A Reply to Galizio's "The Abstracted Operant: A Review of *Relational Frame Theory: A Post-Skinnerian Account of Human Language and Cognition*"

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The publication of *Relational Frame Theory: A Post-Skinnerian Account of Human Language and Cognition* (Hayes, Barnes-Holmes, & Roche, 2001) marked the culmination of two decades of conceptual and empirical research conducted across two continents. After such an extended effort by numerous behavioral researchers, we would have been extremely disappointed had the contents of the book been widely misunderstood and misinterpreted within its home community. This seems less likely in light of Galizio's (2003) erudite and perspicacious review. His review indicates an appreciation of the substance, and we believe importance, of the relational frame theory (RFT) research agenda. Furthermore, he has picked up on many of the important features of the theory that others have often missed.

In his review, Galizio explains succinctly why the term *post-Skinnerian* appears in the title of the book, but is careful to point out that the account remains genuinely behavior-analytic (see also D. Barnes-Holmes, Barnes-Holmes, & Cullinan, 2000). He pro-

vides an excellent summary of the core of RFT (found in chap. 2 and 3), and acknowledges its scope in dealing with such topics as multiple stimulus relations, relational networks, relating relations, pragmatic verbal analysis, rule governance, and perspective taking (chap. 3 to 7). These critical features of the theory are often ignored by others, who tend to approach RFT as a theory of stimulus equivalence rather than an account of human language and cognition (Hayes, Barnes-Holmes, & Roche, in press).

Towards the end of Galizio's review (under the heading *Problems, Puzzles, and Directions for Future Research*), he raises a number of important and interesting issues. In some cases, we find ourselves in broad agreement with his comments, but in others we think it is necessary to clarify our position. In part because his review is so well done, and thus may be relied on as a general orientation to RFT, we are concerned that failure to do so could reasonably be taken to indicate that we are completely comfortable with all points raised.

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Functional Versus "Concrete" Operants

In the book we point out that "the RFT approach invokes a purely functional concept of an operant" (Hayes et al., 2001, p. 146) rather than one that relies on topography. We used terms

like *overarching* operants, *purely functional* operants, or *generalized* operants but we were also careful to say that "No new type of operant is supposed by these terms—the qualifiers are merely to avoid confusion" (p. 147). Galizio ponders this idea and asks, "What is selected when such an operant is reinforced?" (p. 159). There is nothing wrong with this question per se, but we fear that it may be based on a growing idea within behavior analysis that a topographical response definition is necessary, desirable, or somehow more scientifically sound than descriptions of functional relations.

In the book, we went to some lengths to show that the radically functional definition of operant classes found in RFT is entirely consistent with traditional behavior-analytic thinking on this issue (pp. 22–29, 146–150). The concept of a response class defined by its functional properties rather than its topographies has been at the conceptual core of behavior analysis from the very beginning (e.g., see Skinner, 1938, pp. 33–41). As Galizio points out, the RFT position is that if orderly functional relations are observed within a particular response definition, then that definition is successful (this too is a key feature of the behavior-analytic tradition, e.g., Skinner, 1938, chap. 1). If operant classes are functionally defined analytic units, there should be no basis, theoretical or empirical, to distinguish between functional and topographical operants, because all operants, by definition, are functional units (see D. Barnes-Holmes & Barnes-Holmes, 2000, for a detailed discussion; see also Catania, 1998, pp. 119–120).

Thus, although we agree with Galizio that additional research is needed on purely functional operant classes and we hope that RFT will continue to contribute towards that important work, we remain uncomfortable with the idea that a functionally defined operant is somehow less explanatory than a topographically defined operant. In our view, the temptation to make a technical distinction between purely functional operants and what Galizio describes as their "more concrete relatives" may reveal the unwarranted emergence of implicit nonfunctional or structuralistic thinking within the behavior-analytic tradition. Given the history of behavior analysis and the success of the purely functional approach, in our view it is the *topographical* definition of the operant that needs to be further developed, clearly articulated, subjected to empirical analysis, and then compared, in terms of meeting the scientific objectives of behavior analysis, with the more traditional functional approach.

Presumably, such a research program would start with a description of a set of response forms, articulated in precise spatiotemporal dimensions (e.g., left paw presses at a 45° angle to the manipulandum), which could be shown to be functionally related to a specific consequence. It would be absolutely essential that only the precisely prescribed response forms, and no others, produce the consequence; otherwise, the formal definition would break down. Furthermore, if the same form produced different effects, it would have to be considered to be the "same response" despite the different outcomes; otherwise, it would once again collapse into a functional, not a topographical, definition. As yet, no one in the behavior-analytic community seems willing to flesh out this approach to the definition and study of operant behavior. Until such an attempt is made and its scientific value is assessed, it seems unhealthy, behavior-analytically speaking, to treat purely functional operant approaches as if they are somehow less solid than topographical approaches. Given our history as a discipline, we would argue that the exact opposite is true.

The Origin of Relational Frames

In speculating about the possible histories involved in generating the

generalized operant of the frame of co-ordination, Galizio states,

Initial development of such frames must occur very early in childhood and would involve the establishment of generalized symmetry and transitivity by multiple exemplar training, presumably in a common context, to produce the linkage between the forms of mutual and combinatorial entailment required for the frame of co-ordination. RFT predicts that only after these types of training would frames of coordination emerge. (p. 166)

Although this quotation nicely captures the emphasis that RFT places on the role of reinforcement contingencies in establishing contextually controlled derived relations, RFT has always adopted the position that some limited repertoire of derived relational responding may be present prior to selection by operant contingencies. In the words of Hayes and Wilson (1993): "Some types of relational responding may be unlearned, but it seems very likely that most of it is learned, much as generalized imitation is an operant but is based on some degree of unlearned imitation" (p. 286; see also Barnes & Roche, 1996, p. 501).

We have not focused on the issue of unlearned relational responding in our research activity or writings, and thus our colleagues often miss this subtle feature of RFT (e.g., Tonneau, 2001, pp. 120–121; cf. D. Barnes-Holmes & Hayes, 2002). From our perspective, however, this issue seems relatively unimportant in terms of dealing with the vast and extended behavioral histories that are involved in generating the richness and complexity of human language and cognition. The search for "unlearned" forms of derived relational responding has been underway for some time now by experts in that field (e.g., Zentall & Urcuioli, 1993), but it seems only indirectly connected to the stated analytic goals of RFT, namely, the prediction and influence of human language and cognition.

What we have focused on is the developmental and historical nature of relational frames. That has been so from the earliest days of RFT research (e.g.,

Devany, Hayes, & Nelson, 1986; Lipkens, Hayes, & Hayes, 1993) and it is becoming more explicit and direct over time (Y. Barnes-Holmes, Barnes-Holmes, Roche, & Smeets, 2001a, 2001b). As Galizio correctly points out, there is currently little research available on the histories necessary to produce certain features of the relational operants described by RFT, such as the transformations of function through relational frames other than frames of coordination. These data are coming (e.g., see Hayes et al, 2001, pp. 185–187), but the research is inherently difficult and much more is needed. We hope that other human operant laboratories will also direct their attention towards this important area of research. For this to happen, however, more behavior-analytic laboratories need to become directly involved in the basic human research that RFT-type questions entail. That point brings us, however, to another matter that was raised in the review.

Dismissal of Nonhuman Research

Galizio is concerned about our "dismissal of the value of nonhuman research" (p. 168). "Dismissal" does not capture what we are saying about nonhuman research and the nature of the challenge human language and cognition presents to our field as currently organized. In the book, we bemoan the "tremendous experimental inconvenience" (p. 49) that the processes involved in relational framing are shown so readily humans, even infants, but with great difficulty or not at all with nonhumans. RFT is not a modern tabula rasa theory of human language and cognition. To the contrary, RFT carries forward the hard-won knowledge that our discipline has developed—we recognize that the "continuity with the rest of the animal kingdom means that [humans] . . . live in a world of direct contingencies" (Hayes et al., 2001, p. 49), and the importance of evolutionary history is embraced (e.g., Hayes et al., in press; Hayes,

Strosahl, & Wilson, 1999). Thus, everything we know from animal learning applies to human beings. What we challenge is an overattachment to the idea that animal learning necessarily provides all of the principles needed for the analysis of complex human behavior. This was never the behavior-analytic position (e.g., Skinner, 1938, p. 442), and we believe that overattachment to this assumption has caused a distortion in the distribution of basic research effort within behavior analysis and an unnecessary delay in rising to the experimental challenge of human language and cognition.

Nonhuman research may well have direct value for RFT. The study by Schusterman and Kastak (1993), for example, suggests that multiple-exemplar training may have generated derived equivalence relations in a nonhuman very much in line with the kinds of operant claims made by RFT. Nevertheless, such nonhuman studies will constitute only one small part of the research agenda, not the core or most important piece as has typically been the case within the basic science of behavior analysis (Hayes et al., in press). Relational operants dominate in the behavior of human beings. After over 25 years of serious effort by animal laboratories, it seems obvious that the same cannot be said of nonhumans. As we say in the book, "That is a difficult thought for behavioral psychologists interested in the analysis of human behavior, and yet, it seems to be the case" (p. 49). To us, that is not a dismissal of the value of nonhuman research but a call to face facts and to get on with the important work that lies in front of us, the analysis of human language and cognition, despite the inconvenient reality that much of this basic work will have to be done with human beings.

Transformation of Respondent Functions

In discussing arousal and the transformation of respondent functions, Galizio argues that "respondents, by definition, are not members of operant classes, so it appears that a history of exemplar training would not be sufficient to establish generalized transfer or transformation of such behaviors" (p. 167). It is worth noting in passing that the distinction between respondent and operant behaviors is not without ambiguity (see Coleman, 1981), with some researchers arguing that respondent and operant classes constitute different behavioral outcomes from the same behavioral process (e.g., Donahoe, Crowley, Millard, & Stickney, 1982; Palmer & Donahoe, 1993). But even if we accept, unconditionally, that respondent and operant behaviors are completely independent processes, RFT still predicts the transformation of respondent functions.

RFT has always defined entailment as a limited form of transformation of function (Barnes, 1994, p. 99; Hayes, 1992, p. 111; Hayes et al., 2001, p. 150). In the typical equivalence experiment, for example, in appropriate contexts (what RFT refers to as C_{rel} control) the function of the sample as a conditional discriminative stimulus is transformed into the function of a comparison stimulus or there would be no equivalence class when symmetry and transitivity are tested. It follows that as various patterns of entailment are being established across multiple exemplars, limited forms of transformation of function are also being trained, by definition. Critically, however, once events are related, additional stimulus functions that were not involved in that relational response may now, in appropriate contexts (what RFT refers to as C_{func} control), be transformed in accordance with the established entailment processes. Entailment thus defines the minimal unit upon which more and more elaborate transformation of function can be based (Hayes, 1992, p. 111; Hayes et al., 2001, p. 150). Although there may be differences in ease of establishment, magnitude of response, or degree of contextual control, the source or nature of the functions that are

transformed is not important to the existence of the contextually controlled transformation of stimulus function process. Functions could be unlearned, perceptual, operant, respondent, visual, gustatory, emotive, discriminative, reinforcing, and so on. The contextually controlled transformation of respondent functions thus does not require that those functions become members of operant classes during multiple-exemplar training, as Galizio suggests.

Furthermore, the typical procedures of respondent conditioning may generate processes beyond those of respondent conditioning, provided that the organism has an appropriate history of arbitrarily applicable relational responding. Imagine that such an organism was exposed to a respondent conditioning procedure in which an arbitrary stimulus, A, reliably preceded the delivery of shock. If A acquires increased autonomic arousal functions, we cannot yet conclude that this was a clear case of the process of respondent conditioning, because it is possible that the organism's prior history of relational responding allowed certain features of the procedure to function as contextual cues for relational responding between the A stimulus and shock (see Leader, Barnes, & Smeets, 1996, for a detailed discussion of this issue). For example, the regular and sequential pairing of A with shock could function as a contextual cue for a temporal frame of before–after between these two stimulus events (i.e., A before shock/shock after A; see O'Hora, Barnes-Holmes, Roche, & Smeets, in press, for a study involving before–after relations). If this relational response was generated by the respondent conditioning procedure and it was the source in part of the arousal shown to A, then that arousal was produced not solely by respondent conditioning per se but also by relational framing activity. Consequently, any subsequent transfer of functions from A to other stimuli (e.g., from A to B to C via a relational frame of coordination) would not require the transfer of a "pure" re-

spondent function (e.g., from A to C), but the transfer of an arousal function that was established, at least in part, through the before–after frame between A and shock. We suspect that many so-called instances of respondent conditioning with verbally able humans actually involve relational framing of this kind (cf. Lovibond & Shanks, 2002).

It is this kind of realization that leads us to emphasize with some urgency the need for basic behavior analysts to deal more directly and experimentally with the issue of human language and cognition. The principles and procedures used to explain relational operants in RFT are identical to those used to explain any operant: contacted consistencies in contingencies across multiple exemplars. RFT thus appeals to no new principles at all to explain the existence of what we take to be the core of human language and cognition, and it should be a simple empirical matter to decide whether or not such operants exist. But this behavior-analytically comfortable aspect of RFT quickly disappears when the implications of the operant described by RFT are examined: "Relational frames involve a new type of generalized operant. We say new, because the instrumental behavior of relational framing alters the functions of behavioral processes. We know of no term for such an effect" (Hayes et al., 2001, p. 45).

The transformation of function enabled by arbitrarily applicable relational operants seems capable of affecting virtually every other known behavioral principle (as in the respondent example just given). This realization explains why we agree with Galizio that the transformation of function is a fundamentally important target for future research within the discipline. It seems possible that everything the basic science of behavior analysis has established, largely through the study of nonhuman behavior, will have to be reexamined and possibly reworked in light of this empirical phenomenon.

This is indeed a daunting task, but if the basic premise of RFT is correct, it is a task we cannot afford to ignore.

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