Who Can Understand Relational Frame Theory?
A Reply to Barnes-Holmes and Hayes

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Before replying to Barnes-Holmes and S. C. Hayes' questions (this issue), some background may help the readers understand the nature of the current debate. Among the commentaries on *Equivalence relations: A critical analysis* (Tonneau, 2001) that were published in this journal, some defended relational frame theory (RFT) as a promising alternative to a correlation-based approach to symbolic behavior. Barnes-Holmes, S. C. Hayes, and Roche (2001), in particular, defended RFT and criticized the approach I proposed on two main issues.

A first issue concerned possible difficulties with symmetry in matching to sample and with A-B, A-C function transfer when the training trials are mixed. After suggesting that a Pavlovian process could not operate in the latter conditions "unless one wishes to invoke the problematic concept of backward associative conditioning," Barnes-Holmes, S. C. Hayes, and Roche (2001, p. 37) concluded that I had "completely failed" to address the emergence of bidirectional behavioral phenomena. A second challenge raised by Barnes-Holmes, S. C. Hayes, and Roche (2001) involved experimental results such as those of Barnes, Hegarty, and Smeets (1997) and Dymond and Barnes (1995).

Unfortunately for these arguments, explanations of matching symmetry in terms of forward pairings have been available since 1992 (L. J. Hayes, 1992: cited in my target article on p. 19; more on this below), function transfer after mixed A-B, A-C trials does not require backward con-

ditioning (e.g., Hall, 1996), and the presumed weakness of the latter is a matter of dispute (e.g., Savastano & Miller, 1998). Importantly, from the very perspective of RFT, if function transfer were impossible through backward pairings then no amount of operant reinforcement could make it grow (Tonneau, 2001, p. 121). Finally, in my reply to commentators (pp. 121-123) I sketched a correlation-based account of the main results of Barnes, Hegarty, and Smeets (1997) and Dymond and Barnes (1995).

As far as I can judge, Barnes-Holmes and S. C. Hayes (this issue) do not question that this kind of account could work; instead they turn to broader conceptual and theoretical problems concerning associationism, RFT, and behavior analysis. Barnes-Holmes and S. C. Hayes also object to my depiction of RFT as confused (Tonneau, 2001, pp. 118-120). Thus, the nature of the debate has shifted significantly.

Tactics of Scientific Research

As an example of strategic difference between RFT and the stimulus-correlation framework I proposed, Barnes-Holmes and S. C. Hayes suggest that the potential scope of the former (namely, "human language and cognition") is broader. I think the reverse is true. Because a correlation-based account aims at explaining results that are usually discussed in terms of memory retrieval (Tonneau, 1990), its potential scope with respect to both human and nonhuman behavior is much broader than that of RFT (Tonneau, 2001, p. 124). The phenomena that relational frame theorists study are complex examples of the ef-

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fect of environmental networks on behavior. The very complexity of these examples, however, make them ill-fitted to serve as a basis for a comprehensive theory of behavior. If RFT cannot even explain basic memory phenomena in Pavlovian conditioning (see Tonneau, 1990), for instance, how can RFT hope to explain complex human behavior any better?

Admittedly, the approach I have proposed is at an incipient stage of development (Tonneau, 2001, p. 26), whereas RFT has already generated numerous experiments; no one could seriously deny the heuristic value of the package of intuitions and procedures known as RFT. Having a list (even a lengthy one) of intuitions and procedures, however, is not the same as having a theory. The analogy with Darwin is particularly revealing here, since his goals were obviously *not* the "pragmatic" ones that Barnes-Holmes and colleagues like to present as "the" goals of behavior analysis; prediction and influence are appropriate aims for animal breeding, but not for biology. As to Barnes-Holmes and S. C. Hayes' claim that Darwin presented "a very limited data set" in favor of his theory of evolution, I can only ask the readers to have a look at *The origin of species* (Darwin, 1859) and judge for themselves.

Another point that needs correcting is Barnes-Holmes and S. C. Hayes' suggestion that I recommend "computational models" in addition to (or in replacement of?) data and experiments. As I pointed out (Tonneau, 2001, p. 123, Footnote 11), I do *not* support the development of computational models. What I do support is the computer modeling of *behavioral* theories (e.g., Epstein, 1985; Mazur, 1982), for the same well-known reasons that computer modeling is used in physics, chemistry, or evolutionary biology. In short, I recommend the development of *computable* models of *behavioral* processes to evaluate theoretical predictions in situations that involve multiple causation and highly complex histories. And, of course, I nowhere suggest that computer simulations should replace experiments.

The Obscurity of RFT

Barnes-Holmes and S. C. Hayes note that I find RFT vague and obscure. My charge of obscurity (Tonneau, 2001, pp. 118-120) obviously concerned the *concepts* used in RFT, not the *experiments* devised by relational frame researchers. I mention this point because Barnes-Holmes and S. C. Hayes seem to believe that experimental precision guarantees theoretical clarity; they state for example that "RFT is far from vague when it comes to suggesting what a researcher might do to establish specific relational performances." But that RFT is far from vague in suggesting particular procedures has no implication for the theoretical coherence of RFT. Equivalence-class research, for example, involves perfectly clear procedures associated with confused theoretical discourse. The procedures (or more exactly, their descriptions) are far from vague; yet the theory seems confused and illogical.

The same holds of RFT, since, among other things, relational frame theorists appeal to "relations" that don't exist and "behaviors" that don't exist either, such as the relational "behavior" or "responding" of less-than. As I pointed out (Tonneau, 2001, p. 118), the only way to make sense of this sort of discourse is to interpret it on an as-if basis; in the study by Dymond and Barnes (1995), for instance, some subjects behaved *as if* a syllable were larger or more than another. But these as-if descriptions cannot serve as an account of behavior, since an account of behavior should appeal to actual variables as opposed to *as-ifs*.

When we turn to the unpacking of these *as-ifs* in terms of actual variables, RFT proves equally obscure. The notion of a history (or is it a responding?) being "brought to bear" on something else is particularly difficult to reconstruct (see Tonneau, 2001). Barnes-Holmes and S. C. Hayes now claim that the notion of "bringing to bear" a history was only a turn of phrase and that the technical details can be found in a recent book on RFT (S. C. Hayes, Barnes-Holmes, & Roche, 2001). After reading the book in question from cover to cover, however, I have been unable to discern the promised details. As far as I can see, the book largely repeats the turns of phrases of RFT without clarifying them. The notion that contextual cues "bring to bear" a history merely in the sense of being discriminative for relational responding doesn't help, since in

experiments such as those of Dymond and Barnes (1995) this relational "responding" can't be defined at all. Likening "relational responding" to generalized imitation and other response classes (see S. C. Hayes, Barnes-Holmes, & Roche, 2001, p. 23) doesn't help either, since in these examples behavior is *not* defined through as-if clauses. In generalized imitation for instance, subjects do not behave *as if* their behavior were similar to that of the model; rather, their behavior *is* similar to that of the model. Thus the defining criteria of the response class *actually* hold, in contrast to those of the "behaviors" (e.g., "less") that relational frame theorists routinely invoke.

In sum, the principal concepts of RFT seem obscure, if not incoherent, and Barnes-Holmes and S. C. Hayes' commentary provides no clarification. Importantly, the computer simulations of Barnes-Holmes and colleagues (e.g., Barnes & Hampson, 1993) do nothing to clarify the concepts used in RFT, since these simulations do *not* rely on such concepts (e.g., the concept of a "relational responding" of less, or the concept of a history being "brought to bear" on something else), but instead provide straightforward cognitive explanations of RFT data in terms of internal associations and representations.

Associations and Reinforcement

Barnes-Holmes and S. C. Hayes allude to associationism in their commentary, but different views of associationism coexist in psychology and should be distinguished from one another. Moreover, the position of RFT on associationism is difficult to understand, since after presenting the associationist model of Barnes and Hampson (1993) as a precise account of RFT data, Barnes-Holmes and S. C. Hayes suggest that associationist accounts of RFT data have difficulties that RFT hasn't.

Traditionally, associations have been said to exist (a) in the mind, (b) in the brain, and (c) in the environment. The mediational theorizing of Barnes and Hampson (1993) is an example of associationism in senses (a) and (b). Consistent with a behavioristic stance (e.g., Skinner, 1977, p. 1), however, position (c) is the one I have advocated. I have argued that function transfer arises

from stimulus correlations and independently of the process of operant reinforcement (as shown by the fact that function transfer can be demonstrated in purely Pavlovian settings).

Importantly, a Pavlovian conception of function transfer does not imply that symbolic *performance* can be understood independently of operant responses and their reinforcers. Far from it. Since (Pavlovian) stimulus correlations are supposed to transfer not only traditional respondent behaviors, but also the responses that are maintained by operant reinforcement, a complete account of symbolic performance must clearly include the latter (see Tonneau, 2001, p. 124). If, for example, a response R_1 is reinforced conditionally on a stimulus A, so that R_1 comes to predominate in the presence of this stimulus, then it is R_1 (and not, say, R_2 or R_3) that will transfer to the B stimuli correlated with A. Hence operant reinforcement determines *what* is being transferred through the A-B correlation; but it is the A-B correlation, not operant reinforcement, that explains why function transfer takes place at all. Of course, if responses and reinforcers have themselves stimulus functions, they can support function transfer by association with other stimuli.

To this notion of function transfer as being driven by stimulus correlations, I have added an important, but highly speculative, hypothesis to deal with the *extinction problem* in relation to symbolic behavior (see Tonneau, 2001, p. 25). To explain why the function-transfer effects of correlations between words and objects seem to persist (at least in daily life) even when the original word-object correlations are degraded, I have proposed that operant reinforcement might protect function transfer from extinction (Tonneau, 2001, p. 25). This speculation raises complex theoretical issues that are beyond the scope of the present reply. If the hypothesis turns out to be correct, however, it still grants no role to operant reinforcement in *producing* function transfer; the hypothesized role of operant reinforcement is to *maintain* function transfer once it occurs, not to produce it out of nothing.

In general, I do not see how operant reinforcement could produce a phenomenon like function transfer; here as elsewhere, reinforcement as such can only influence the rate of phenom-

ena that occur for non-operant reasons (Tonneau, 2001, p. 121). This point of logic about reinforcement underscores the necessity of specifying principles of behavioral induction to complement principles of reinforcement and punishment. Contrary to Barnes-Holmes and S. C. Hayes's allusion to ultimate and proximate causes, induction principles deal with what produces function transfer in the history of particular organisms, not with the evolutionary origins of function transfer in previous generations. I have argued that as far as function transfer is concerned, the relevant principles are Pavlovian.

Associations and RFT

According to Barnes-Holmes and S. C. Hayes, RFT already includes a significant Pavlovian component. They state for example that "respondent processes are important for relational frame theory" and that RFT "has always explicitly incorporated respondent processes into its explanatory framework."

Note here that Barnes-Holmes and S. C. Hayes refer to RFT as a *theory* and as an *explanatory framework*, as opposed to a series of experiments. (I take it for granted that a theory is not an experiment. The readers will certainly understand the difference between, say, the *theory* of cognitive dissonance and the *experiments* it has generated.) Notice also that Barnes-Holmes and S. C. Hayes speak of incorporating respondent *processes*, and not merely respondent *behaviors*, in RFT. Now one would surely expect respondent processes to be relevant to respondent behaviors and vice versa; however, relational frame theorists have a tradition of experimenting with stimulus pairings, and sometimes even respondent behaviors, without incorporating respondent processes into RFT.

The studies of stimulus pairing that Barnes-Holmes and S. C. Hayes mention, for example, emphasize empirical issues and sometimes barely mention RFT (e.g., Leader & Barnes-Holmes, 2001). When an explanation of the findings is attempted, the operative processes are said to be "largely *operant*" (Leader, Barnes-Holmes, Smeets, 2000, p. 76) and to involve "equivalence relations" or histories that were "brought to bear by various contextual cues" (Leader, Barnes, & Smeets,

1996, p. 703), consistent with the suggestion of Barnes-Holmes, S. C. Hayes, and Roche (2001) that the effects of stimulus pairings in such studies may *depend* upon "the generalized operant process of relational framing" (p. 37). Roche and Barnes (1997) similarly attribute their findings on respondent behavior not to respondent mechanisms but to RFT processes (p. 289) that are presumably operant in nature (cf. Barnes-Holmes, S. C. Hayes, & Roche, 2001, p. 35). I do not see how Barnes-Holmes and S. C. Hayes can conclude on the basis of such reports that RFT (the *theory*) incorporates respondent processes into its explanatory framework.

When we turn to the theoretical statements made by relational frame researchers, understanding the place of respondent processes in RFT becomes even more challenging. The earliest material that Barnes-Holmes and S. C. Hayes mention is a book chapter by S. C. Hayes and L. J. Hayes (1989). Far from incorporating respondent processes into RFT, these authors *deny* that respondent processes could explain RFT data. After contrasting Pavlovian conditioning with bidirectional behavioral phenomena (p. 162, p. 165), S. C. Hayes and L. J. Hayes state that "upon further consideration of equivalence findings, the plausibility of a respondent conditioning explanation, *even* for the finding of symmetry, *cannot be sustained*" (1989, p. 166, emphasis mine). S. C. Hayes and L. J. Hayes (1989) later discuss the transfer of perceptual functions in terms of a "relational class" (p. 171).

Barnes-Holmes and S. C. Hayes also quote an article of Barnes and Roche (1996) to show that respondent behavior is "an important foundation for relational framing," whereas Barnes-Holmes, S. C. Hayes, and Roche (2001, p. 37) had previously mentioned the same article to suggest that respondent processes "do not play a primary explanatory role in RFT (Barnes & Roche, 1996)." How an important foundation for relational framing could fail to play a primary explanatory role in RFT is unclear to me, but in any case, Barnes and Roche (1996, p. 501) did comment in a footnote on the importance of respondent behavior, referring the reader to the theoretical work of Barnes (1994).

In this work, Barnes (1994) explains matching symmetry (among other findings) through mediational processes that involve the Pavlovian conditioning of covert perceptual behaviors. Aside from the RFT notion that the presence of respondent behaviors and other elements must work "as a contextual cue for the relational frame of sameness" (p. 109), the explanation is similar to that of L. J. Hayes (1992) which Barnes mentions (1994, p. 108) and that I did cite in the article portrayed by Barnes-Holmes, S. C. Hayes, and Roche (2001, p. 37) as having completely failed to address the issue of matching symmetry.

Importantly, the Pavlovian part of Barnes' (1994) account does not "facilitate" the emergence of matching symmetry (as Barnes-Holmes and S. C. Hayes misleadingly say), but is a *sine qua non* for its occurrence: Remove the Pavlovian component, and matching symmetry would instantly collapse. Barnes-Holmes and S. C. Hayes nonetheless write that RFT as a theory will not stand or fall on the veracity of Barnes's (1994) account, in spite of the fact that no alternative *account* of matching symmetry is available in RFT.

Meanwhile, after finding all sorts of "difficulties" with a correlation-based account of matching symmetry and related phenomena, and remarking that "given these difficulties, it is perhaps not hard to understand why Tonneau refused to address the issue of symmetry in his article" (p. 37), Barnes-Holmes, S. C. Hayes, and Roche (2001) introduce RFT as an operant *alternative* to a respondent account (p. 37). Paradoxically, we are now informed that RFT does *not* dispense with bidirectional, correlation-based processes and that I am simply wrong to imply that it does (Barnes-Holmes and S. C. Hayes, this issue).

Some relational frame theorists, however, certainly believe that RFT dispenses with bidirectional, correlation-based processes, because they claim that RFT can operate in their absence. S. C. Hayes, Fox, Gifford, Wilson, Barnes-Holmes, and Healy (2001), for example, explicitly state that B-C function transfer through AB, AC pairings "cannot be due to classical conditioning because it would require an appeal to backward conditioning" (p. 46), the alternative being RFT. Again, I do not understand how operant reinforcement

could make function transfer grow if it did not occur through stimulus pairings, but in any event, these authors' claims about AB, AC effects in classical conditioning are false (e.g., Ward-Robinson & Hall, 1996).

So what is the role of correlation-based processes in RFT? Six possibilities may be discerned: (1) Respondent behaviors are an important foundation for relational framing, but they do not play a primary explanatory role in RFT. (2) Respondent processes are important for RFT, but it doesn't matter whether they are present or not. (3) RFT does not dispense with bidirectional, correlation-based processes, but RFT is an *alternative* to a correlation-based account. (4) RFT does dispense with respondent processes, which cannot deal with matching symmetry and other findings. (5) Respondent processes *can* deal with findings such as matching symmetry, but RFT does not depend on such speculations. (6) RFT does involve respondent processes, and relational frame theorists understand it from time to time, but otherwise keep claiming that respondent processes can't explain RFT data.

Transfer, Transformation, and History

One of Barnes-Holmes and S. C. Hayes' central points is that a correlation-based account of RFT data is similar to RFT. One could of course put it the other way round, and suggest that it is RFT that is similar to an associationist explanation (although the status of the "relations" invoked in RFT seems obscure from a behaviorist standpoint). All detailed explanations of results such as those of Dymond and Barnes (1995) will obviously need to take into account the correlational structure of the environment, and thus exhibit a family resemblance at a formal level.

Barnes-Holmes and S. C. Hayes suggest, however, that our accounts of the data of Barnes, Hegarty, and Smeets (1997) might differ. I have not been able to understand all of the details of the argument, which appeals to RFT notions that I find unintelligible (such as the relational response SAME AS). To reiterate what should be obvious, SAME AS is not a "relational response" but an environmental relation, and this relation does *not* hold of the compounds present in the test

phase of Barnes, Hegarty and Smeets' (1997) study. From a molar perspective, SAME AS *is* nevertheless present in the subject's environmental path (since the latter includes non-contiguous stimulus pairs such as C1 and C1, for example), and, pending potentiation by other variables, the presence of SAME AS in the path can influence performance in testing. What the subjects will do in testing depends on what operant responses were reinforced conditionally on SAME AS and contextual variables such as the matching-to-sample format itself (in which identity matching has previously been reinforced, I suppose). Notice that the behavior reinforced conditionally on SAME AS, and that reappears in testing, is the behavior of *matching* and not SAME AS.

In contrast to the data of Barnes, Hegarty, and Smeets (1997), my account of the examples of *function transformation* reported by relational frame theorists highlighted an important difference between a correlation-based approach and RFT (Tonneau, 2001, pp. 122-123). The issue of function transformation, far from being semantic, is theoretically crucial; for if processes of function transformation actually existed, they would seriously challenge any stimulus-correlation account of complex behavior. Unfortunately for RFT, however, I have argued that processes of function transformation do *not* exist and that the apparent cases of function transformation reported by relational frame researchers can be explained as function transfer (Tonneau, 2001, pp. 121-123).

I am not sure that Barnes-Holmes and S. C. Hayes fully understand my argument. Thus they discuss a hypothetical case of function "transformation" in which a stimulus A is established as the "opposite" of B (in RFT parlance), and in which A is avoided whereas B isn't. According to Barnes-Holmes and S. C. Hayes, speaking of function transfer from A to B would be inaccurate. I certainly agree that speaking of function transfer from A to B would be inaccurate. However, I suggest that speaking of function transformation from A to B would *also* be inaccurate. The hypothesis I propose is that the situation discussed by Barnes-Holmes and S. C. Hayes actually involves function transfer—*not* from A to B, of course, but from *C* to B, where C is a *third*,

extraneous stimulus whose behavioral effects, *different* from those of A, transfer to B.

Such extraneous C stimuli are often hidden in the subject's pre-experimental past, which makes the actual source of function transfer difficult to identify. If the approach I propose is correct, these stimuli nevertheless exist and play a crucial role in the putative cases of function "transformation" that relational frame theorists report. I have previously explained, in one case chosen for illustrative purposes, how this sort of account could apply to the behavior observed in Dymond and Barnes' (1995) study—namely, choosing C2 after two bar presses (Tonneau, 2001, p. 123). Admittedly, my explanation is highly tentative, but I do not see how Barnes-Holmes and S. C. Hayes can write that I avoid to provide appropriate descriptions of this sort of behavior.

Barnes-Holmes and S. C. Hayes also claim that they "find nothing objectionable in the idea that a training history transferred to current stimuli." But this notion is theirs and not mine. Clearly it is relational frame theorists who believe that *histories* transfer (or are "applied," "brought to bear," etc.) to current stimuli. From the perspective I propose, of course, it is behavioral functions that transfer, not histories (the RFT notion of a history transferring to something seems incoherent to me).

Finally, my remark on the lack of historical specification in RFT (Tonneau, 2001, p. 123) concerned the all-too-easy RFT accounts of data such as those of Barnes, Hegarty and Smeets (1997) and Dymond and Barnes (1995) in terms of the subjects' abilities to "relate relations" or "transform" functions in accordance with relations that don't even hold. How Barnes-Holmes and S. C. Hayes can maintain that my explanations (Tonneau, 2001, pp. 121-123) *lack* "real detail" as compared to the turns of phrases of RFT, is a puzzle.

As to Barnes-Holmes and S. C. Hayes' suggestion that the reinforcement of identity matching is an important component of RFT, the only supporting statements that I could find in the RFT literature (aside from Barnes' 1994 comments on the "relational frame of sameness") are along these lines: "It is necessary to recognize an event as itself in order to respond to that event in terms of derived relations" (S. C. Hayes, 1991, p. 32).

If these considerations on event recognition are the RFT way of saying that the reinforcement of identity matching is involved in results such as those of Barnes, Hegarty, and Smeets (1997), I admit to having misunderstood the message.

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