RELATIONAL OPERANTS: PROCESSES AND IMPLICATIONS: A RESPONSE TO PALMER'S REVIEW OF RELATIONAL FRAME THEORY

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Palmer has recently criticized Relational Frame Theory (RFT) on the grounds that it has developed data in search of a principle. In this reply, we show that he has done so by attacking fundamental concepts within behavior analysis itself, including the functional nature of an operant and contingencies of reinforcement as a behavioral process. His claim that RFT appeals to new behavioral principles to explain the development of relational operants is shown to be incorrect: As with any operant, RFT appeals to a history of contacted consistencies in contingencies across multiple exemplars to explain them. New principles only emerge later as a logically necessary extension of such operants if they exist—a view that Palmer failed to address or appreciate. Palmer's desire to see the use of methods other than matching-to-sample is proper but already largely satisfied in the empirical literature on RFT. We show Palmer's defense of Skinner's definition of verbal behavior to be illogical and unresponsive to the empirical challenge behavior analysis faces. Palmer's alternative common sense mediational associationistic account is another in more than a century of such accounts, all of which have failed empirically. At its root, Palmer's criticism is based on a mechanistic philosophy that is hostile to a traditional functional behavior analytic approach.

Key words: Relational Frame Theory, verbal behavior, functional operants, mechanism, contextualism

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In "Data in search of a principle," Palmer (2004) questions the clarity and importance of Relational Frame Theory (RFT: Hayes, Barnes-Holmes, & Roche, 2001a). Palmer characterizes his review as conservative and behavior analytic, but far from conserving the behavior analytic tradition, it challenges the validity of fundamental concepts within that tradition, at least as we understand them. Furthermore, it appeals instead to mediational associative processes that are a century or more old, are more characteristic of stimulus-response psychology than behavior analysis, and that have so far shown themselves to be inadequate to the task of developing an adequate behavioral psychology of cognition. Palmer is not the first behavioral critic to adopt this strategy (e.g., Burgos, 2003; Tonneau, 2002). In this reply we will speak to the details of Palmer's review, but our response applies with equal force to these issues as raised by others (e.g., Burgos, 2003; Tonneau, 2002; see also the replies by D. Barnes-Holmes, Hayes, & Roche, 2001; D. Barnes-Holmes & Hayes, 2002, 2003; Hayes, Barnes-Holmes, & Roche, 2003).

Reinforcement Contingencies, Behavioral Processes, and Relational Operants

In discussing the concept of the relational frame, Palmer appears to be attacking the traditional and well-established concept of the operant itself. The critical section from the RFT book that Palmer cites is as follows:

> A relational frame is thus both an outcome and a process concept. The contextually controlled qualities of mutual entailment, combinatorial mutual entailment, and transformation of stimulus functions are outcomes, not processes. They do not explain relational frames; they define them. The process is the history that gives rise to a relational operant that is under a particular kind of contextual control. Stated another way, the process involved is contingencies of reinforcement, but unlike Sidman (2000) relational responding is not a previously unknown secondary effect of such contingencies, it is the target of them. (Hayes, Fox, et al., 2001, pp. 33–34)

Palmer breaks this section down into the following three propositions:

1. "The process is the history that gives rise to a relational operant."
2. "The process involved is contingencies of reinforcement."

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3. "Relational responding is not [an] effect of such contingencies; it is the target of them." (Palmer, 2004, p. 195).

Palmer states that this is muddled because the third statement contradicts the other two. This muddle comes only because Palmer incorrectly paraphrases the quotation. As the actual quotation shows, we argued that relational responding is a *primary* effect of such contingencies (i.e., a direct target of them) not a *secondary* or accidental effect. There is no contradiction between that claim and the other two. Palmer also complains that a contingency is not a process (although he admits that contingencies are relevant). Again, the longer quotation clarifies what we were speaking about. Behavior analysts have long used terms like "contingency" or "reinforcement" to speak about both experimental operations and behavioral processes (Catania, 1998, p. 69–70). As an operation, reinforcement refers to the externally programmed relation between a response and a consequence, whether or not it is contacted or comes to control behavior, and indeed in such usage no behavioral process is implied. As a behavioral process, the term refers to changes in behavior that occur based on contact with particular kinds of response-consequence relations. The longer quotation makes clear that we were speaking of contingencies of reinforcement in the second sense because in referring to contingencies we explicitly noted that we were describing the subject's history "stated in another way" (Hayes, Fox, et al., 2001, p. 34). Thus the penultimate sentence of the above quotation should be interpreted as follows: "the process is the history [of contingent reinforcement] that gives rise to a relational operant." Our use of "contingencies of reinforcement" as a behavioral process is fully in accord with established practices in behavior analysis.

Palmer also seems to stumble over the idea that a relational frame involves both a description of behavior and an appeal to history. Palmer asks the following rhetorical question, "Is a relational frame a class of behavior . . . or is it the history that produced that class of behavior? That's two concepts, not one, and they cannot be fused into one simply by giving them the same name" (p. 194). In raising this issue, Palmer appears to be challenging the functional definition of operant behavior itself, presumably in favor of a more topographical or mechanistic definition that seems to us to be out of keeping with traditional behavior analytic thinking.

The definition of operant response classes includes the history of contingent reinforcement that gave rise to them—the operant does not exist alone and cut off from that history. Imagine, for example, that we observe a rat pressing a lever that delivers a pellet of food, which the rat then eats. Seeing this, could we define this behavior as an operant response? Surely not—the lever press may have been elicited, for example. To define an instance of behavior as a member of an operant response class requires that it occur *because* of a history of differential reinforcement for responses within the *descriptive* or *nominal* class (Catania, 1998, p. 117). Responses may also occur that fall outside the descriptive class, and thus are not reinforced, but these, too, are defined in terms of the history of reinforcement for responses that fell within the descriptive class (Catania, 1998, p. 117–118).

If one accepts this well-established definition of operant behavior, it should be completely noncontroversial to define a relational operant in terms of the history of reinforcement that gave rise to the relevant relational properties (e.g., mutual and combinatorial entailment, and so on). To define an operant class independently of the history that produced it transforms the operant from an explanatory and functional-analytic unit into a descriptive and topographical concept. If we restate Palmer's objection to relational frames, but simply replace the term "relational frame" (which is claimed to be a type of operant) with the more general term "operant," the fundamental contradiction between Palmer's views and traditional behavior analysis becomes more obvious: "Is an operant a class of behavior or is it the history that produced that class of behavior? That's two concepts, not one, and they cannot be fused into one simply by giving them the same name." Palmer may be willing to embrace the fundamental change in traditional behavior analytic thinking this quotation reveals, but it is incumbent upon him to demonstrate what will be gained by doing so.

Is There A New Behavioral Principle Involved in Relational Frames?

Palmer repeatedly claims that RFT appeals to a new but mysterious principle: ''a new behavioral principle is assumed, but in contrast to the thesis of Sidman (2000), none is explicitly described'' (p. 200). Supposedly we appeal to this mysterious principle to account for relational frames: ''derived relational responding is the product of a special principle, unmediated by other events'' (p. 199).

The only place we are quoted directly to this effect is in the following quotation, which taken out of context, seems to support Palmer's claims: ''Despite the conservatism of an RFT approach, therefore, a new type of behavioral process is suggested and a new technical term is offered. The new process is arbitrarily applicable relational responding (or framing events relationally)'' (Hayes, Fox, et al., 2001, p. 46).

This quotation, however, comes at the end of a section in which we state quite specifically why a new principle must be involved in relational framing if RFT is correct. We do not understand why Palmer has difficulties understanding this claim, when other reviewers do not (e.g., Galizio, 2003). As we have reiterated elsewhere in response to critics (D. Barnes-Holmes & Hayes, 2003), RFT does *not* explain relational operants through an appeal to *any* new principles. Functionally oriented behavior analysts (e.g., Catania, 1998, p. 158; see quotation later in this article) seem to have no problem understanding our idea that relational operants are explained by differential reinforcement, and differential reinforcement is hardly a new behavioral principle. The new principle in RFT comes because of the *logically necessary implications* of such operants if they exist.

We will quote from the critical section of the RFT book so that readers can determine for themselves if we have failed to specify the nature of this process or the reasons for it:

> The instrumental behavior of relational framing alters the functions of other behavioral processes. . . . Said another way, relational framing is operant behavior that affects the process of operant learning itself. Behavior analysts distinguish between stimulus functions on the basis of history and current context. . . . If relational framing is a learned process of altering behavioral processes, we need a name for a previously unseen behavioral effect. Consider the case of a transformation of stimulus functions. A discriminative stimulus is a stimulus in the presence of which there has been a greater probability of reinforcement for a given behavior than in its absence. Suppose a child is rewarded for waving when the word ''dog'' is heard. The word ''dog'' is a discriminative stimulus. Suppose, however, that the child is now taught to say ''dog'' given the word D-O-G, and to point at actual dogs given D-O-G. Suppose that as a result of this training the child now waves upon seeing a dog. Such an outcome has repeatedly been seen in the literature (e.g., Hayes, Brownstein, Devany, Kohlenberg, & Shelby, 1987). The dog cannot be a discriminative stimulus because the child has no history of greater reinforcement for waving in the presence of dogs than in the absence of dogs. The effects cannot be stimulus generalization because there are no formal properties that are shared between the word and actual dogs. The effect cannot be due to classical conditioning because it would require an appeal to backward conditioning. The effect cannot be due to compounding because ''dog'' and dogs have not even occurred together. Relational Frame Theory suggests that the performance is due to a learned process that transformed these discriminative functions. In normal discriminative control, the stimulus function is learned, but not the process itself. In contrast, the derived performance is discriminative-like, but it is not discriminative. These discriminative-like effects seem to depend on a learned process of altering behavioral processes, and that is something that is not covered by an existing technical term. Despite the conservatism of an RFT approach, therefore, a new type of behavioral process is suggested. (Hayes, Fox, et al., 2001, p. 45–46)

We clearly are not arguing in this section that such a process *explains* or *gives rise to* relational operants. We are not even yet arguing that this process is proven. Thus Palmer's previously cited claims that we are ''assuming'' (p. 200) a new principle and that relational operants are the ''products'' (p. 199) of this ''special principle'' (p. 199) are simply false. We are saying *if* there are relational operants of the kind envisioned by Relational Frame Theory *then* we need a new process to accommodate the behavioral effects that relational operants appear to generate. We see only two scientifically healthy ways that behavior analysts can avoid this conclusion:

demonstrate that it is illogical or conclude on empirical grounds that there are no such operants.

It does not seem to be illogical. Consider a different but parallel example to see if the conclusions are necessary. Suppose it was possible to establish or strengthen backward conditioning through operant procedures (Hayes et al., 2003). Imagine, for instance, that an animal is presented with several CS-UCS pairings. Subsequently, the UCS is presented alone and reinforcement is delivered contingent upon the animal producing a response similar to that originally evoked by the CS. In our imaginary experiment, reinforcers might be delivered to one of Pavlov's dogs if it showed some minimal auditory response when presented with meat powder. Over scores of trials, the magnitude of the response required and the types of CS and UCS events employed could be varied widely. Imagine now that this operant contingency trained our subject in some contexts to show strong CS-related responses to the UCS following any new set of CS-UCS pairings. It would be improper to label the result "backward associative conditioning" which would falsely imply that the process involved was respondent conditioning per se: only the preparation was respondent; the process was operant. It would also be improper to discuss the result without mentioning this operant history. Furthermore, it would not be "conservative" for behavior analysts to reject the data merely because a new name is needed to describe a previously unrecognized effect in which an operant modified classical conditioning processes. This is analogously the kind of situation we find ourselves in regarding the central claim of RFT. If there are indeed relational operants that modify other learning processes, then a new technical term is needed because no existing principle seems to capture this modifying effect.

Of course, as an empirical matter RFT may be incorrect. It is up to RFT researchers to provide the evidence, and they have been busy doing so. There are now over 60 empirical studies on RFT, and the data are very consistently supportive. We are not claiming that these data are yet conclusive, but the central claims become more and more plausible as additional evidence emerges. Behavior analysts cannot stand on the sideline forever, however, as if this were merely a matter of preference, terminology, or philosophy. Relational operants of the sort that RFT posits either exist or they do not. We believe that this question is entirely answerable empirically, and indeed must be so if behavior analysis is a fully viable scientific area. If a behavior analyst argues otherwise, then the claim is being made that operant psychology has no empirical way to determine if critically important behaviors are operants—which would make "operant psychology" a considerably less worthwhile area, empirically speaking.

If such operants do exist, we must face their logical implications. We believe that the implications would be very large indeed because a new principle would be established that could alter the impact of all other behavioral principles. That is good reason to be cautious, but it is not a good reason to reject the theory.

Palmer also seems to have a concern over our use of the term "generalized operant class." He argues, for example, that this concept is "vague and troublesome" (p. 195). Here, too, Palmer is out of step with behavior analytic traditions. The principles and procedures that are employed to explain relational operants in RFT are identical to those used to explain any operant: contacted consistencies in contingencies across multiple exemplars. Catania's (1998) undergraduate text book, *Learning,* contains the following, apparently noncontroversial statement on generalized operants and relational frames:

> . . . training with many instances may sometimes be a sufficient prerequisite for higher-order or generalized classes (e.g., training with many symmetry problems may produce generalized symmetry, training with many transitivity problems may produce generalized transitivity, and so on; such generalized classes have been called relational frames . . .). (p. 158)

The concept of a generalized operant class should thus be relatively unthreatening: it is a traditional behavior analytic concept. Although there are more and more data on this issue (e.g., Y. Barnes-Holmes, Barnes-Holmes, Friman, & Smeets, in press; Y. Barnes-Holmes, Barnes-Holmes, Smeets, Strand, & Friman, in press; Healy, Barnes-Holmes, & Smeets, 2000), it is fair to point out that the specific histories that give rise to relational

frames have not yet been fully demonstrated, but that is not because those histories are mysterious: it is because the relevant demonstrations are an empirical matter. By the time such empirical evidence has been fully demonstrated there will be (or should be) nothing left to argue about within behavior analysis regarding relational operants. But conducting such empirical work requires considerable clarity about the unit of behavior that is being shaped—clarity about that unit and its implications are precisely why Relational Frame Theory is needed at this point.

Restriction to Matching-to-Sample Procedures

Palmer writes that some of his objections to RFT would be addressed if the range of dependent variables and experimental procedures were expanded, mentioning alternatives such as brain imaging, eye-tracking, response latencies, talk-aloud protocols, developmental studies, use of distractor tasks, ostensive naming, and other procedures. It is encouraging that Palmer's suggestions for future research seem to overlap with what has already been done or is presently planned. Current preparations include the Relational Evaluation Procedure (e.g., D. Barnes-Holmes, Healy, & Hayes, 2000; Hayes & Barnes, 1997; O'Hora, Barnes-Holmes, & Roche, 2001; O'Hora, Barnes-Holmes, Roche, & Smeets, in press; Stewart, Barnes-Holmes, & Roche, in press), which leads to a successful model of ostensive naming; respondent-type pairing procedures (e.g., Barnes, Smeets, & Leader, 1996; D. Barnes-Holmes, Barnes-Holmes, Smeets, Cullinan, & Leader, in press; Leader, Barnes, & Smeets, 1996; Leader & Barnes-Holmes, 2001; Smeets, Leader, & Barnes, 1997), which have shown that it is possible to establish derived relations without matching-to-sample training with both adults and children; precursors to the relational evaluation procedure (e.g., Cullinan, Barnes, & Smeets, 1998; Cullinan, Barnes-Holmes, & Smeets, 2000, 2001; Smeets, van Wijngaarden, Barnes-Holmes, & Cullinan, 2004), which have highlighted the relational, as opposed to the class-based properties of equivalence relations; developmental studies (Barnes, McCullagh, & Keenan, 1990; Devany, Hayes, & Nelson, 1986; Lipkens, Hayes, & Hayes, 1993; McHugh, Barnes-Holmes, & Barnes-Holmes, 2004; Peláez, Ge-

wirtz, Sanchez, & Mahabir, 2000), each of which has found evidence for the functional overlap between derived relations and language development; talk-aloud procedures (Cabello, Luciano, Gomez, & Barnes-Holmes, in press), which provide evidence for a real-time correlation between derived relational responding and human language; response latencies (O'Hora, Roche, Barnes-Holmes, & Smeets, 2002; Steele & Hayes, 1991), which provide chronometric evidence for the existence of different classes of relational operants; semantic priming and implicit association test procedures (D. Barnes-Holmes, Staunton, et al., in press; Hayes & Bissett, 1998; Staunton & Barnes-Holmes, 2004), which show the functional similarity between derived stimulus relations and common sense verbal categories; and the recording of event-related potentials (D. Barnes-Holmes, Staunton, et al., in press; Staunton & Barnes-Holmes, 2004), which provide evidence for the semantic-like properties of equivalence relations.

Palmer worries that only equivalence has been demonstrated in young children, but recent research has shown derived relational responding in accordance with comparative and opposite relations in normally developing preschoolers (Y. Barnes-Holmes, Barnes-Holmes, Friman, & Smeets, in press; Y. Barnes-Holmes, Barnes-Holmes, Smeets, et al., in press); mutual exclusion has been shown in human infants (Lipkens et al., 1993); the development of deictic frames has been studied naturalistically using a cross-sectional design (McHugh et al., 2004); and a whole series of studies have developed RFT models of analogical reasoning using both adults and children (Barnes, Hegarty, & Smeets, 1997; Carpentier, Smeets, & Barnes-Holmes, 2002, 2003; Carpentier, Smeets, Barnes-Holmes, & Stewart, in press; Stewart & Barnes-Holmes, 2001, 2004; Stewart, Barnes-Holmes, & Roche, 2002, in press; Stewart, Barnes-Holmes, Roche, & Smeets, 2001, 2002). The current body of work has only scratched the surface of what needs to be done to explore the empirical implications of RFT as a theory of human language and cognition, but it is hardly the case that the analysis stands on matching-to-sample preparations alone.

Skinner's Definition of Verbal Behavior

In the RFT book we suggested that the failure to mount a robust experimental program on language and cognition is an inevitable result of Skinner's definition of verbal behavior. Stripped to its essentials, our argument is that the definition is: (a) not a functional one in a behavior analytic sense, because it is not based on specific aspects of an individual organism's history but on aspects of some other organism's history (namely that of the audience trained to mediate reinforcement to the speaker); (b) it is so broad as to include virtually all animal operant behavior in traditional behavior analytic research; and thus (c) "any attempt to apply the analytic categories described in the book [*Verbal Behavior*] lead basic behavior analysts inexorably back to what they were already doing in the [animal] laboratory" (Hayes, Blackledge, & Barnes-Holmes, 2001, p. 15). Palmer admits the truth of all of these points, but claims surprise at our interest in them.

Palmer's defense of the Skinnerian account is two-fold. First, he claims that Skinner's definition of verbal behavior is irrelevant to his book on the topic: "even if it were true that Skinner's definition is inadequate, nothing follows, since his definition is little more than a footnote; it plays no role whatever in his analysis. Even if his definition is not functional, his analysis certainly is. Nothing in his discussion of tacts, mands, intraverbals, autoclitics, multiple causation, the audience, composition, editing, thinking, to name just a few topics, rests upon his definition. Damning a 470-page book of cogent behavioral interpretation because one disagrees with a few incidental paragraphs is a curious overreaction" (p. 202). This seems to us to be a rather bizarre defense. A definition of an analyzed domain is not an "incidental paragraph." Palmer is in essence trying to strengthen adherence to Skinner's account by appealing to a dramatic and inexplicable disconnection between fundamental aspects of it. That is not much of a defense. If it is true that Skinner's analysis of verbal behavior has no relation to his definition of it, then that alone should cause the book to be put aside. But Palmer's claim is *not* true. Tacts, mands, and the other units Skinner describes only make sense if you understand his definition of verbal behavior. If the definition is fundamentally flawed, the analysis is fundamentally flawed.

Second, Palmer admits that Skinner's "definition embraces the behavior of animals in operant experiments" (p. 202). His defense is that this was Skinner's whole purpose: "the very point of his analysis is that verbal behavior is *not* different in kind from other behavior" (p. 202). This does not disturb Palmer because he claims that verbal behavior is only special because of "its power, and it is powerful only insofar as it affects the conditioned behavior of other people in systematic ways" (p. 202). Palmer uses the following example of how "powerful" verbal behavior can be even in animal preparations, in comparison to nonverbal behavior: "By simply changing the verbal contingencies of our small verbal community, we can leave the nonverbal rat in the dust. Now let every lever press no longer mean (so to speak), 'Give me a pellet,' but instead mean, 'Give me a 50-pound bag of rat chow and access to a female in estrus' " (p. 203). Palmer, in essence, is saying yes, an animal foraging for food in the natural environment is engaging in nonverbal behavior by Skinner's definition, and yes, an animal pushing a bar for food in an animal operant experiment is engaging in verbal behavior by Skinner's definition (because the food presentation is mediated by an audience specially trained to do so, namely, the experimenter); but no, this is not worrisome (even though from the point of view of the rat the two situations could be functionally identical) because the bar presses of the "verbal" animal in the latter case could just as easily "mean" not simply "give me a pellet" but also "give me a whole bag of chow and a female in heat" or any anything else we could imagine. This example, however, begs the very question that it is designed to answer—exactly how are these powerful effects produced by verbal behavior?

Palmer sees the problem and attempts a solution: "The characteristic effects depend on the presence of a verbal community whose members have all acquired a standard repertoire with respect to verbal stimuli. Skinner's definition was a way of operationalizing in behavioral terms the manipulation and interpretation of symbols" (p. 202). In other words, the "power" of such behavior depends

not upon the "verbal" rat but upon the presence of listeners who somehow mediate such reinforcement to speakers due to their "interpretation of symbols." Rather than being a source of support for Skinner's analysis, however, it is the inability to explain that very process that has been a foundational failure, as we point out in the book (e.g., Hayes, Fox et al., 2001) and elsewhere (e.g., Hayes & Hayes, 1989). In Skinner's analysis, the behavior of the listener is not verbal at all and thus requires no special analysis (Skinner, 1957, pp. 2, 34); verbal stimuli are merely the products of verbal behavior—they have no special functional status for the listener qua listener (Skinner, 1957, p. 34); and there *are* no "symbols" that are issued by the speaker and "interpreted" by the listener. This is precisely why Skinner's account of rule governance has no coherent way to define what it means to "specify" a contingency without going afoul of his definition of verbal behavior and of verbal stimuli (Hayes & Hayes, 1989; Parrott, 1984). Thus, although verbal human beings can indeed provide extremely powerful audience control, this fact alone cannot save Skinner's account because his analysis of verbal behavior explicitly declines to deal with the symbolic or verbal nature of the stimuli controlling that audience behavior. RFT explains the behavior of both the speaker and the listener and in the same terms. Indeed, it provides an account that could conceivably explain how it is that human experimenters can provide a female in heat, a pellet, or a whole bag of rat chow at will as a consequence for arbitrary actions such as bar presses. Conversely, Palmer has no way of positing an audience of rats who would arbitrarily do the same thing without the intervention of verbal human beings, and empirically no such demonstrations have been offered.

Furthermore, if Palmer is right (i.e., Skinner's definition stands because the "verbal rat" is advantaged via the powerful response of the human audience), we should then logically be able to model in the animal operant laboratory the claims Skinner makes for the effects of verbal behavior. For example, Skinner claims that self-knowledge leads to greater self-control because of verbal behavior (Skinner, 1974, p. 35). This seems to be true in human beings (e.g., Bentall & Lowe, 1987), but although it is easy to shape self-

knowledge in nonhumans (e.g., Shimp, 1982), we are unaware of any demonstration that such behavior leads to increased self-control. From an RFT perspective, the bidirectionality of relational frames provides a ready explanation for the difference, and indeed RFT research has shown that human self-discrimination responses readily participate in derived relations (Dymond & Barnes, 1994, 1995, 1996, 1997).

Creating Progress

Palmer claims to be puzzled by our emphasis on the problems of Skinner's approach, despite the fact that we are behavior analysts. In the book, we are very clear about why we are concerned: it seems to us that Skinner's analysis "moved the field into empirical cul-de-sacs" (Hayes, Blackledge, et al., 2001, p. 19). As the RFT research program picks up steam, it becomes increasingly clear that such is not the case with RFT. The nature of Palmer's defense of Skinner, and his presentation of a vague covert mediational account of derived stimulus relations, serves only to increase our fears.

Consider Palmer's claim that behavior analysts are already studying verbal behavior in animal operant work and that this was Skinner's whole point. We find this worrisome. The lack of a robust and recognized research program on human language and cognition has caused basic behavior analysis to be pushed aside almost to the point at which the continuance of the field (other than in applied areas) is in doubt. When animal learning researchers leave well-known universities they are rarely replaced. When research grant dollars are distributed, basic behavior analysts are rarely at the front of the line. Many graduates of the best behavior analytic basic research laboratories will have few academic alternatives other than jobs in master's programs or four-year colleges, where it is impossible to produce additional doctoral graduates. It is hardly protective of behavior analysis to allow such trends to continue.

Palmer does offer an alterative account, but it is more in keeping with failed stimulus-response analyses than with behavior analysis. In Palmer's account, relational performances occur because humans talk themselves through relational tasks: "SAB . . . Looks like Saab . . . three stars down here and three up

there . . . One star, three, six . . . Last time I picked the one that matched, but this one has SAB at the top . . . I'm going to pick the three stars . . . Oops, I guess that wasn't it'' (p. 197). His evidence is based on common sense examples, citing such things as a story drawn from *Alice in Wonderland* (p. 199).

On empirical grounds we are a bit at a loss to explain why covert associationistic mediational analyses seemingly continue to have such an appeal (we will offer a more philosophical explanation later), but several other RFT critics (e.g., Burgos, 2003; Malott, 2003), have appealed to them as well. In every case of which we are aware, the analysis has: (a) been relatively nontechnical, usually based heavily on introspection; (b) failed to include specific data of relevance to the account; (c) required an appeal either to unknown behavioral processes (e.g., robust backward conditioning) or to methodological concerns that would apply to only a fraction of the RFT literature (e.g., that subjects in some preparations can look back and forth between samples and comparisons and thus stimulus sequence may not be controlled for); (d) been unaccompanied by technical explanations of how these putative covert associative processes could explain other than a small corner of RFT data even if successful; (e) been unaccompanied by proposed studies that would prove the account to be true or false, or that would distinguish it from predictions from RFT (RFT would predict much the same thing as Palmer in his ''White Queen'' example); and (f) failed to note the long history of failed accounts of this kind. Palmer's common sense covert association model retains all of these unfortunate features.

RFT does reject a mediational account of operants (Hayes, Fox, et al., 2001, p. 34), as Palmer notes, but that should not be taken to mean that RFT rejects the idea that there are sequences of actions, including private actions, involved in complex tasks. Rather, our point was that functionally defined operants are explanatory terms in behavior analysis; they do not depend on other hypothesized mediating processes. In the sense in which he means it, however, Palmer's claim that RFT ''obliquely reject[s] a role for mediating events'' (p. 197) is demonstrably false. The RFT book contains several chapters that are almost entirely devoted to how humans can use relational frames in complex sequences to reason and problem-solve (e.g., D. Barnes-Holmes, Hayes, & Dymond, 2001; D. Barnes-Holmes, O'Hora, et al., 2001; Hayes, Barnes-Holmes, & Roche, 2001b: Hayes, Gifford, Townsend, & Barnes-Holmes, 2001). Humans can indeed talk themselves through relational or other cognitive tasks, once they have a sufficient repertoire of relational frames to do so, and the book explicitly encompasses self-rules of the sort Palmer describes. We acknowledge the ubiquity of such self-rules and explain why it is necessary to assess them in order to understand human problem solving (e.g., D. Barnes-Holmes, Hayes, & Dymond, 2001). Indeed, from the very beginning of RFT we have worked on methods for doing so, including some methods such as talk-aloud procedures that Palmer now recommends (Hayes, 1986). Moreover, RFT research itself has explored the correlation between private and overt relational responding (Cabello et al., in press). But there is a big difference between all of this and Palmer's appeal to *Alice in Wonderland* or to his own thoughts said aloud to explain overt performances. RFT provides a technically adequate account of such mediational behavior. Palmer does not.

From a behavior analytic point of view, it is dangerous, distracting, and unhelpful to use common-sense descriptions of verbal activity to ''explain'' human behavior. Other critics of RFT have fallen into a similar trap by suggesting that relational frames may be explained in terms of rules, logic, or mathematics (e.g., Burgos, 2003; Salzinger, 2003) without then providing a technical and empirically based account of these very behaviors (see Hayes et al., 2003, for a detailed discussion). In Palmer's case, he does not attempt a technical account beyond suggesting that one option might be to accept that ''backward conditioning, unreliable in the animal laboratory, is robust in humans'' (p. 196). Palmer does not note the inconsistency in his own thinking that this idea reveals: If Skinner's account applies to both nonhuman and human cases, as Palmer admits, then why suppose that backward conditioning is robust only in human beings? Furthermore, as we have shown elsewhere, backward conditioning at best could only explain stimulus equiv-

alence (D. Barnes-Holmes, Hayes, & Roche, 2001). Indeed, the logic needed to explain multiple stimulus relations, and transformations of stimulus functions based on them, is so overwhelming that to this point even adherents of Palmer's general approach have openly admitted that an experimental analysis in these terms is impossible (Tonneau, 2001, p. 123). Palmer seems to agree since he describes his own approach as "simpler conceptually, however intractable it may be experimentally." In other words, it is easy to say but hard to test. But it appears simple only because it is based on common-sense examples that appeal to an intuitive sense of understanding in verbal humans, without the inconvenience of an experimental analysis. This is precisely the lethal combination that behavior analysis has been faced with in this area for 50 years.

Contemporary behavior analysts should be sobered by the intellectual history of mechanistic behavioral accounts of covert mediation. During much of the twentieth century, some of the best and brightest psychological scientists tried and failed to turn associative mediation into a robust psychology of language and cognition. Watson's behaviorist manifesto (1913) explicitly described as "tenable" the idea that higher cognition is due to sequences of verbal responses integrated by what he called "associative mechanisms" (1913, footnote 7), and Sechenov proposed somewhat similar ideas 50 years before that (1863/1965). Watson also developed a talk-aloud procedure to study thinking in these terms, and described a verbal protocol while trying to determine the use of an unfamiliar object that looks very much like Palmer's description above: " 'It must be something to use in washing or weighing the baby—but they have no baby (*cul de sac* again). The thing is curved at one end so that it would just fit a person's neck. Ah! I have it! The curve does fit the neck. The woman you say is a hairdresser and the pan goes against the neck and the hair is spread out over it.' This was the correct conclusion. Upon reaching it there was a smile, a sigh and an immediate turn to something else (the equivalent of obtaining food after search)" (1920, p. 92). Associative mediation was at the very core of stimulus-response learning theory accounts of language and cognition, and chaining and

stimulus equivalence was appealed to as the source of such mediation (e.g., Palermo, 1966; Shapiro & Palermo, 1968). Just a few years later, some of these very same researchers had abandoned a behavioral approach to such mechanisms and had openly embraced a cognitive paradigm instead (e.g., Palermo, 1971; Weimer & Palermo, 1974). What has changed that would now lead such an approach to a more successful conclusion? Palmer does not say, and we see no reason to be optimistic.

It seems most likely that covert associative mediation is being brought up more for philosophical than for empirical reasons. Whatever else one wishes to say about them, it seems clear that there are two quite different philosophies of science within behavior analysis—one mechanistic, associationistic, and molecular, and one contextualistic, functional, and molar (Hayes, Hayes, & Reese, 1988). RFT is housed in the latter camp, as indeed we have argued was true of Skinner himself (Hayes et al., 1988). It is perhaps understandable that those who criticize RFT from a more mechanistic standpoint (e.g., Burgos, 2003; Malott, 2003; Palmer, 2004) have tried their hand at analyses that better comport with their philosophical and metatheoretical assumptions, and covert associative mediation seems to be the best idea available. If behavior analysis is to be a coherent discipline despite its philosophical differences, however, it seems only fair to be equally demanding about the strength of the technical accounts and empirical evidence emerging from both wings. To date, we are unaware of anyone who has attempted to explain more than a small part of RFT data using an associative mediational account. We await a formal attempt and the data said to be supportive of it.

What Behavior Analysts "Must" Do

It is a bit ironic that Palmer claims to be "an ardent fan of Skinner's *Verbal Behavior*" (p. 190), while challenging traditional behavior analytic ideas in so many fundamental areas. In his ardor, Palmer goes so far as to state outright that relational operants "*must* be interwoven with the verbal phenomena identified by Skinner in 1957" (p. 203). We have previously stated that "there is much of value in Skinner's account, once this key flaw [his

definition of verbal behavior] is corrected'' (Hayes, Blackledge, et al., 2001, p. 15), and we have provided examples of how to do that (D. Barnes-Holmes, Barnes-Holmes, & Cullinan, 2000), but a sentence of this kind is a bit surprising in a scientific document. If he means that relational operants must be interwoven with all of the data relevant to the domain covered by the account, we agree, but we await reference to conflicts between the data that exist and RFT. If he means that all basic behavioral principles belong in an integrated behavioral account, we again agree, but Skinner's categories of verbal behavior hardly rise to that level—especially since he himself described them as an exercise in interpretation. If the word ''must'' must be used, we suggest a better place for it: in order to assume its rightful role within psychology, behavior analysis *must* develop a robust and progressive empirical program on human language and cognition. RFT researchers are busy providing a way to do exactly that.

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