude is very functional. Is music verbal behavior? I think that it fits the definition.

T. V. Joe Layng (JL): Although it is clear to those with children that amplitude may have autoclitic effects, we have included amplitude as a targeted topography in our Headsprout Early Reading program for other reasons. In our user testing we found that asking a child to "say it again louder" was critical to reliably establishing good articulation of the letter sounds we teach. Children who may (autoclitically?) almost whisper the response the first time will often shout the response after the response request. The louder response is usually accompanied by affective changes as well—sitting up straighter, a smile, or laughter—and it increases the likelihood the next response, when another stimulus is presented for the first time, will be louder. (The performance also raises questions about the effects of reinforcement history and penalties for false alarms, which may act to decrease the amplitude of initial responses. Our findings suggest a paradigm for experimentally investigating these processes.) Getting the first response at low amplitude in a largely echoic task, then asking for it louder helps to firm the articulation of the response, which can then be transferred to independent textual control. Early tests showed us that without the "say it louder" request (at least in the beginning) firm articulation was not likely for many children. This investigation, among others in our laboratory (N = 250+ children), led to an important series of instructional subroutines in our speaker-as-own-listener sequences (for which we have been issued a U.S. patent). We found that psychophysical indicator response methodology, together with on-going *Verbal Behavior* analysis, was quite important throughout our R&D effort, particularly since we need to shape verbal behavior in the absence of a "true" independent listener.

JE: Amplitude modulation in the reverse direction is also of general importance, i.e. teaching learners to go from reading out loud to "silent" reading. The culture generally demands this. (Who would want to sit next to a passenger on an airplane who reads the in-flight magazine out loud?)

In Precision Teaching there is a learning stream called "see/think," in which you see some text and "think" the response. Is see/say different from see/think? Is an intraverbal in which you say the response out loud the same behavior as an intraverbal in which you "say" the same thing silently? The controlling relation for the different amplitudes may be the context: See boss, say "idiot." You wouldn't say "idiot" out loud, unless it was your last day at work anyway and you had nothing left to lose.

How could research on such questions be carried out? It would be difficult to achieve inter-observer reliability. I've recorded my own see/think frequencies (see front of card, think response; where think = silent say). I can, with myself as subject, reliably (in the sense of each time) obtain much higher response rates with see/think than with see/say (over 100/minute see/think compared to around 60/minute with see/say, with the same cards). When I do an A-B-A-B design, the rates go down and up accordingly. But in the see/think condition there's no way that an outside observer could tell that I'm doing anything other than merely flipping 3 by 5 cards at a high rate.

Christopher McDonough (CM): Can the difference in amplitude alone (i.e., subaudible vs. audible "say") account for these differences in rate?

PB: We return to basic methodological problems: How do we know the details of the stimulus control of a given individual's tacting of private events? With math problems, when we can specify the precurrent behaviors necessary to produce a correct solution, we can infer that they have occurred. As a practical matter, sometimes we can verify that a given treatment package is effective without being able to tease out the controlling variables.

JL: The approaches to covert and overt behavior must be different. One cannot differentiate "thinking" fast from thinking, "I covertly said it fast." What I think is fast thinking may not be so, however firmly I believe it. The problem is not insurmountable, however. One can see if the instruction to say something (at "low amplitude") produces a change in some indicator that the instruction to say louder, perhaps in an increasing gradient, does not. In addition, one could look at the effects different establishment and practice programs have on the dependent measure under the differently instructed environments as well. In the 1950s, Benjamin Bloom at the University of Chicago found that while major differences in problem-solving approaches used by college students might lead to the same solution, all paths were not equal, nor were all equally effective. He was able to increase the amplitude, adjust the "path," and then decrease the amplitude and maintain the improvement on the dependent measures.

David Palmer (DP): Regarding the methodological question of how to study covert verbal behavior, whatever our scruples, the wrong answer is to run away from it. Covert behavior is part of the domain of interest, and to many people, it is the dominant part of the domain of interest. I think the solution is clearly to distinguish interpretation from experimental analysis. We can't manipulate or measure phenomena beyond the range of detection, but we can interpret incomplete data in light of basic principles that have been well established in an experimental analysis. This is what Skinner does with such force in *Verbal Behavior*. Moreover, most of what passes for scientific fact is just an interpretation. Newton analyzed experimentally the motion of pendulums and rotating buckets of water, but he offered interpretations of the motion of planets, the tides, and a lot of other phenomena, interpretations that we take to be scientific fact. But he never achieved experimental control over these phenomena, and neither has anyone else since. Nevertheless, we regard the mystery of the tides as having been solved.

So I think we should confidently, without embarrassment, investigate phenomena that are partly covert, so long as we acknowledge that our exercise is interpretive. The purpose of science is not just to master nature but also to resolve mysteries about it, and in the latter enterprise interpretations are often powerful (Skinner, 1957, again). Almost all evolutionary accounts are interpretations, but they help us make sense of natural phenomena, and they help us resist the temptation to invent magical solutions.

John Eshleman's see/think exercises remind me of a technique used by Headsprout. Headsprout's program asks children to speak to the computer (and even prompts them to speak louder), but the program has no speech recognition software. The speech of the child is covert to the program. Nevertheless the procedure is powerful. The point of the procedure is that children can (usually) detect when they have matched to a model and when their behavior is discrepant from the model. This permits the automatic reinforcement of conformity to the model and the rapid and efficient shaping of behavior. It's an ingenious way of avoiding the need for human supervision of the student, and in my opinion it is a much more realistic model than ordinary of how children actually learn in natural settings. It's a good example, because the behavior is only covert to one audience (the computer and the people downstream of the computer); to other audiences (parents, visiting consultants, the child), the behavior is quite observable. If, as I believe, this procedure extends the power of the Headsprout program, it is possible that John's see/think exercises, and similar efforts, might tell us something important. Covert behavior is difficult to study, but that doesn't make it less real. As long as we are clear about the interpretive nature of the enterprise, I think it plays a role in our science.

PB: Since we are talking about covert *verbal* behavior, we can distinguish between responses where the *only* listener is the speaker (covert), and responses where at least some listeners are not the speaker (overt). Thus there could be cases where a response of a given topography (amplitude) shifts between the two categories depending upon circumstances (if a tree falls in the forest where no one is listening, it does so covertly!); but this is as it should be in a system of functional definitions.

JL: I think we must make the distinction between private events and covert behavior. The term *private event* acknowledges that there are phenomena that should be investigated for which independent evaluation is currently impossible, whereas covert behavior assumes these events are of a particular *kind,* i.e., behavior. But for this we have no evidence.

I think this area is far more complicated than has been generally acknowledged in our literature. Attributing causal or even dependent variable status to these events may be preventing us from a more complete analysis. My position is to use reports of private events (especially my own) as occasions to ask questions about what reinforcement contingencies and histories might account for such reports. Is what I report the actual private event or simply my rationalization of the changing response probabilities shaped, as Skinner has pointed out, by being asked questions for which an overt answer is required? In writing a sentence, a word may be written and then withdrawn; I may engage in a private dialogue such as, "that is not what I want to say, let me try ..."; "no that's not it ..."; "that's it ...," etc. Is this covert "trying out words" actually describing covert behavior? Can this supposed behavior be used to understand how the word eventually appears on the paper? Or is it my way of rationalizing the changing response probabilities of historically reinforced patterns (a program variable) that compete with one another, potentiated by the current words on the paper, and audience variables, until a response occurs that provides an absolute "match to sample" (as opposed to a comparative match) so that the written response meets the criteria reinforced in the past? The private events may indeed be occurring, but as a by-product, not only of the history responsible for the written word, but of the requirement to describe the origin of our behavior. Once a report becomes public, it is an operant under the control of the reinforcement of past contingencies in its own right. The literature on how the difference in accuracy between spoken and nonspoken indicators, which defines subliminal perception, can be traced to different consequential histories for each class provides some clear evidence of this. All this is to say that what may account for an observed behavior and correlated private events (not one causing the other) are consequential contingencies and their programs. We may have to pay a little more attention to behavioral development if we want to understand our private events as well as our overt behavior.

CM: I wonder if this won't lead to infinite regression. If your rationalization is an interpretation of "the changing response probabilities," etc., that interpretation is open to a contingency analysis of the same type. Won't we just go around and around?

JL: Yes, and that is one of the problems. Here is a passage from an earlier paper where I discussed this problem (Layng, 1995):

> ... In essence, they were examples of what Kripke (1982) describes as Wittgenstein's skeptical paradox.... The skeptical paradox cannot be easily dismissed by those who contend that thought causes behavior. Even if we were to accept repeated congruence of an indicator response to a score sheet as evidence of private control, how do we or the subject ever know that what the subject describes as governing either the indicator or the referent behavior is indeed the private event that is governing the observable response? (p. 251)

Here is an example, from among several in the paper:

> When the behavior being studied is private, that is, the event is accessible only to the subject and not the experimenter, the question of the validity of the experimental subject's description of the private event as a causal variable is raised. For example, a subject, who had no history of stuttering, and who had been told that his physiological reactions to reading aloud certain passages of text were being measured by electrical leads to his fingers, began stuttering while reading the passages. When asked why he began stuttering, he reported that he had become anxious having to read aloud and began stuttering as a result. He reported that the stuttering slowed down his reading, making him even more anxious, which in turn led to more stuttering. In fact, the experimenters (Flanagan, Goldiamond & Azrin, 1959) had arranged an avoidance schedule in which electric shocks delivered to the subject's fingers could be postponed by stuttering. The normally fluent subject stuttered to such an extent that by the second day almost no shocks were delivered. The shocks became so rare that in the post-experiment interview with an experimenter the subject denied that shock had any effect on his verbal behavior. The subject, who never expressed an awareness of the relation between the shock or the avoidance contingency and his stuttering, and who attributed the stuttering to a sudden feeling of anxiousness, was shown to systematically vary his stuttering with changes in the shock avoidance contingency arranged by the experimenter. (p. 256)

Did the subject become anxious? Perhaps. Would counting private instances of anxiousness have told us anything about the determinants of the stuttering? Doubtful. It is more likely that a history of explanation giving was involved, which may in turn have led to the subject "feeling, or simply reporting feeling, and perhaps believing, he was anxious."

JE: Also see the work of A. Calkin (2002) regarding private events, in a recent issue of *The Behavior Analyst*.

In the 60s, 70s, and 80s, some of Ogden Lindsley's students studied pregnant women counting and charting fetal kicks, which is something that apparatus can detect as well. A couple of Og's students charted these frequencies on Standard Celeration Charts and published their data in *Science* (Edwards & Edwards, 1970). They had measures of reliability, obtained from periodic instrument detection and recording of the fetal kicks, that showed that the women were reliable counters and measurers of the behavior that was going on inside them.

People can be trained to be reliable counters and measurers of inner behavior, but that's a separate question from that of using amplitude as a dependent variable. Og's students measured frequencies, not amplitude, as a dependent variable. Amplitude remains a virtually unstudied, seemingly elusive dimension to behavior.

PB: Training is exactly the issue here, and that's what makes the above example of internal behavior different from the usual cases of private/inner/covert responding. Fetal kicks can be detected by both apparatus and by "outside" observers (ask any father). Hence we can set up reliable training contingencies to teach pregnant women to reliably report these events.

Jack Michael: I would like to suggest a sharper distinction between the terms *stimuli* and *responses*. In this case, the pregnant woman is responding to stimuli that are being produced by the fetus. The fetus is responding, but not the woman. If these stimuli control some behavior on her part, such as counting, then the counting behavior is her responding, and the internal stimuli are functioning as possibly discriminative stimuli for a tact of a private stimulus. I have noticed in some earlier posts that the term "response" is used when in fact it is the response product, which is a stimulus, that is relevant to the issue being discussed. In verbal behavior this is a common problem, to overcome which the term "response product" was invented. My response to your saying "Hello" is not a response to your response but rather to the auditory product of your vocal response. I think it helps avoid confusion and unnecessary complexity to have a very sharp distinction between stimulus and response. When "stimulus" is used, it is usually possible to modify it with "visual," "auditory," "olfactory," etc. When a "response" occurs it is usually possible to identify a part of the body that is responsible for the response (a vocal response, a manual response, a head nod), or the response can be identified in terms of its effect on the environment—a lever press, a key peck, a computer screen touch. In many cases the response product can be taken for granted, but in this area of private events I think unusual clarity is needed. Also, I tend to use the term "covert" as a way of identifying a response which cannot be observed by someone else, or that does not have an observable effect on the environment, or that is an inferred response. I think covert stimulus is better rendered *private stimulus*. Of course, covert responses may very well have private response products, which are stimuli. But I think blending these concepts with each other harms rather than helps. I realize that there is no reason why my verbal practices should be universally adopted, but at least discussing them may lead to less ambiguity.

DP: The skeptical paradox may be unavoidable, but I think we can live with it, so long as our goal is to offer a unitary and parsimonious account of all human behavior. We invoke covert behavior, not because someone reports it and we have faith in his report, but because a) it resolves a puzzle that would otherwise remain mysterious, and b) it is plausible under prevailing conditions. To return to the example of the tides: For all Newton knew, the tides might be caused by the respiration of a gigantic sea monster; he never ruled out that possibility. But his interpretation appealed to principles derived from controlled studies, where sea monsters had been ruled out, and thereby offered a naturalistic explanation for a phenomenon that would otherwise remain mysterious. If he had stayed his hand, out of a reluctance to extrapolate, the vacuum would have been filled with sea monsters and worse things.

An appeal to covert behavior is pointless unless it helps us make sense of a performance that would otherwise be baffling. The purpose is to show one path that nature might take to produce a phenomenon. It helps us lay tentative claim to a domain. Our account need not be right in every detail to have that effect. Most of the claims of evolutionary biology are similarly tentative, since the past is largely "covert" to us, but they offer satisfaction nonetheless and fortify us against the temptation to drift into superstition. So it is with appeals to covert behavior: For some phenomena, if we don't acknowledge the mediating role of covert behavior, we simply have no explanation at all. (I allude, for example, to problem solving behavior in which the subject's terminal behavior depends upon a pattern of response-produced supplementary stimuli, as in "mental arithmetic.") Our accounts must be tentative, as the skeptical paradox insists, but all that is required is that we have at least one possible explanation for a performance that would otherwise invite mysticism.

JE: Why must covert behavior "make sense of a performance that would otherwise be baffling" more than any other behavior? Why not study "covert" behavior in its own right as behavior, when it is methodologically possible to do so? What controls it? What are its functions? What are its frequencies and celerations? How can we distinguish between nonverbal and verbal "covert" behaviors? How might "covert" behaviors chain together in response-response chains? What amplitude differences can be found with "covert" behavior, and what would this range of amplitudes be? How might modulations in amplitude of such behavior be important? Are any such modulations related in some way to frequency differences (i.e., are the two dimensions correlated)?

JL: My problem is not with the privateness of the events, but in the assignment of causal status. Although such assignment may make a particular phenomenon easier to understand, it may lead us to a comfortable explanation that masks the real controlling variables. It also leaves us to account for the private event itself, which I believe will lead us to contingencies of reinforcement, which in turn will lead us to program variables as the locus of the causal determinants. We may find that program variables (which I believe are sorrowfully neglected by behavior analysts) are responsible for both overt behaviors and the private events that accompany them. Remember the most parsimonious account of all, that resolves all puzzles for many individuals, and is indeed plausible, even to many noted scientists.... "in the beginning there was the Word...."

Ted Schoneberger (TS): Wittgenstein's skeptical paradox offers a serious challenge to behavior-analytic explanations of behavior that attribute causal efficacy to thoughts and feelings. Interestingly, Chomsky has also acknowledged that this paradox offers a serious challenge to his linguistic theory. (Those who read Chomsky know that such admissions are rare.) Indeed, in *Knowledge of Language: Its Nature, Origin, and Use* (Chomsky, 1986), he devoted 20 pages to a discussion of this challenge. Quoting Chomsky, "Of the various general critiques that have been presented over the years concerning the program and conceptual framework of generative grammar, this [i.e., Wittgenstein's skeptical paradox] seems most interesting." (p. 223) Chomsky believes he escapes any destructive implications of the paradox because his (Chomsky's) theory "is a descriptive theory of ... attained or initial competence ... not a causal or dispositional theory." (p. 241)

Returning to the issue of how behavior analysis responds to the skeptical paradox, I agree with Joe Layng that it cannot be easily dismissed. I also agree with Dave Palmer that we can live with it. However, please let me qualify my response by saying that I think Dave's suggestion that "we invoke covert behavior, not because someone reports it and we have faith in his report, but because a) it resolves a puzzle that would otherwise remain mysterious, and b) it is plausible under prevailing conditions" can easily lead—if we are not careful—to a form of instrumentalism in which "anything goes" (a criticism of instrumentalism offered, I believe, by Jay Moore). So "living with" the paradox, while honoring Joe's entreaty that it cannot be easily dismissed, suggests, to me, a response to the paradox that includes a sophisticated integration into behavior analytic theory of Wittgenstein's own response to the paradox, coupled with a neo-pragmatist (i.e., Richard Rorty's) approach to the issue of truth.

PB: The term "causation" itself is ambiguous. (I believe that Aristotle identified five different uses of the term, and it's only gotten worse since.) Neither Palmer nor Layng (nor any other radical behaviorist that I know of) is claiming "hard" (mechanistic) causality for private events. The "party line" is that private events are behaviors, not causes. The question is: Can we posit that private events, although unobservable to others, are behaviors in a chain whose end points *are* publicly observable, and can we then use this hypothesis to account for the end points of the chain in a manner consistent with the behavior analytic system? To oversimplify, it looks like Palmer is arguing that this approach is pragmatically justified, while Layng is pointing out some of the dangers inherent in this approach.

CM: Why is there such a need to explain mysteries? Or, explain mysteries prematurely? What is a good explanation? One that lasts for 50 years? 200? 1000? As Dave pointed out, "the vacuum" will be "filled with sea monsters and worse things." Yes, but would Newton's vacuum be filled with such explanations? It seems to me that it's better to leave things messy than to have things cleaned up in such a way that you reach for the bar of soap and end up with the axle grease. People offer up little explanations about phenomena they encounter all the time; every day; many times a day. Sometimes variables are manipulated before explanations are offered and sometimes they aren't. Clearly, we can't live without them.

Suppose we were forced to make a choice right now. Given what we currently know, should we (a) pour all of our collective energy into a unified explanation of verbal behavior or (b) pour all of our collective energy into verbal behavior experiments and applications (R&D)? What choice should we make?

DP: Whether behavior is covert or overt depends on the point of view of the observer, not on some essential property of the behavior itself. If you are in the next room, all of my behavior is covert to you. If you have me wired up to an electromyograph, and other such devices (some of which have not yet been invented, perhaps), you will be able to detect behavior that even I am unaware of. So there is nothing special about covert behavior; it's just behavior that we aren't at the moment in a position to observe. Unfortunately, we usually observe only a small part of the behavior of any organism. It is perhaps inevitable that much of the behavior of any organism is effectively covert to us, but we should not make the mistake of assuming that because we have not measured something, it must not be there.

We are able to experimentally analyze only a tiny fraction of natural phenomena, be it behavior or anything else. So our understanding of the world will consist of a few islands of experimental facts and principles amid a great sea of application and interpretation. (Beyond that lies the endless fog of bootless speculation [sea monsters].) I take it as an axiom that this state of affairs will prevail, and that is the reason I argue that we should offer, without shame, interpretations that appeal to covert behavior. That isn't pseudo-science; it's just plain science. All science is mostly interpretation.

Interpretation isn't just the table scraps of science; it's the main course. We engage in experimental analysis so that we will be able to interpret the natural phenomena around us. It is true that the mastery of nature that arises from achieving experimental control leads to wonderful things—

medicines, vaccines, gadgets of every sort—but the most priceless gift of science is the understanding, however tentative, of how the world works. Cosmology, geology, evolutionary biology, behavior analysis—almost all of it is interpretation. Sometimes we will grab the axle grease, not the soap, as Chris so aptly puts it, but nothing can protect us from such errors. If we are to try to understand phenomena like continental drift, the origin of a species—or problem solving behavior and memory—we will undoubtedly make mistakes. That's why it's important to acknowledge the interpretive nature of our enterprise.

JL: Although much of what we do in an adjoining room may be covert to others, it is at least open to independent sampling. I have always preferred procedural (as opposed to operational) definitions. All such definitions involve congruence with a score sheet. That is why I view behavior as the relation of the organism (in the case of behavior analysis) to its environment as measured by the investigator. Since we are interested in consequential relations (among others) certain "natural fractures" define the parameters of our measurement systems. However, suggesting that private events have properties that somehow function as does behavior in direct contact with contingencies causes me some concern. The very real issue that Jack brought up aside (Is it a stimulus or a response?), the problems that arise seem to me to outweigh the potential utility of most interpretations. There are so many instances where an initial explanation based upon a reasonable interpretation of the action of covert behavior has been shown to be highly questionable or dead wrong that one has to question if any such interpretation can really move a science of behavior ahead.

But, I have a more fundamental problem than that. Private events need to be investigated in their own right. We should make few assumptions about the stuff from which they are made (seeing, speaking, hearing) or their role (behavior, stimuli, etc.) We need creative "ideas" and experiments about their ontogenetic evolution and utility (if any). I agree with Skinner in his stand against both operational definitions and to appealing to hypothetical mediating events, but I also agree with his assertion that private events be studied in their own right. They deserve their own classification and perhaps some new approaches to their possible role in verbal and other behavior.

DP: Paul Brandon summarized the discussion thus: Palmer likes interpretation on pragmatic grounds; Layng notes the dangers of this approach; nobody wants to impute ultimate causal status to private events. That sounds right to me.

But there is at least a disagreement in degree of concern. In his recent post, Joe Layng notes that interpretations have so often been found to be wrong that he wonders if they can really move a science of behavior ahead. He concludes with the suggestion that we study private events in their own right, a suggestion which I believe is congruent with John Eshleman's position.

Perhaps there is no disagreement after all. If a topic is amenable to experimental analysis, then by all means it should be investigated that way. If past interpretations have been proven wrong, as Joe suggests, then presumably an experimental analysis was in fact possible. I agree that if a phenomenon can be studied, it should be. Armchair interpretations are the lazy man's enterprise.

But I insist that there are many phenomena of interest cannot be experimentally analyzed because we do not have access to the relevant data. I think evolutionary biology and cosmology offer compelling examples. We have only fragments of the relevant data, and it is unlikely that we will ever have complete data. The viceroy butterfly looks just like a monarch butterfly. Why? Because God's species stamp got stuck, and he turned out two species with the same coloration? Maybe. But the monarch is bitter and poisonous, and there is presumably a selective advantage to looking like one. There is a tidy little "just so story" of how the viceroy mimics the monarch because of differential predation. It's an interpretation, and the facts are out of reach. Should we wait until we have a complete fossil record that will support the interpretation, recognizing that such a record might never be available? It depends on the cost of being wrong. Our interpretation of mimicry might be entirely wrong. Maybe it is the monarch that mimics the viceroy for reasons that we are unaware of. But I argue that it doesn't matter if our interpretation is wrong. The point of the exercise is not to offer certainty, but to show that the facts currently at our disposal offer a possible solution of the puzzle of mimicry without recourse to mysticism. As long as we have one plausible natural explanation for a puzzling phenomenon, it displaces occult explanations. It doesn't matter if it is eventually displaced by a better and more accurate natural account.

Moreover, I insist that if we don't offer interpretations in certain domains, we don't have any account at all, and that opens the door for the spiritualist, the mentalist, and the mystic. Consider one of my standard examples, "What is the 10th letter after H?" [Try it.] We can assume that for most people, this is a novel question. Thus, even if we were able to look in the subject's history for prior instances of the question and reinforced responses, we wouldn't find them. But given a minute, most people can answer the question. Most people twitch their lips, twiddle their fingers for a minute or so, and answer, "R." We interpret this performance as follows: the subject is covertly reciting the alphabet while simultaneously ticking off the numbers from one to ten on his fingers. This interpretation converts a problem that has no explanation at all in terms of overt behavior into one that has a perfectly mundane one. Is it plausible? Yes: The intraverbal chains of counting to ten and reciting the alphabet are well established in most adults (and we can test for them), as is the strategy of lining up objects and picking out the Nth instance. The latency of the target response leaves a window for mediating behavior. The twitching of fingers is often overt and supports the notion of covert counting.

The account might be wrong. Jack Michael, when posed the problem, took an "educated leap" and landed on a nearby letter (or so he reported). Suppose he had gotten it right and had "leapt" to the right letter, in contradiction to our interpretation. It doesn't matter. The point of the exercise is to show that behavior analysis can offer at least one plausible account of a phenomenon that might otherwise be left to others. If we can offer an even more compelling experimental analysis, so much the better. But I am greedy and don't want to leave anything to mainstream approaches.

CM: Here are a few quotes from Dave's last post that I think sum up his position. I've offered my opinions after each quote or set of quotes.

With respect to butterfly mimicry: "Should we wait until we have a complete fossil record that will support the interpretation, recognizing that such a record might never be available?"

My opinion: I see no reason why we should not wait. My response to interpretations such as the one offered about butterfly mimicry is usually something like this: "Interesting. Let's get back to work!"

On the consequences of not offering interpretations: "... we don't have any account at all.... We open the door for the spiritualist, the mentalist, and the mystic."

My opinion: I can accept not having an account. I think interpretation is overvalued. We sometimes talk about changing a person's worldviews as if that is necessary in order to change their behavior from moment to moment. I don't think so. Even parents of young children with language delays who have experienced the power of behavior analysis directly will frequently turn around and say, "Thank God. It's a miracle"; no matter that the therapist is saying, "No, it's the systematic application of basic principles of human behavior." The parent can learn to apply the principles we teach them while rejecting our interpretation of why the principles work. I think this ability might get more at the heart of Wittgenstein's rule-following dialectic. (I don't claim to be one of the dozen or so people—in my view—who seem to grasp what Wittgenstein was saying; so this is really just a shot in the dark.) The door will always be open to mystical interpretations.

On the value of offering interpretations: "It depends on the cost of being wrong....

The point of the exercise is not to offer certainty, but to ... offer a possible solution ... without recourse to mysticism.... As long as we have one possible natural explanation ... it displaces occult explanations."

My opinion: Suppose we "displace occult explanations" of language. How will things be different? What if Skinner's interpretation of verbal behavior (or some neo-Skinnerian interpretation) became widely accepted by academics and in the popular culture alike? Would our lives (moment to moment) be all that different?

JL: There is not much difference of opinion, but there is some. As Dave notes we must have an analysis of private events. My concern is that we offer an account that does not simply take something observed and extend it (metaphorically) into the world of the unobserved to explain the actions of the observed event, and further, assume that the unobserved and the observed belong in the same categories.

The counterexamples to which I have alluded have all shown that variables other than private events have accounted for the phenomena, and that the reported private event, if actually governing or influencing behavior, would have yielded a different outcome.

There are indeed many evolutionary "just so stories" and many such descriptions have come under increasingly harsh criticism, particularly in the area of behavioral characteristics. The generally accepted ones find their basis in reference to processes examined under experimental control. Even so, recent assumptions have been turned upside down by new developmental data that have shown that comfortable and accepted interpretations in this area have slowed our understanding of the relations of genes to development. This is a problem for fields other than ours as well.

Dave's alphabet example is a good one. What the question "What is the 10th letter after H?" may occasion is an intraverbal training history, which potentiates a private occurrence, and a sequence that leads to the answer R. The question is, how much of what I think the private sequence I'm using is actually the sequence. Is it just my metaphorical way of describing it, because I have no other way of describing it, since I have no direct training in this area?

Here is a variant I saw somewhere and have used with students (particularly good with poor spellers): Write the word STRENGTH on the blackboard. Tell the students to take out a piece of paper and a pencil. Now ask the entire class to stare at the word. Tell half the class to close their eyes and visualize the word. (As a variant, choose those who claim that they can clearly picture the word with their eyes closed.) Now tell both groups to write the word, spelling it backwards. One will find that this is little problem for those looking at the word, and a more difficult task for those "clearly picturing" it. What one notices is that most of the students whose eyes are closed engage in an intraverbal sequence not unlike what Dave describes, but the students in the other group do not. The actual stimulus, and the pictured "stimulus" do not share the same properties. Was there a private event there, or did the students whose eyes were closed just think there was? At least we cannot ascribe to the private event the same categorical status as the public one. Now, if we provide a training history that involves spelling in both directions and repeat the experiment, we may find no difference between groups. Are we now to conclude that the private event acts in the same way as the public? Or, should we say that the differences or similarities between groups is a function of their program history, and that the results can be the same or different as a function of the program? The private event may or may not occur. The difference in outcomes is attributable to other variables, and an interpretation that has us overlook those variables is what I see as the real problem.

A recent *NOVA* described String Theory, for which there is no direct or indirect evidence, but it explains a lot, and is beautifully mathematically consistent. That is, so far, things we see in the world do not contradict it. Now, it is unlikely that Strings will ever be directly observed, but a whole range of technologies may indeed be developed as a result of their investigation. Or, it may all be an "ornamental lump." Nonetheless, Strings have their own characteristics and functions; they are not simply relations borrowed from the observed world and used to explain the unobserved. Private events may indeed play a major role in our ultimate understanding of behavior, but my concern is that they be treated with care and that we not assume for the sake of ease in explanation that they are simply events (perhaps stimuli and responses) that act in much the same way, and play the same roles as do events that are in direct contact with, and thereby defined by, contingent environments.

Nathan Stemmer (NS): I have an alternative proposal. Newton's hypothesis postulated the existence of non-observable gravitational forces that explained, among other things, the tides. But Newton could have complicated his hypothesis. He could have assumed that in the case of the tides, the gravitational forces had, in addition, monstrous features, or behavioral features. But although the hypothesis that attributed these additional features to the gravitational forces would also explain the tides, it would no longer be the most parsimonious hypothesis.

Suppose now that a child has appendicitis, which among other effects evokes crying behavior. We can offer the normal physiological explanation of the behavior. But we could also propose a different hypothesis, a hypothesis that adds the assumption that the inner events that generate the crying behavior had monstrous and/or behavioral features. Although the hypothesis that attributed these additional features to the inner events would also explain the crying behavior, it would no longer be the most parsimonious hypothesis.

Finally, suppose that the child not only cries but also utters "I am in pain." Which is now the most parsimonious hypothesis? In my view, it is the hypothesis which, just as in the first case, attributes the behavior to physiological factors, and the verbal behavior is explained by attributing it to the fact that the previous learning events—the events in which the child "learned" the expression "[I am in] pain"—modified the child's physiological constitution. (Quine, 1974, speaks in this connection of the "traces" that the learning events left in the physiological organism.) Therefore, the appendicitis now operates on a modified organism, and it therefore evokes not only the crying but also the verbal behavior. The assumption that in the present case, the physiological factors also have covert behavioral features—e.g., the child subvocally produces, say, seven times the subvocal substitutes of the letters "p" and "n"—does not add to the explanation, and is therefore no longer the most parsimonious hypothesis.

Notice moreover that, by definition, we can never know whether the child's inner events had covert behavioral features—e.g., whether it also included subvocal substitutes of the letters "p" and "n"—because the moment we have scientific evidence of this covert behavior, the behavior is no longer covert; it is overt.

On the other hand, physiological experiments such as those described in, e.g., Byrne (1987) and Hawkins and Kandel (1984) can indeed increase our knowledge about the neurophysiological factors, and, in particular, about the nature of the physico-chemical features of the neurophysiological effects—the traces—of the verbal learning events. In short, my proposal is to avoid attributing behavioral features to the inner events. The addition does not explain more phenomena than the more parsimonious hypothesis that restricts itself to neurophysiological factors. (See also Stemmer, 1992, 1995, 2001, in press.)

PB: Your example could work either way. It depends upon what we know about the situation. If we have independent evidence of appendicitis (tender abdomen, evidence of infection) we might assume that an underlying medical condition is the most likely explanation (and parsimony is simply one tool for identifying the most likely explanation) of the child's behavior. In other words, it would be a relatively uncontaminated tact. On the other hand, learning histories can also be assumed to result in changes in the state of an organism (we assume a neurophysiological basis for behavior even if that science has yet to provide a useful account). But neurophysiology has not yet developed to the point of meaningful control and prediction of behavior.

Analytically, one must distinguish between internal stimuli and covert responses. Both are (potentially) private events. Assigning an event to the stimulus or response category (and it may have both functions) must be based on our observations of past and present collateral events.

Ray Weitzman: In behavioral science, it seems to me that using parsimony as a criterion for choosing among explanations is highly undesirable. For example, if one explanation claims that environmental variables A, B, C, and D are the functional determinants of a certain class of behaviors and another explanation claims that just A, B, and C are the functional determinants of the behavior, then according to the "law of parsimony," the latter explanation is to be preferred to the former. But only by experimental investigation can we really determine which explanation is correct. Selecting explanations on the basis of parsimony seems to cut off scientific investigation of the phenomena we are interested in. As far as I can see, parsimony is only useful in dealing with formal systems or when the claimed independent variables are unobservable. In the latter case, we must then ask ourselves what is the value of deciding among different explanatory fictions. Parsimony doesn't seem of much value in empirical investigations.

In Nathan Stemmer's example of the child who has appendicitis and cries, neuroscience might be able to give a description of the neurophysiological relationship between the appendicitis and the crying. I can imagine that such a description would be quite complex. In a primitive society the explanation might be in terms of evil spirits inhabiting the child's body and acting in concert, one causing the appendicitis and the other evoking the crying. This seems to be a much simpler explanation. However, because the two explanations offered are framed in terms of totally different domains, the physiological vs. the spiritual, there can be no basis for really deciding which is more parsimonious. Only if the terms of the explanations lie within the same domain can parsimony be useful, but even in this case its value is highly limited.

Why then would we prefer a physiological explanation to an evil spirit explanation? Certainly not on the basis of parsimony. But could it possibly be due to our cultural history of effective contingencies of reinforcement?

TS: Parsimony is frequently invoked without further explanation. However, its invocation raises two troubling questions: (1) By what measure are we to determine which explanation is more parsimonious? (2) Why is the more parsimonious explanation preferred? In his post, Ray Weitzman rightly (in my judgment) raises the second, logically prior question. As Elliot Sober (1993) has noted, this topic has not escaped the attention of philosophers of science. For example, Quine (1966) argued that simpler explanations are more probable, while Popper (1959) maintained that they are more falsifiable. For Sober himself, there is no *global* justification for invoking parsimony; rather, "justification for using parsimony ... depends on assumptions that are specific to the inference problem at hand." (Sober, 1993, p. 105)

NS: Brandon admits that we may have knowledge of some of the biological bases of behavior, but "not yet to the point of meaningful control and prediction of behavior." This is certainly true. But this also holds for our knowledge of the behavioral features of the covert-behavior events. Our knowledge of the behavioral dimension of these events—e.g., whether they contain seven times the subvocal substitutes of the letters "p" and "n"—is insufficient for basing on them our predictions of the child's behavior. As Brandon suggests, our predictions will "be based on our observations of past and present collateral events." But this is also the strategy adopted by the physiologists. Our predictions of the child's behavior will not be based on our physiological knowledge but rather "on our observations of past and present collateral events." As Quine observes, "the physiological is the deepest and most ambitious" level of our explanations of behavior ... "and it is the place for causal explanations." But presently the "behavioral level ... is what we must settle for." (1975, p. 87) It follows that physiologists, too, have at present no alternative but to look for environmental factors on which to base their predictions of the subject's behavior. To formulate my position in a sharper way: I assume that Brandon agrees that the covert-behavior events are physiological events. But he makes an additional assumption. He assumes that these physiological events have also behavioral features. My argument is that this additional assumption is superfluous, because it does not increase our controlling and predictive power. This power continues to derive from "our observations of past and present collateral events."

I agree that Brandon's additional assumption may be quite innocent. It does not necessarily deviate us from our goal of looking for the environmental variables that control the behavior. But there is a disadvantage. Eventually, behavior analysts intend to establish a dialogue with other psychologists. This will probably not be an easy task, mainly because most psychologists, even those who consider themselves monist materialists, are influenced by mentalist ideas. But one obstacle to such a dialogue can be removed, namely, the use by behavior analysts of the notion of covert behavior. This notion is so strange, so bizarre, for people who have not been educated in a behavior analytic environment, that they immediately turn away from any position that admits such inner events. But this reaction can be avoided because there is no need at all for behavior analysts to introduce the notion of covert behavior. All relevant phenomena can be accounted for by assuming only past and present environmental factors. And if we look for a causal explanation, then we add that these environmental factors operate on the subject's physiology, and this causal factor then generates the relevant behavior.

PB: I'm not sure that the (implicit) dichotomy between physiology and behavior is necessary. Behavioral events are a particular subset of physiological events; covert behaviors in turn a further subset. Of course not all physiological events are covert nor are they all behaviors.

As for the claim that other psychologists find the notion of covert behavior bizarre, this has not been my experience. Do your cognitivist colleagues deny that people think, and that thoughts cannot be observed by others? The main obstacle seems to be the epistemological one—that thoughts are not causes of overt behavior.

NS: Paraphrasing Brandon, I hope faithfully: Covert behaviors are inner (covert) physiological events, and these events have additional behavioral features. If we now apply this terminology to the example of the child who produces the word "pain," we receive the following conclusion: The child's verbal behavior is evoked or occasioned by an inner physiological event (probably together with other inner and outer factors), and the inner event has behavioral features.

In my previous communications, I argued that the assumption that the inner event is not only a physiological event but also has behavioral features does not increase our capacity for control and prediction of behavior. I therefore concluded that "this additional assumption is superfluous." But maybe I was mistaken. I therefore raise the following question: (1) Does the attribution of behavioral features to the inner event increase our capacity for control and prediction of behavior? A positive answer suggests the further questions: (2) How is the increase in controlling and predictive capacity achieved? (3) Is there a method that enables us to establish the physical dimensions of the behavioral features of a specific covert-behavior event? For example, can we answer the question of whether the covert behavior, which (perhaps together with other factors) evokes or occasions the child's utterance of "pain," contains subvocal substitutes of the letters "p" and "n"?

PB: All behaviors are physiological events, but I'm not sure how a behavior can be said to have "behavioral features." What does this statement add? In your example I don't see a covert behavior, just an internal stimulus. While in practice this stimulus is usually covert, such inner physiological events are usually amenable to overt observation (i.e., medical examination). Thus we are analyzing the overt verbal report of private (covert) stimuli, a behavior which we assume has been acquired through the appropriate reinforcement of verbal reports of publicly observed painful events, and maintained by reinforcement based on the observation of collateral events. I would not term the private event controlling the word "pain" a *behavior*, as it is not under the control of either environmental antecedents or consequences. Rather, it can serve both functions. In this case it is a discriminative stimulus (the antecedent) for the child's emitting the overt verbal response "it hurts," correlated with reinforcement by attention from adults (the consequence), and it might also lead to alleviating the pain (another consequence). The parent's response to the child's verbal behavior might in part be based on the observation of the respondents elicited by the same internal event (respondent rather than operant crying).

Matthew Normand: Regarding Paul Brandon's re-statement of "pain" as a stimulus and not a behavior, it seems to me that painful event would function as a Motivating Operation (MO) rather than an S^D, as things such as parental attention are just as available in the absence of the pain as they are in the presence of pain. That is, "pain" is related to the differential effectiveness of the various forms of reinforcement rather than the differential availability. The verbal response of "pain" (or some other response that has, in the past, been followed by reduction in pain or parental attention) is then momentarily frequent due to the evocative effect of the MO, and the consequence is more effective due to the establishing effect. Various other stimuli or stimulus conditions, such as the presence of parents or other caregivers, would function as S^Ds, as they are correlated with the differential availability of relevant reinforcers. (Most people typically do not mand in the absence of listeners.)

Thus, the "pain," as a Motivating Operation, determines the form of the verbal response, establishes the effectiveness of certain consequences, and evokes the types of behavior that have in the past produced those consequences. Perhaps there are several types of MOs corresponding to different aspects of the situation: e.g., parental attention and pain reduction. Whether or not the response occurs is controlled by an S^D, such as the presence of members of a verbal community.

PB: Good point. I'd still say that "pain" could have both manding and tacting functions. I will agree that in the example as presented the main function would be manding, and thus the controlling functions of the internal events would be MO's (establishing operations, to use slightly older terminology). However, I suspect that there would be tacting functions as well. A response to "Where does it hurt?" would be a tact.

[Following this entry, the forum drifted into other topics.]

Summary

The discussion ended without consensus, which is not surprising, considering that it was unstructured and spontaneous, that it was distributed over several weeks, and that its participants composed their entries in isolation. But most of the disagreements were about policy and strategy rather than about the place of private events in the science of behavior. Some participants, notably Eshleman and Brandon, raised mainly methodological and empirical questions: What is the role of amplitude in determining units of analysis? Can low amplitude responses be studied if measures of interobserver reliability cannot be obtained? How does thinking a response differ from speaking a response, both conceptually and practically? These questions set the occasion for the subsequent discussion, but they were not answered.

Disagreements arose over the value of interpretations that invoke private events. Palmer urged the liberal use of interpretation to lay claim to a subject area, that is, to provide tentative explanations for phenomena for which experimental analysis is impossible, a position he has discussed at greater length elsewhere (Donahoe & Palmer, 1989; Palmer, 1991, 1998, 2004). McDonough suggested that such interpretations have little force and that our time is better spent working on experimental resolutions of puzzling questions than on trying to persuade others of the potential scope of a behavioral analysis. In the same vein, Layng, invoking Wittgenstein's "skeptical paradox," argued that the virtues of interpretations are often outweighed by the problems they raise. In particular, he cautioned against the uncritical assumption that private events are no different in kind from their public counterparts and that they necessarily play a causal role in the flow of behavior. He argued that if more effort were devoted to the control of behavior by programs of contingencies distributed over time, some of the pressure for ad hoc interpretations would be dissipated. Accounts that ignore such contingencies are likely to be misleading. Schoneberger echoed these reservations and cautioned against a climate in which "anything goes." Fortunately, these viewpoints are not mutually exclusive and mainly reflect differences in emphasis or differences in preferences.

Michael urged discussants to distinguish sharply between the terms *stimulus, response,* and *response product.* The tendency to attribute stimulus control to a response rather than to a response product is imprecise.

Stemmer suggested that private events be viewed exclusively as physiological events; endowing them with behavioral properties is a gratuitous assumption that adds nothing to the account. The physiological account, he argued, is preferred on grounds of parsimony and the extent to which it permits easy dialogue with those in other fields. But Weitzman and Schoneberger remarked that parsimony, by itself, is an inadequate criterion for deciding between alternative proposals in the behavioral sciences. Once again, the virtues of experimental analysis were advanced. Brandon noted that physiological accounts are of little use in prediction and control. Stemmer asked what use inferences about covert behavior are in prediction and control, referring to a hypothetical example of a subject reporting pain. Brandon and Normand refined the terms of Stemmer's question but left it, in its general form, unanswered.

To bring some closure to the discussion, I will propose an answer to Stemmer's question here. The reason for distinguishing behavioral events from physiological events is that science has formulated principles of behavior from tightly controlled laboratory investigations. We can bring these principles to bear on the interpretation of behavioral events, but not other kinds of events. A response of interest may be unobservable on some occasion, but it is unlikely that all relevant behavioral variables are equally obscure. We are likely to have access to relevant discriminative stimuli and motivational variables, and we may know something of relevant historical contingencies as well. Thus, when we ask a question that requires counting letters of the alphabet (e.g., "What is the 10th letter after H?"), we have arranged motivational and discriminative variables that potentiate reciting the alphabet as an intraverbal chain. If the matter were of sufficient importance, we could even gather data on the subject's acquisition of that intraverbal chain. Having asked the question, we cannot be certain that the chain will be emitted, but we can be confident that it has increased in probability, for our behavioral principles, derived independently, tell us so. If additional variables are present that would punish overt responding (e.g., the instructions to "do it in your head"), we have objective reasons for predicting that an overt response will not occur. Finally, when a relevant overt response does occur (the subject says, "R!"), we can suppose that some relevant discriminative stimulus evoked it. We know from the subject's history that one such discriminative stimulus is the response product of saying "Q," a part of the putative intraverbal chain. Thus, armed with the data at hand, knowledge of the subject's history, and a set of principles of behavior, we can make predictions about covert responses and even control them. We cannot do so with certainty, but neither can we do so with overt behavior. Of course, we can never confirm our predictions or demonstrate our control, but the purpose of interpretation is not to offer proof but to make sense of the world.

Stemmer is presumably right that eventually one might offer a parallel account at the level of physiological processes, appealing to principles that have emerged from corresponding experimental analyses. However, the relevant variables are likely to be as private as the behavior under investigation. One would presumably have to measure simultaneously the activity of thousands or millions of neurons, all without perturbing the phenomenon under study. Behavioral principles do not have any conceptual or empirical superiority to physiological principles, but the relevant variables are usually far more easily measured and manipulated. It was just such practical considerations that led Skinner to argue, in the early years of our field, for an independent science of behavior.

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