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DATA IN SEARCH OF A PRINCIPLE: A REVIEW OF RELATIONAL FRAME THEORY: A POST-SKINNERIAN ACCOUNT OF HUMAN LANGUAGE AND COGNITION

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Responding to derived relations among stimuli and events is the subject of an accelerating research program that represents one of the major behavior analytic approaches to complex behavior. *Relational Frame Theory: A Post-Skinnerian Account of Human Language and Cognition* (Hayes, Barnes-Holmes, & Roche, 2001) offers a conceptual framework for this work and explores its implications for verbal behavior and a variety of other domains of complex human behavior. The authors dismiss Skinner's interpretation of verbal behavior as unproductive and conceptually flawed and suggest a new definition and a new paradigm for the investigation of verbal phenomena. I found the empirical phenomena important but the conceptual discussion incomplete. A new principle of behavior is promised, but critical features of this principle are not offered. In the absence of an explicit principle, the theory itself is difficult to evaluate. Counterexamples suggest a role for mediating behavior, perhaps covert, thus raising the question whether a new principle is needed at all. The performance of subjects in relational frame experiments may be a mosaic of elementary behavioral units, some of which are verbal. If so, verbal behavior underlies relational behavior; it is not defined by it. I defend Skinner's definition of verbal behavior and argue that an account of relational behavior must be integrated with Skinner's analysis; it will not replace it.

Key words: equivalence classes, definition of verbal behavior, private events, relational frame theory, relational frames, verbal behavior

Relational frame theory made its debut in 1985 in a paper presented by Steven Hayes and Aaron Brownstein at a meeting of the Association for Behavior Analysis and has fostered considerable empirical work, conceptual discussion, and controversy ever since. It has emerged as one of several major threads in the analysis of complex human behavior within behavior analysis, and the pace of activity has continually accelerated. Steve Hayes, at the University of Nevada in Reno, is the principal architect of the theory, and Dermot Barnes-Holmes and Bryan Roche at the National University of Ireland in Maynooth are its most active researchers and exponents, but their work has been supported by dozens of colleagues and students. *Relational Frame Theory: A Post-Skinnerian Account of Human Language and Cognition* (Hayes, Barnes-Holmes, & Roche, 2001) presents an overview of the theory, discusses its role in language, development, and cognition, and shows how it might be extended to education, therapy, social processes, and even religion. The scope of the book is ambitious, its tone confident and enthusiastic. That it is so vigorously advanced by such prominent and active behavior analysts requires that, whatever our biases, we weigh it carefully.

The 13 chapters in the book are separate papers, written by shifting subsets of the 19 authors. These subsets overlap considerably, however, with either Barnes-Holmes, Hayes, or both, contributing to every chapter, and it is evident that the chapters are intended not to represent separate theses but to be smoothly integrated into a unified position. Therefore, I will simply refer to the contributors as ''the authors,'' without distinction, as though each endorsed the whole. I will use the abbreviation *RFT* to refer to the book; the italics differentiate it from RFT, which is commonly used in the literature as an abbreviation for the theory itself. To avoid confusion, I write out the term relational frame theory whenever I have occasion to mention it, except when it appears in quotations.

The purpose of the book is to provide a conceptual consolidation of work that has been evolving and expanding for over a decade. It is written for a broad audience, both within and beyond behavior analysis; consequently, the authors use technical terms sparingly, and the experimental literature is reviewed only lightly. Nevertheless, the book is controversial, for it suggests that we add a new analytic tool to the workshop of the behavior analyst.

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Small changes in one's conceptual tools can have far-reaching effects. Unfortunately, the immediate effects are likely to be disruptive: Imagine trying to replace one brick in a wall with a slightly larger brick. You can't simply rearrange the neighboring bricks; if you move one brick aside you must move the whole row. Perhaps most of the wall must come down and be rebuilt if you are to replace that brick. Similarly, *RFT* suggests that we replace some of our current concepts in the domain of verbal behavior with new and different ones, but such terms cannot simply be squeezed into the hole left by discarded terms. Our entire conceptual edifice may need to be reconfigured. For this reason, new concepts in science are generally resisted, particularly by those satisfied with the status quo. Relational frame theory has already faced plenty of opposition, and whatever its merits, it is likely to continue to do so, for it requires that we reexamine and perhaps change some familiar fundamental concepts and analytic tools. The authors do not shrink from this task; rather, they embrace it with revolutionary zeal.

Consequently *RFT* is partly expository, partly polemical. The authors open the book with a vigorous attempt to persuade the reader that the current conceptual tools of the behavior analyst are inadequate to an understanding of complex behavior. In particular, those concepts developed by Skinner in *Verbal Behavior* (1957) are described as insufficient and sterile, an inevitable consequence of his erroneous definition of verbal behavior itself. They offer an alternative definition arising from relational frame theory that they assert is both adequate and productive. However, they are emphatic that their new proposal lies squarely within the scope of behavior analysis. That is, they do not suggest that one must fly to a more permissive paradigm but that relational frame theory is a natural development within our own field. This opening chapter, which I will discuss again at the end of this review, touches on some of the most important concepts in our field and will be of very general interest, whether one is persuaded by the authors or not.

The next six chapters discuss the theory and its implications for our understanding of language and cognition. Chapter 2 introduces technical terms and outlines the theory. Chapter 3 discusses what I regard as one of the most important problems in our field, the transformation of stimulus function. Successive chapters extend the analysis to analogical reasoning, thinking, problem solving, understanding, rule following, and finally the concept of self. Chapter 8 offers a summary and overview of the first seven chapters and stands on its own. It is wisely inserted here to permit the reader to rehearse and review the main points of the preceding chapters. The final five chapters discuss possible applications and extensions of the theory. Topics include development, education, social processes, psychopathology, therapy, and finally religion and spirituality. The book, then, will appeal in different ways to different audiences. Those with any interest in the conceptual foundations of behavior analysis will want to read the first three chapters, whatever they think about relational frame theory, for there is plenty of grist here. As I read the book in preparation for this review, I underlined statements that I wanted to consider more carefully. At the end of two chapters I found that I had underlined nearly every passage; moreover, I found myself turning to various other sources to review foundational concepts. Those who wish only to find out what relational frame theory is all about will want to read chapters 2 through 8, and can even get by with reading chapter 8 alone. Those with strictly applied interests will be drawn to the last five chapters. In short, even if one is skeptical of the authors' thesis, this book is likely to find a place in one's behavioral library.

My repeated allusions to skepticism may have already alerted the reader to my own reaction to the book. I was not persuaded by the authors' conceptual analysis, and I will close this review by discussing some of my objections. But I should reveal my biases. I am an ardent fan of Skinner's *Verbal Behavior.* My own speculations on the topic, however modest, are dear to me, and they are straightforward extensions of Skinner's position. I believe that his analysis is sound and serves as an excellent foundation for subsequent work; it is not a sacred text but a remarkable first approximation to an operant analysis of verbal behavior. Despite my disagreement on this score, however, I regard relational frame theory with equanimity. The proponents of

the theory have been extraordinarily active and are addressing some of the most formidable questions in our field. Moreover, the empirical work is important, however one chooses to interpret it. Science is a selectionist enterprise, and variability is fundamental to progress. The most effective and elegant interpretation will eventually prevail.

A Matter of Style?

My purpose is not simply to review the book but also to translate it. Despite the authors' best effort, it is difficult to understand everything they say—at least, I found it so—and I think it will be useful to describe my own interpretation of the theory, provided that the reader recognizes that it is the interpretation of a critic, not that of a proponent. To the extent that relational frame theory is an empirical enterprise, it is clear enough. But the interpretation of the data and the theoretical edifice built upon the data are by no means clear.

The difficulty in understanding the book does not arise from a deficiency in style. The writing is always competent and sometimes excellent, despite a few lapses. The grammarian will bristle at ''like'' being used as a conjunction, and the fastidious stylist will object to the repetitive use of distinctive expressions, but these are exceptions. Somewhat more troublesome is the tone of the book. The evident enthusiasm of the authors is at first refreshing, but the breathless tone soon cloys. For example, in a crescendo of images, virtually in successive paragraphs, we are warned that patterns of verbal relations can be *highly complex,* that they are *unmanageably complex,* that they can be *amazingly complex, shockingly complex, incredibly complex,* and finally *unbelievably complex* (pp. 55–62, passim). But however inelegant such overheated rhetoric may be, it is not unclear.

Rather, the confusion in the book arises, at least in part, from the authors wanting to have it both ways: Relational frame theory is either Skinnerian or post-Skinnerian. Either it is an unexceptionable extension of current operant theory, worthy of neither messianic fervor nor great alarm, or it is revolutionary and should be viewed with suspicion. Before we take a revolutionary text seriously, we must scrutinize it minutely for conceptual adequacy; we should not discard our old mule for a glossy thoroughbred with weak legs. But the authors want to arouse our excitement without simultaneously arousing our suspicious scrutiny, and that can't be done.

The preface trumpets the dawning of a new day, and chapter 1 asserts that it is time to relegate *Verbal Behavior* to the shelf of historical curiosities. But subsequently the authors argue that the mainspring of relational frame theory is simply another generalized operant and should not, as it were, be kept waiting at the door until we have satisfied ourselves that it is harmless. If so, one wonders if this is indeed a new principle or simply an important topic. Although a new principle is promised, none is explicitly described, and the reader is left unsure just what is being claimed. However, I will put aside these matters until I have discussed uncontroversial topics. I will begin by describing one example of a relational frame experiment and its interpretation, for it is through such an example that the conceptual discussion acquires substance.

A Typical Relational Frame Experiment

Relational frame experiments commonly have several phases. In the first phase, previously learned discriminations, that is, discriminations already in the repertoire of the experimental subjects, are brought under the control of arbitrary stimuli, such as nonsense syllables, in a ''relating-to-sample'' procedure. For example, choosing a line longer than a sample line would be reinforced in the presence of SAB, whereas choosing a shorter line than the sample would be reinforced in the presence of LUZ. At the end of Phase 1, SAB would control ''picking a longer element'' and LUZ ''picking a shorter element.'' Speaking nontechnically, one might say that the purpose of Phase 1 is to give a relation-specifying meaning to the nonsense syllables. By training with a variety of examples, the specificity of control of the conditional discriminative stimulus can be made increasingly abstract. That is, the relation ''longer than'' can be disentangled from confounded properties of the exemplars such as shape, color, modality, and so on. The relation becomes the most reliable predictor of reinforcement and presumably eventually blocks control by other stimulus properties or dimensions.

In the parlance of *RFT,* the relation between the short and long line is said to be ''non-arbitrary.'' That is, the relation is not a social convention; one line really is longer than another. (Nothing of what follows depends on the relation being nonarbitrary; any preexisting discrimination will serve. The importance of the distinction between arbitrary and nonarbitrary relations will emerge later.)

In Phase 2, discriminations between novel stimuli are acquired under control of SAB and LUZ. The novel stimuli might be other nonsense syllables. For example, in the presence of SAB, choosing CUG is reinforced when BEH is the sample. Speaking loosely, we would say that, thanks to the presence of SAB, subjects have learned that ''CUG is longer than BEH.'' In the parlance of *RFT,* the BEH-CUG relation is said to be ''arbitrary.'' That is, one would never guess by looking at the stimuli that one was longer than the other; the relation is established only by the contingencies within the experiment and is thus analogous to a social convention. To cite an example from the text, a dime is less than a nickel in a nonarbitrary physical sense but is more than a nickel in the arbitrary world of coin values. The arbitrary relation is conditional, presumably, on experiences like those of Phase 2 training.

Phase 3 tests for derived relations among stimuli. If A has been established as longer than B, and B longer than C, then A must be longer than C. Behavior controlled by this relation is said to be ''derived,'' because it has not been specifically trained. The nature of the derived relation will depend on the nonarbitrary relations in Phase 1. If A is the opposite of B, and B is the opposite of C, then A must be the same as C. If A is the same as B, and B is different from C, then A must be different from C. If A is the same as B, and B is the same as C, then A must be the same as C. If A evokes two lever presses, and B is more than A, B should evoke more than two lever presses (a transformation of stimulus function).

The typical finding of such experiments is that subjects indeed show such derived relations and transformed stimulus functions. These empirical findings lie at the heart of the book, for they require explanation. Relational frame theory is the authors' attempt to provide one.

Types of Relational Frames

The kinship to the equivalence class paradigm will be obvious to many readers. To a first approximation, relational frame theory is the extension of the equivalence class paradigm to all other kinds of relations. A sample of the relations discussed in *RFT* includes the following ''families'':

1. *Coordination.* Coordination is said to be the most fundamental type of relation (p. 35). Coordination embraces relations of sameness, identity, and similarity, and therefore includes equivalence relations, ''the simplest form of relational response.'' Similarity is said to be more complicated than equivalence:

> Suppose a child is shown a cup and told ''this is similar to a bowl.'' Depending upon what the child already knows, more contextual cues may be needed to relate the term and the object reliably. ''Is similar to'' requires a dimension along which two events are similar. That dimension might be purely verbal (e.g., ''loathing is similar to hate'') or it may be abstracted features of the environment (e.g., ''a cup is similar to a bowl because it can hold liquid''). (p. 35)

2. *Opposition.* Opposition is said to be more complicated than coordination in that classes of coordinated events can be in opposition to one another. In that case, if A is the opposite of B, and B of C, then we might conclude that A and C are coordinated.

3. *Distinction.* This is a nonspecific relation common to any discrimination. We learn that A is not B, but we learn nothing of the nature of the difference.

4. *Comparison.* Comparative terms, such as *bigger, faster, weaker, less dense, shorter,* and so on, apply when events can be arranged along some qualitative or quantitative dimension. The number of such relations is indefinite and can be vague or specific. ''For example, 'A is twice as fast as B and B is twice as fast as C' allows a precise specification of the relation between all three elements of the network'' (p. 36).

5. *Hierarchical relations.* '' 'A is an attribute or member of B' is the general form of a hierarchical frame'' (p. 37). *John is a man,* is one example; *Bananas are fruit* another. Derived relations do not follow a single pattern but depend on the nature of the hierarchy.

If A is the father of B and also the father of C, then B and C are siblings.

6. *Temporal and spatial relations.* Events occur in time and space and are related in systematic ways. ''For example, if you are told that house A faces the back of house B, you could order the front and back doors of both houses into a linear sequence (back door of A, front door of A, back door of B, front door of B). This is because front and back doors are relative to each individual house, and knowing the orientation of the two houses implies the more detailed information'' (p. 38).

7. *Deictic relations.* Deictic relations are determined by the perspective of the subject, for example, left–right, here–there, and now–then. ''These properties appear to be abstracted through learning to talk about one's own perspective in relation to other perspectives. . . For example, if Peter is asked, 'What did you do when you got there?' he should not simply describe what someone else is doing now'' (p. 39).

Relational frame theory is even extended to embrace numerical relations and logic. For example, consider the following number series:

1, 7, 13, 19, 25. . .

This series is said to exemplify the relation ''plus six,'' which we then apply to get the answer (p. 162). As for logic, it does not explain relational framing; rather, it is the other way around (p. 191).

Because relational frame theory is an expanded paradigm, concepts appropriate to equivalence classes require modification. ''Symmetry'' becomes ''mutual entailment''; if A is longer than B, B must be shorter than A (not longer than A, as symmetry would suggest). The relation is constrained, but it is not symmetrical. ''Transitivity'' becomes ''combinatorial entailment''; if A is the opposite of B, and B is the opposite of C, then A is plausibly the same as (not the opposite of) C. ''Transfer'' of stimulus function becomes ''transformation'' of stimulus function; if A is more than B, and B evokes one response on a key, then A will perhaps evoke more than one response on a key (e.g., Dymond & Barnes, 1995). The point is that if A is related to B, then B must be related to A, but not necessarily in the same way. If C is related to B, then C may also be related to A, but again, the relation may be complex. Thus equivalence can be seen as a special case of a more general phenomenon.

What a Relational Frame is

In the example above, we distinguished nonarbitrary relations from arbitrary relations. The latter are of special interest, for they arise only from the practices of verbal communities. One line might be longer than another line irrespective of such a community, but CUG is longer than BEH only in the convention established in the small verbal community of a particular experiment. The term ''relational frame'' tacts a class of such relational responses.

> A relational frame is a specific class of arbitrarily applicable relational responding that shows the contextually controlled qualities of mutual entailment, combinatorial mutual entailment, and transformation of stimulus functions; is due to a history of relational responding relevant to the contextual cues involved; and is not solely based on direct, non-relational training with regard to the particular stimuli of interest, nor solely to nonarbitrary characteristics of either the stimuli or the relation between them. (p. 33)

Thus the term *relational frame* is not analogous to the term *equivalence class*; whereas an equivalence class is the term for a set of stimuli that control behavior in characteristic ways, a relational frame, according to this passage, is the term for the various behaviors that are controlled by equivalence and other relations. The frame is not a set of related stimuli or a set of relational contingencies; it is a set of responses that relate classes of stimuli. This distinction is fundamental and creates a wide conceptual divide between relational frame theory and equivalence class theory, as developed by Sidman (1994, 2000).

Taking Stock

So far, so good; there is little to argue about. The authors and their colleagues have shown that their subjects do behave in conformity with this account. This is a set of empirical findings, not a theoretical claim. Moreover, it is a plausible inference that, given training with other kinds of relations between stimuli, even those that have not yet been thoroughly investigated, subjects outside the

laboratory will tact untrained relations in systematic ways without explicit training. This is an important phenomenon. Much of what we deem ''complex'' in human behavior reflects our highly adaptive sensitivity to, and rapid acquisition of, relational stimulus control. In fact, sensitivity to relations is the central dependent variable in aptitude tests. A plausible supposition is that sensitivity to relations accounts for much of the variance among people, as well as between humans and other species, in what we loosely call ''intelligence.'' The authors are surely justified in their enthusiasm for the topic, and they deserve credit and support for beginning to bring this important topic into order.

A New Definition of Verbal Behavior

Recognizing the centrality of relations in cognitive tasks, and having rejected Skinner's definition of verbal behavior, the authors offer an alternative definition, one rooted in derived stimulus relations. Specifically, ''*Verbal behavior is the action of framing events relationally,*'' and ''*verbal stimuli are stimuli that have their effects because they participate in relational frames*'' (pp. 43–44). Thus directly trained discriminations of nonarbitrary relations would not count as verbal behavior, even if the relevant responses were words or strings of words; such responses are analogous to lever presses under control of a tone and do not reflect a special process (Barnes-Holmes, Barnes-Holmes, & Cullinan, 2000). Although this definition may not conform to our prejudices, there can be no objection to it. Anyone may stipulate a definition of a term for purposes of discussion. As I understand it, the motivation for the definition is this: What is special about language (however defined) is that it permits abstraction, symbol manipulation, and effective action in the absence of direct experience; this is the domain of derived stimulus relations. Other dimensions of language (however defined) are common to behavior such as lever pressing and do not require a special account. Let us therefore define the domain in a way that captures its special nature.

Because the definition excludes behavior that is explicitly shaped, it diverges widely from everyday usage, and it is likely to lead to misunderstanding. Nevertheless, it is the authors' prerogative to define the term as they wish. Once we define verbal behavior as relational framing, however, it is a small step to assume that relational frames *explain* verbal behavior. Definitions are cheap, but explanations are dear, and we must be careful not to confuse them.

Conceptual Trouble: Formulating a New Principle of Behavior

It is neither the empirical observations nor their importance that is debatable; rather it is the conceptual framework of *RFT* that is controversial. (See Burgos, 2003, and Tonneau, 2001b, pp. 117–120 for further critical discussion of this framework.) The authors hold that relational frames are not mere descriptions of behavior, but are processes:

> 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. (pp. 33–34)

I don't understand this passage. Is a relational frame a class of behavior, as I thought I had just established, 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. This may be no more than a bit of careless prose, but if so, it is an exasperating burden on the reader, for it is the distinction between outcome and process that lies at the heart of the authors' bold claim that a new day has dawned. The reader wants to know how we are to *explain* the emergence of derived stimulus relations. Loving economy, we should like to do so by appealing to existing behavioral principles, but if such principles are indeed inadequate, then we should like to know what new principle we must add to our toolbox in order to account for these derived relations. Until this question is answered we do not have a new theory, just a new puzzle.

The research on relational frames shows that when subjects are given certain kinds of experience with novel stimuli, a variety of new environment-behavior relations emerge. In order for these facts to be useful, we must be able to extract a principle, an inductive generalization that will enable us to help account for the variability of future behavior. Not only must we account for future instances of relational behavior, we must account for those anomalies and exceptions in which relational behavior fails to emerge. Sidman (2000) has proposed just such a principle for the formation of equivalence classes; namely, that reinforcement procedures establish the three terms of a contingency as members of an equivalence class at the moment of reinforcement. Whatever the fate of this hypothesis, it has the virtue of being an explicit statement of a principle that predicts when we should expect to see equivalence and when we should not.

Unfortunately, the quoted passage is as close as we get to a statement of principle for relational frame theory. Stripped to essentials, the passage includes three propositions (p. 34):

1. ''The process is the history that gives rise to a relational operant.''
2. ''The process involved is contingencies of reinforcement.''
3. ''Relational responding is not [an] effect of such contingencies; it is the target of them.''

This is muddled. The third statement contradicts the other two. But it appears to have been added as an afterthought, to distinguish the authors' claim from that of Sidman (2000), so I think we can disregard it. As for the second statement, a contingency isn't a process. Contingencies are undoubtedly relevant, but we'd like to know how.

We are left with the claim that history gives rise to relational behavior. Specifically, ''it seems that relating as an overarching class could be formed in a way somewhat similar to that of generalized imitation—through exposure to multiple exemplars across a variety of situational contexts that refine the nature of the response and sources of stimulus control over it'' (p. 25). But we cannot explain one poorly understood phenomenon by appealing to another. Generalized imitation is an important outcome, but although we can sometimes produce it, we understand why it emerges only dimly; it is certainly not a principle. Moreover, the concept of overarching response classes is vague and troublesome. A central feature of response classes is that the members are mutually replaceable, but relational responses are not mutually replaceable. There may be a thematic unity to relational responses, but to call them response classes is not uncontroversial.

Nevertheless, whether or not relational responses are usefully considered a response class, the claim being advanced is that they are acquired through multiple exemplar training. But what counts as an ''exemplar''? In relational frame research, exemplars are embedded in matching-to-sample procedures, but such highly structured procedures are artificial and are surely quite rare in the experience of most people. Presumably, then, the critical features of the relevant exemplars are to be found in commonplace events such as ostensive learning (e.g. Stemmer, 1996), learning names (e.g. Horne & Lowe, 1996), or even eavesdropping on the conversations of others. When a child hears a parent say, ''The rain is really coming down,'' or ''I love Beethoven's Ninth!'' there is an implicit equivalence between the rain and the tact ''rain,'' and between a musical passage and the tact ''Beethoven's Ninth.'' Anecdotal evidence suggests that commands like ''Don't touch the mongoose!'' are sufficient to teach children what a mongoose is in a single trial, provided that only one strange animal is present. If other animals are present, other cues, such as the direction of gaze of the parent's eyes, may be required as well. Notice that these anecdotes provide plausibility at the expense of specificity. They do not describe events analogous to matching-to-sample experiments. In fact, they make no reference at all to the behavior of the child. Surely some behavior of the child is relevant, but what is it? Must the child be ''actively listening'' to Beethoven's Ninth, or is it sufficient that he passively hear it? How are we to distinguish these two cases? Evidently it is not enough to refer to a history of multiple exemplar training, for that expression embraces a wealth of mysteries. Before we can assert that we have a new principle, much more detail is required about the variability of the history and its re-

lation to variability in behavior. In the case of relational frames, what are the critical features of the relevant history, and precisely what behavioral effects follow? The authors are cheerfully agnostic about this question: ''In general it seems more conservative and scientifically responsible to work out these details empirically rather than to allow interpretation and speculation to get too far ahead of the data'' (p. 28).

This is an honorable suggestion, but it removes the last pillar of the principle we have been seeking. It appears, then, that there is no new principle on offer, and there is no basis for saying that a relational frame is both an outcome and a process concept. It is an outcome. We are ignorant of the process.

There is nothing shameful about this. Our understanding of most complex human behavior is quite tentative. Until we understand the role of history, however, it remains a plausible hypothesis that relational behavior is a set of heterogeneous phenomena that emerge from the interaction of elementary operants according to established basic behavioral processes. In this respect, relational behavior might be like mathematical behavior. Following exposure to typical educational contingencies, one can respond effectively to an unlimited number of novel, complicated problems, both abstract and practical. If we are told, ''306 times 22 equals 6732,'' we can reply, ''6732 divided by 22 is 306,'' ''21 times 306, plus 306, equals 6732,'' ''307 times 22 is 6754,'' and so on. If we are told, ''X is a 20-digit prime number,'' and ''Y is a 50-digit prime number,'' we can respond, ''X plus Y is not a prime number.'' These examples share some of the properties of relational frames, but they do not appear to have a unitary explanation. We do not interpret them as behavioral units, nor do we postulate a special behavioral process to explain them. Rather, we interpret them as products of episodes of problem solving behavior—procedures and strategies possibly quite complex—that are appropriate to the particular example. Our educational practices aspire to teach students some of these very procedures and strategies, but they are not merely part of the subject's history; they are components of the behavioral episode of problem solving.

However, this humdrum conclusion that relational behavior is a set of heterogeneous phenomena emerging from the interaction of elementary operants is clearly at odds with the aspirations of the authors. A major heading in chapter 2 announces that ''verbal events involve a new behavioral principle'' (p. 45). Their argument is straightforward: In derived stimulus relations, new stimulus functions appear in the absence of the training that customarily produces such functions. For example, if A equals B, and B equals C, then C will serve as a conditional stimulus and A as a discriminative stimulus in a matching-to-sample procedure in which C is the sample and A one of several sample stimuli. The observed history of the subject does not include conditions under which we would predict such stimulus functions to emerge. As Barnes-Holmes, Hayes, and Roche (2001) argue, in response to Tonneau (2001a), and as the authors note in *RFT* (p. 146), such results seem to require backward conditioning, and backward conditioning has been found to be only a weak laboratory phenomenon (cf. Sidman, 1994, p. 111). We may not know precisely what the new behavioral process is, but it appears that there must be one, and a candidate is at hand: ''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)'' (p. 46).

What is the Alternative?

The argument that we must admit a new behavioral process is an example of argument from incredulity: If we cannot imagine an alternative, then our claim must be true. This is the weakest form of support, for it depends on the scope of one's imagination. Note that, as it stands, the presumed process is simply an inference; we know nothing about it. Giving this hypothetical process a name reifies it and suggests more about the putative process than we know. Might it not be the case, for example, that backward conditioning, unreliable in the animal laboratory, is robust in humans? This proposal would require no more than a parametric footnote to our existing principles, but its effects would be felt in myriad ways, perhaps including the generation of many of the phenomena under discussion.

Another alternative, the one that I prefer,

is even simpler conceptually, however intractable it may be experimentally. It recognizes that a matching-to-sample experiment (to say nothing of a history of multiple exemplar training) is a complex event, only a small part of which is measured in a typical experiment. *RFT* regards the measured dependent variable—usually a press on a keyboard—as though it is the only relevant behavior, and it regards the various symbols as though they are the only controlling variables. In a typical relational frame experiment (e.g., Dymond & Barnes, 1995; Steele & Hayes, 1991), described schematically above, a contextual cue (e.g., SAB, meaning ''longer than'') is presented along with a sample (e.g., three stars) and several comparison stimuli (e.g., groups of one, three, and six stars). Pressing the key for six stars is scheduled for reinforcement. To a naive subject, this is a problem to be solved. He is not inert; he does not press a key at random and then subside into a torpor until the next scheduled event. He glances from one stimulus to another and back again, then furtively to his watch, then back to the sample, emitting discriminative responses at every saccade. He says to himself, ''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.'' And so on. The temporal ordering of events as prescribed by the experimental design is quickly rendered irrelevant as the subject glances back and forth between stimuli and recalls symbols from previous trials. The subject provides himself with a stream of supplementary stimuli, many of which are unobservable. He makes up names, rehearses relations, forms and tests hypotheses, and keeps running rules that control his behavior from trial to trial. The final performance is affected by this problem-solving behavior of the subject and cannot be understood without taking it into account.

Covert behavior cannot serve as data, of course, but there can be no question that it plays a role in many instances of complex behavior. (If you were to count backward from 107 by threes, what would be the fourth number in the series? What is the third state you enter as you travel due west from northern Minnesota?) A possible role for such behavior should be considered if one is driven to proposing a new principle because of an argument from incredulity. This alternative account must be considered whenever verbally sophisticated humans are used as subjects. Unfortunately, most of the research cited in *RFT* used teenagers or adults as subjects. Because the dependent variables measured in these experiments comprised only a small part of the behavior of the subjects, multiple interpretations of their behavior are possible.

The authors of *RFT* obliquely reject a role for mediating events; behavior other than the target behavior is regarded as simply a component of the relational operant:

> Relational frames are not mediated by more basic processes; instrumental learning is the process. Of course, any operant contains other operants, virtually without exception, and so too with relational frames (Barnes-Holmes & Barnes-Holmes, 2000). When a pigeon pecks a key for food it necessarily involves orienting toward the key; orienting involves moving the head and looking; looking involves tracking a visual stimulus with the eye; tracking involves focusing the lens of the eye; and so on ad infinitum or until we get tired or disinterested. Similarly, any operant can expand into other ''larger'' operants, ad infinitum. Such flexible units should be expected in any contextualistic approach (Hayes, Hayes, & Reese, 1988) because the pragmatic qualities of contextualistic thinking preclude foundationalism and other kinds of ontological assumptions. Operants are analytic units that analysts adopt for specific purposes—they are not things. (p. 34)

I disagree strenuously with this passage. That operants are flexible does not mean that they can be defined according to the whim of the experimenter, or that the unit of analysis is a matter of convenience. If we have no independent criteria for deciding units of analysis, all behavioral interpretation becomes an exercise in circular reasoning, and prediction becomes impossible. Unfortunately, to go further would be a digression that I cannot spare, and I refer the reader to Skinner (1935) for what is still the definitive discussion of this topic. Nevertheless, control of relational responding by other behavior, that is, covert behavior and unmeasured overt behavior, under the sway of basic processes, appears to be rejected. I do not understand why, for it appears to me to be self-evident that in many cases such control occurs. Whether it is

sufficient to explain all examples of relational behavior is a topic to be discussed; it cannot be dismissed.

The Relevance of Evidence from Special Subjects

That some researchers have found evidence of relational behavior in very young children (e.g. Lipkens, Hayes, & Hayes, 1993; Peláez, Gewirtz, Sanchez, & Mahabir, 2000), in mentally retarded subjects (e.g. Devany, Hayes, & Nelson, 1986; Sidman, 1971), and even in sea lions (Schusterman & Kastak, 1993) suggests that hypothesis testing and other forms of complex verbal strategies need not play a role in all relational behavior. However, these studies have demonstrated the emergence only of equivalence or of some dimension of equivalence. Relational frame theory cannot rest only on demonstrations of the formation of equivalence classes. Equivalence relations are special; at the least, they are simpler and should be distinguished from the more abstract relations of opposition, relative position, time, kinship, and other relations within the ambit of *RFT*. What follows for equivalence may not follow for relations like opposition, or ''plus 6.'' As examples increase in complexity, it becomes less plausible that the target behavior occurs in the absence of a pattern of supplementary behavior. I think it is unlikely, for example, that most people can solve number series problems without engaging in collateral behavior.

As I mentioned, equivalence relations are special. The concept of functionally equivalent stimuli has always been fundamental to behavior theory (cf. Skinner, 1935). Contemporary research on stimulus equivalence suggests a substantial refinement of our understanding of this concept and of the procedures that produce it (e.g., Sidman, 2000), but whatever the merits of that suggestion, some concept of functional equivalence will remain central to our understanding of behavior. All stimulus events in the natural environment are unique; organisms would be paralyzed if stimulus control were not generalized within groups of functionally equivalent stimuli. In contrast, relations such as opposition are artificial and are found not in nature, but in our models of nature. In what sense is a long stick the opposite of a short stick, or white the opposite of black?

As an aside, whether equivalence relations, as studied in matching-to-sample experiments, are also mediated by other behavior is still an open question, in my opinion. Mentally retarded subjects and very young children may not use sophisticated strategies, but their behavior in matching-to-sample experiments certainly includes behavior that is not measured by the experimenter. I am not postulating imaginary behavior. I am postulating that real behavior that goes unmeasured in the experimental session may play a role in performance and should be considered before we propose new principles. Is it credible that when faced with a matching-to-sample trial lasting at least several seconds, the subject's only behavior is to press a key? At a minimum, subjects must look at each stimulus; but each stimulus may evoke discriminated responses, and such responses need not be verbal. If the stimuli are familiar, they may evoke a mosaic of discriminated operants and autonomic responses. A picture of a dog may evoke slightly different emotional responses from those evoked by a picture of a cat, for example. Even if the stimuli are initially undiscriminated, stimulus control will emerge over the course of training (else the stimuli would remain undiscriminated). The web of controlling relations in such experiments might be quite complex.

Consider the behavior of the subject in Sidman's first experiment on equivalence (Sidman, 1971). He was a 17-year-old boy with severe mental retardation who was unlikely to have learned verbal strategies for mediating stimulus relation. He could say ''cat'' when shown a picture of a cat, and he could choose the picture of a cat from an array when the experimenter said ''cat,'' but the written word ''cat'' controlled no relevant behavior. Following training in which the sample was the spoken word ''cat'' and pointing to the written word ''cat'' was reinforced, the subject acquired, without further training, some new relations, among which was the following: When shown the written word ''cat,'' the subject would *say* ''cat.'' As a result of the experimental procedures, we might expect the auditory stimulus ''cat'' to be equivalent to the written word ''cat'' and a picture of a cat, but why should the *response* ''cat'' have any

strength at all? It played no part in the procedure.

Presumably, the training phase transferred discriminative control of the echoic response ''cat'' from the auditory stimulus ''cat'' to the written stimulus ''cat.'' But the subject apparently did not emit the echoic when the word was presented as an auditory stimulus during training. Why should derived stimulus control be stronger than the stimulus control from which it was derived? I suggest that if the subject said ''cat'' in response to the written word during testing, he also said ''cat'' to the auditory stimulus during the training phase (if not, why not?), but his response was either unrecorded or subvocal. By hypothesis, this response became an unobserved member of the equivalence class and underwent a change in strength as the result of the experimental procedures. Although this interpretation postulates behavior, it does not do so gratuitously; it postulates the very behavior whose control is said to have transferred. It is no more speculative than the alternative account, which appeals to a transfer of discriminative stimulus control when the original discriminative stimulus does not evoke the response. The point of this digression is that even the simplest procedures with human subjects are difficult to interpret unambiguously. In my opinion, this is a problem even with research on equivalence classes, which are fundamental and relatively simple; I think it is premature to propose new principles on the basis of research on more complicated relations.

Problems of Interpretation

One of the strengths of *RFT* is that it appears to help us explain the behavior of both the speaker and the listener. Suppose a child, meeting someone for the first time, hears ''This is Susan; Susan is your aunt. Why don't you show your aunt your pictures?'' These are ''frames of coordination,'' and the child behaves accordingly, treating ''Susan,'' ''aunt,'' and the actual person more or less interchangeably. This effect is one of the justifications for defining verbal behavior as the behavior of framing events relationally.

If we suppose that the authors are correct, that derived relational responding is the product of a special principle, unmediated by other events, then when given a relevant history of multiple exemplar training, subjects should behave appropriately, without the need for mediating behavior, as the example of Aunt Susan seems to suggest. But it is easy to invent other examples that contradict this prediction. Lewis Carroll (1871/1971) offers a paradigmatic case:

> ''Can you do Addition?'' the White Queen asked. ''What's one and one and one and one and one and one and one and one and one and one?''
> ''I don't know,'' said Alice. ''I lost count.''
> ''She can't do Addition,'' the Red Queen interrupted. (p. 194)

Of course Alice can do addition, but only if she is given breathing space to do some covert bookkeeping. Analogously, if I tell you that F is the opposite of B, B is the opposite of D, D is equivalent to R, X is equivalent to B, S is the opposite of R, and S is the opposite of L, can you tell me the relation between S and F? Yes, you can, but only by going back and ''working it out,'' that is, only by rehearsing, organizing the stimuli into groups, and testing yourself as you go. Derived relational responding does not simply appear because of the presence of a contextual cue; it is the product of an interactive program of mediating events. If your success or failure depends on these mediating events, we must include them in our account.

The plausibility of relational frame theory rests on the use of simple examples in which the role of mediating behavior escapes notice, but when more complicated examples are advanced, it becomes clear that novel relational behavior emerges with the support of supplementary behavior. For example, recall that ''if you are told that house A faces the back of house B, you could order the front and back doors of both houses into a linear sequence. . . because. . . knowing the orientation of the two houses implies the more detailed information'' (p. 38). But even though a statement can imply more detailed information about spatial arrangements, these implications may not control our behavior without ''working them out.'' Suppose a house is octagonal, with sides numbered sequentially. Which side is opposite side three? Several ways to answer the question leap to mind, but the answer itself does not. The very notion of a ''way to answer'' suggests mediation.

For every example in which relational frame theory seems to sweep away difficulties, there is another in which it seems to create them. The following statements have the same form as ''Susan is your aunt'' but have different effects:

Bill is Tom's friend.
Charlie is Tom's friend.

Your only data come from the text, but you don't conclude that Bill is Charlie. It is something outside of the relational frames that bars that conclusion.

Sarah loves Alec.
Alec loves Maya.

How does Sarah feel about Maya? My guess is that she hates her, but that does not emerge from the structure of the frames or the contextual cues. Rather, the conclusion is mediated by the private scenario I have visualized under guidance of the sentences. If you imagined Maya as Alec's infant daughter, you would draw a different inference.

I traveled a hundred miles due east.
Then I traveled a hundred miles due north.
What direction should I travel to get back to my origin?

Your answer to this question depends upon covert mediating behavior. If you visualize the problem on a plane, you will answer ''Southwest,'' but if you visualize the problem on a globe near either pole, almost any answer is possible, including ''Due south for a while and then due north.'' Your answer is not the result of an unmediated behavioral process.

The effects of verbal behavior on a listener are not automatic, as we would expect if relational frames were a basic behavioral process. Rather, the effects are often, loosely speaking, inferential. The listener is not an empty vessel into which we pour relational frames. One need not be a mentalist to recognize that the listener is a boiling cauldron of behavior—images, speech, emotions, actions—much of it private, and it is into this stew that verbal behavior is stirred. The effect on the listener emerges from the interaction of many variables. The appropriate behavior of a child toward her Aunt Susan arises from such interactions. Of course, one cannot be confident that these effects can be reduced to familiar elementary principles, for we see only glimpses of the relevant variables, but neither can we be confident that a new principle is required.

Summary

The authors present derived relational frame theory as both an empirical and a theoretical enterprise, both an outcome and a new behavioral process, a process by which, they propose, we should define verbal behavior. Much of the book is an introduction to the implications of this proposal. The book is vigorous, ambitious, and exciting, and should be read by everyone with an interest in the conceptual framework of behavior analysis. However, perhaps because I am wedded to a behaviorally conservative viewpoint, I have claimed in this review that although the empirical enterprise is commendable, the theoretical one is premature. Specifically, I have pointed out that a new behavioral principle is assumed, but in contrast to the thesis of Sidman (2000), none is explicitly described. The argument that one is required assumes that there is no alternative, but this assumption has no force; human behavior is so complex that alternative accounts cannot be easily evaluated. The polemical force of relational frame theory arises from its apparent ability to account for a wide variety of examples of novel, complex behavior, but the argument is too facile. It does not explain Alice's bewilderment or our failure to confuse Bill with Charlie. An adequate theory must account for the variance in behavior; that is, it must explain both the successes and the failures of relational behavior. As the White Queen problem illustrates, even the simplest of relational problems may require mediating behavior, and our interpretations must acknowledge that behavior. Relational frame theory makes no such acknowledgement; indeed it cannot do so, for once a role for mediating behavior is admitted, the need for a new theory evaporates. Relational behavior may not be an elementary phenomenon but rather the outcome of the interplay of many variables. In other words, relational behavior is operant behavior, but it is not ''an operant.'' Behavior in such cases is better captured by the everyday terms deduction, inference, and problem solving, that is, complex patterns of behavior that are rendered simple only if we restrict our viewpoint to some terminal index

of performance, such as a key press at a choice point.

Notice that nothing I have said shows that the authors are wrong, only that they have not made a good case that relational behavior is a homogeneous phenomenon unified by a new behavioral principle. Relational frame theory is not a theory at all; it is a prediction—a prediction about the direction in which we must look for such a theory.

Some of the objections I have raised to relational frame theory may be addressed as the range of dependent variables and experimental procedures is expanded. For example, Dickins and his colleagues have made suggestive use of functional magnetic resonance images of subjects engaging in relational tasks (Dickins et al., 2001). Dube and his colleagues are now recording their subjects' eye movements using an instrumental eye-tracking device (e.g. Dube et al., 1999). Although seldom reported, response latencies can easily be extracted from the data; latency could be an important variable in its own right or as an index of other behavior, including covert behavior. When adult subjects are used, overt verbalizations can be encouraged and analyzed (e.g. Ericsson & Simon, 1993). Such additional dependent measures will help to give a more complete picture of the behavior of the subjects in relational tasks and should account for some of the variance in the target response. To evaluate the role of mediating behavior, overt distractor tasks can be assigned to subjects between the presentation of stimuli and the recording of target responses; if such tasks disrupt performance, mediating behavior may be required in the target task. Alternatively, more work can be done with very young children and with animals. (What's special about sea lions?) Finally, experimental preparations with greater external validity can be adopted. Procedures that employ ostensive learning, modeling, or naming are better models of natural contingencies than matching-to-sample procedures.

I will close this review with a discussion of the authors' criticism of Skinner's definition of verbal behavior. I have deferred this topic because, in contrast to the authors themselves, I regard the matter as peripheral to relational frame theory, and I do not wish to confuse the evaluation of one book with a debate about another. Nevertheless, the theme that Skinner's definition of verbal behavior is inadequate occurs at high strength in the book, in other papers by the authors, and in presentations about relational frame theory at professional meetings. Some of the authors have begun to consider an integration of relational frame theory with Skinner's interpretation of verbal behavior (e.g., Barnes-Holmes et al., 2000; O'Hora & Barnes-Holmes, 2001), but this ecumenical spirit is not evident in the discussion of *Verbal Behavior* in *RFT*. I fear that silence will be taken for assent (but see Leigland, 1997).

On Skinner's Definition of Verbal Behavior

Skinner attempted ''to define a field of inquiry having certain unitary properties'' (1957, p. 224). He defined verbal behavior as behavior mediated by other people, specifically, by other people who are ''responding in ways which have been conditioned *precisely in order to reinforce the behavior of the speaker*'' (p. 225).

According to the authors of *RFT,* ''the problem is that [Skinner's] book did not lead to a progressive research program that raised a large set of new and important empirical questions about language. It did not lead to a rising cycle of research and analysis in the domain it addresses. If one believes that language and cognition are central issues in human psychology, this is disappointing'' (p. 11).[1] The blame, they argue, rests squarely with his definition of verbal behavior. Two faults are cited:

1. The definition is not functional. Verbal behavior is defined, not according to the history of the behaving organism, the speaker, but according to the behavior of another organism, the listener.

2. The definition is too broad. It embraces operant conditioning experiments with animals, for example, because reinforcement is

[1] What counts as a rising tide of research and application is a matter of opinion. John Eshleman has reviewed presentations at the annual meetings of the Association for Behavior Analysis since 1975 and found that of those conspicuously inspired by Skinner's book, the past 4 years have seen a three-fold increase relative to the 1990s, a five-fold increase relative to the 1980s, and an eight-fold increase relative to the 1970s, clearly an accelerating trend. Moreover, the application of Skinner's analysis to the shaping of verbal behavior in autistic children is now widespread.

mediated by the experimenter, and the experimenter's behavior has been shaped to reinforce the behavior of the subject. This is claimed to have discouraged verbal behavior researchers who had hoped to study something new but found themselves back in the animal laboratory studying stimulus control.

The centerpiece of the argument is a thought experiment—a kind of Turing test for rats. Imagine 2 rats in identical chambers, exposed to identical variable-ratio 5 schedules. For one chamber, the reinforcement schedule has been arranged by an experimenter and is mediated by electronic gadgets designed by the experimenter; the rat's behavior therefore meets Skinner's definition of verbal behavior. The other chamber happens to have been placed next to a sack of pellets, and by chance, the lever jiggles loose a pellet through a rip in the bag every five presses, on average. There is no difference in the rats' behavior. We can even switch the rats from one chamber to the other without relevant effects on their behavior. Skinner's definition is patently absurd, the argument goes, to distinguish these two cases. Any interpretation arising from such a flawed definition must be riddled with error.

Perhaps I am blinded by idolatry, but I find this critique, however clever and catchy, to be misguided. I suspect that the authors felt that the reader must be persuaded that Skinner is wrong in order to take *RFT* seriously, but this is a battle they do not need to fight, and it is a digression from their constructive proposals.

First, even if it were true that Skinner's definition is inadequate, nothing follows, because 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.

As for the breadth of Skinner's definition, the very point of his analysis is that verbal behavior is *not* different in kind from other behavior. It can be analyzed with the same tools as all other behavior. That his definition embraces the behavior of animals in operant experiments seems like an embarrassing blunder until we realize that he encompassed such behavior deliberately. What were Skinner's reasons for passing up intuitive, uncontroversial definitions of verbal behavior in favor of a baroque definition that nobody understands—one that embraces lever-pressing rats? The authors of *RFT* seem not to have asked this question.

Skinner's purpose was to define a field of inquiry that has certain unitary properties. What is it that is special about verbal behavior? According to Skinner, it is not a special type of behavior, nor does it obey qualitatively different rules. Rather, the special property of verbal behavior is its power, and it is powerful only insofar as it affects the conditioned behavior of other people in systematic ways. It is not a different type of behavior, but it has special characteristic effects, and it is these effects that define ''the domain of interest.'' 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. As with so many other things, he seems to have gotten it just right.

We can now make sense of the example of our lever-pressing rats. The thought experiment in *RFT* has explicitly assumed conditions in which the verbal rat exerts no more power over the world than the nonverbal rat. Under these conditions there is no domain of interest and no point in distinguishing verbal from nonverbal behavior. Nor would there be any point in making the distinction with humans if the vibrations of our vocal cords had direct effects on the physical environment comparable to the effects they exert on other people in our verbal community. If I can hum to a driverless taxicab and get whisked off to the airport, why should I put up with the gossip of a cabbie? But it is only in a farfetched example that verbal behavior has a substantial unmediated effect on the world.

The comparative advantage of the verbal rat over the nonverbal rat quickly becomes apparent if we are allowed to change the rules a little. 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.'' Because the ''meaning'' of a response is simply a verbal convention, this is easily arranged. Meanwhile, our nonverbal rat is still shaking individual pellets out of a bag behind his chamber. This example reveals the power of verbal behavior; instances of verbal behavior that differ but slightly can have dramatically different effects. As long as we are disposed to cater to our rat, its verbal behavior can be powerful indeed. It is quite true that there is nothing special about the behavior of the rat; what *is* special is the interlocking contingencies of the speaker and the listener.

I see no problem with Skinner's definition of verbal behavior, but I can understand why others might find fault with it. However, I am baffled by the importance the authors of *RFT* attach to the matter. Do they believe that an utterance cannot come under stimulus control of a property of objects (as in the tact), or of script (as in the textual operant), or of the formal properties of antecedent verbal behavior (as in the echoic), or of a state of deprivation or aversive stimulation (as in the mand), or of the cadence and sound of antecedent verbal behavior (as in the intraverbal), or of properties of an audience, or of variables affecting strength of response (as in the descriptive autoclitic), or of combinations of such variables? Surely not. *RFT* may be correct; perhaps a new principle of relational behavior really is required if we are to fully account for complex human behavior. But if so, it must share control of behavior with existing principles. The behavioral effects of such a principle must be interwoven with the verbal phenomena identified by Skinner in 1957. The evolution of our understanding of verbal behavior will be incremental and integrated, not desultory.

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