Why Relational Frame Theory Alters the Relationship between Basic and Applied Behavioral Psychology

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Abstract

The promise of a synthesized basic and applied psychology has been most strongly realized and appreciated in behavior analysis. The traditional model for this relationship has been largely unidirectional in that the experimental analysis of behavior has fed applied behavior analysis with its principles. Despite the relative strength of this relationship, which should lead to a more coherent and broadly effective discipline, behavior analysis seems to be narrowing its domain of influence both on the basic and applied fronts. We argue that this paradox can be explained by the inability to develop a broadly useful behavior analytic theory of language and cognition. Relational Frame Theory corrects that deficit, but it leads to a fundamental shift in the organization of the discipline, and the relationship between basic and applied behavior analysis. The current paper details what we view are the components of this new relationship and the reasons why this shift must occur.
Key Words: Basic and applied psychology, experimental and applied behavior analysis, language and cognition, RFT.

Resumen

La promesa de una síntesis entre psicología básica y aplicada ha sido principalmente valorada y llevada a cabo dentro del análisis de conducta. El modelo tradicional de esta relación entre ámbitos de investigación ha sido básicamente unidireccional, con el análisis experimental del comportamiento surtiendo de hallazgos y principios al análisis aplicado del comportamiento. A pesar de que esta relación es relativamente fuerte, lo que debería conducir a una disciplina más coherente y de más eficacia general, la influencia del análisis de conducta parece estar disminuyendo tanto en el frente básico como en el aplicado. Sostenemos en este trabajo que esta paradoja puede explicarse por la incapacidad para desarrollar una teoría analítica-conductual del lenguaje y la cognición que sea de utilidad. La Teoría del Marco Relacional viene a subsanar este déficit, pero lleva por fuerza a un cambio fundamental en la organización de la disciplina y en la relación entre los dominios básico y aplicado. Este artículo entra a detallar los que entendemos como componentes de esta nueva relación y las razones por las que creemos que el mencionado cambio en la disciplina debe tener lugar.
Palabras clave: Psicología básica y aplicada, análisis experimental y aplicado del comportamiento, lenguaje y cognición, RFT.

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Behavior analysis began as a basic field focused on the study of animal behavior but always aspired to extend its analysis to socially important human behavior: "The importance of a science of behavior derives largely from the possibility of an eventual extension to human affairs" (Skinner, 1938, p. 441). The avenue to complex behavior that was adopted was bold and not guaranteed to succeed. The general strategy of this approach involved attempting to find principles of behavior with high precision but also broad scope by examining the situated actions of non-human animals in relatively contrived environments. Nothing in evolutionary theory, or behavior analysis itself, demands that principles identified at the tip of one evolutionary branch will generalize to other branches. Nevertheless, this bold strategy did pay off. Direct contingency principles were repeatedly shown to be general across species, and even to be quite useful when carried into applied domains (Ribes-Inesta, 1977).

As a result, with the possible exception of neuropsychology, there is no area of psychology with a stronger relationship between basic and applied science than behavioral psychology. The experimental analysis of behavior and applied behavior analysis share the same language, philosophy of science, experimental methodologies, and commitment to empirical science. Both deal with manipulable functional variables, both embrace a pragmatic truth criterion, and both seek both prediction and influence of the behavior being considered. All of these factors promote the integration of basic and applied domains in behavioral psychology (Gifford & Hayes, 1999).

Over the nearly seventy years since Skinner's first text the discipline has made major steps in the application of behavioral principles to problems of social concern. Applied behavior analysts have regularly examined the degree to which knowledge acquired in basic laboratories is useful in applied arenas (e.g., Critchfield & Kollins, 2001; Fisher & Mazur, 1997; Hayes, Rincover, & Solnick, 1980; Mace, Mauro, Boyajian & Eckert, 1997; Stromer, 2000; Stromer, McComas & Rehfeldt, 2000; Vollmer & Hackenberg, 2001) and citation analysis indicates that applied behavior analysis does indeed attend to the basic area (Critchfield, 2002).

Despite the relative success and constant concern about creating an integrated discipline, both basic and applied behavior analysis have tended to have less or at least a more narrowly focused influence over time in psychology as a whole (Critchfield, 2002). Applied behavior analysis has increasingly focused on work with young children and language delayed individuals, or with humans in controlled environments. Basic behavior analysis has suffered much the same fate, as general interest in animal learning has waned. Both areas seem to be under-funded and under-represented in the world of psychology at large.

This situation presents a paradox. On the one hand, behavioral psychology is becoming more integrated and coherent. On the other hand, the major promised benefit of a comprehensive, coherent field –a broad and useful account of the full range of phenomena that confront psychology– has simply not appeared. Instead, behavioral psychology is simultaneously more integrated and coherent –and more limited and narrow. The paradox disappears, however, when one considers two topics: the behavior analytic research strategy, and the history of behavior analysis.

Behavior analysis is often described as an atheoretical area, but nothing could be further from the truth. It is not that behavior analysis is atheoretical –it is that its brand of theory is unusual.

From the standpoint of behavior analysis the psychological level of analysis involves "whole organisms interacting in and with a historical and situational context" (Hayes, Strosahl, & Wilson, 1999, p. 18). The purpose of behavior analysis is to predict and influence those interactions with precision (a limited set of concepts may be applied to any given event), scope (a numbers of events may be successfully analyzed with these concepts), and depth (concepts maintain contact with scientifically successful concepts at other levels of analysis). Concepts that help achieve this analytic goal with high levels of precision, scope, and depth are called "behavioral principles."

At the level of the individual, the analysis of behavioral interactions using behavioral principles so defined is called "functional analysis." Functional analysis includes both conceptual and empirical aspects, beginning with observations of behavior-environment relations and continuing through the manipulation of contextual events.

When sets of functional analyses exist within a behavioral domain, broader generalizations can be made that explain complex forms of behavior. These generalizations are theories, but they are analytic abstractive, not hypothetico-deductive: "Analytic abstractive theories are simply organized sets of behavioral principles that are used to help predict and influence behaviors in a given response domain" (Hayes et al., 2001b, p. 143).

The research strategy of behavior analysis makes sense given these theoretical goals. In order to identify concepts that have both high precision and high scope, it makes sense to begin with behavioral events that are both specific and simple, and learn to detect these processes in multiple contexts. When behavior analysts studied schedules of reinforcement with rats and pigeons in relatively barren environments this work was not being done to understand rats and pigeons per se, but to understand a functional aspect of organism-environment interactions that might apply to a wide range of behavior. Attempting to develop concepts first in the context of complexity itself could easily lead to analyses that are high in precision but low in scope (as with the stories told about an individual human life) or high in scope but low in precision (as with the broad dispositional generalizations about human beings that are common in the general culture). But as Skinner emphasized in the quote in the first sentence of this paper, a bottom-up strategy only makes sense if it actually leads to analytic-abstractive theories of complex behavioral events. Behavioral principles are not ends in themselves.

THE HISTORY OF BEHAVIOR ANALYSIS

Skinner was well aware of the dangers of his strategy: the principles identified might not be adequate to understand human behavior:

> Whether or not extrapolation is justified cannot at the present time be decided. It is possible that there are properties of human behavior which will require a different kind of treatment... I may say that the only differences I expect to see revealed between the behavior of a rat and man (aside from enormous differences of complexity) lie in the field of verbal behavior. (Skinner, 1938, p. 442)

Not quite 20 years later, Skinner managed – in his own mind at least –to account for the differences of human verbal behavior by defining it as behavior reinforced through the mediation of another who is trained precisely so as to do so (Skinner, 1957).

In our book on Relational Frame Theory (RFT: Hayes, Barnes-Holmes, & Roche, 2001a) we detail the problems this definition posed for the experimental analysis of human verbal behavior. We will not repeat those arguments here, because it is more the history of the issue than its substance that is presently at issue. Skinner's book seemingly provided behavior analysts with a way to approach human language. However, this solution was rejected by the field of psychology. There are good reasons for this, that go far beyond bigotry toward behavioral interpretations. Skinner's account of verbal behavior is a theory, in the analytic abstractive sense of that term. But the purposes of such theories in behavior analysis are empirical: the prediction and influence of a response in a given domain. Other than the applied utility of the mand/tact distinction, the empirical results of Skinner's theory were slim (e.g., Pino, 1994).

Unified and Yet Narrowly Focused

The previously described paradox of behavior analysis makes sense if we consider both the purpose and history of the field. Behavior analysis sought to and has, to a large extent, achieved a unified, coherent discipline with applied and basic branches. A unified, coherent discipline should, in principle, lead to a field with robust empirical programs across the range of major topics in psychology that exerted a strong influence in all of the important areas of psychology. That, however, has not happened. Instead, both its applied and basic branches have become narrowly focused and relatively non-influential in the context of the field of psychology as a whole. The problem can easily be seen in its two major journals. The *Journal of Applied Behavior Analysis* (*JABA*) is dominated by studies of developmental disabilities, with only limited contact with the scores of other applied problems. The *Journal of the Experimental Analysis of Behavior* (*JEAB*) is dominated by the study of direct contingencies in non-human animals, with only limited contact with the scores of experimental problems in basic human psychology.

We believe that the source of the paradox lies in the failure to develop a relatively adequate analysis of human language. As a result of this failure basic behavior analysis has spent far too little time developing the preparations and principles needed to research and understand human language, and applied behavior analysis has not had the analytic tools needed to venture into the many domains of complex human behavior that seem to require such principles. Like twins huddled together to ward off the cold, the relationship

between applied and basic behavior analysis is unified and supportive, but encapsulated. Relational Frame Theory, we argue, provides a way forward.

We will not explain RFT and defend it intellectually in this paper. There is an extensive literature of that kind already written, and other papers in this special issue will deal with some of the intellectual issues involved. Rather we will reflect on the relationship between applied and basic behavior analysis in the context of RFT.

THE HISTORY OF RFT

Many of the early applied behavior analysts were first basic behavior analysts, and they brought their sensibilities with them into the applied environment. RFT developed in precisely the opposite way. It originated in the attempt by an academic clinical psychologist to find an approach to language and cognition that was functional, behavioral, experimental, and practically useful. The first article on what would later become RFT was also the first article on what would later become Acceptance and Commitment Therapy (Hayes, 1984). RFT is an example of the practical need for a basic account driving the development of the account.

There have not been many good examples of that kind of implanting of clinical interests into basic psychology research programs, but applied behavior analysts in particular have always been skeptical of the idea that applied work does not have basic implications: "...differences between applied and basic research are not differences between that which 'discovers' and that which merely 'applies' what is already known. Both endeavors ask what controls the behavior under study." (Baer, Wolf, & Risley 1968, p. 91). However, applied behavior analysis is usually not in a good position to discover or research new principles. This is exactly the same reason why basic behavior analysts have emphasized the analysis of simple responses in simple environments in their basic work: complexity and unexplained variability creates barriers for successful inductive, analytic/abstractive approach. RFT, while emerging from applied concerns, developed first in the basic environment. Thus, a more interactive strategy was forged.

MUTUAL INTEREST

Most approaches to a successful relationship between applied and basic psychology suppose that applied psychology makes use of knowledge largely developed elsewhere (the *clinical application model*), or that applied workers will pass along meaningful issues and hypotheses to their basic colleagues who will test them (the *mutual obligation model*). Neither model tends to work very well (Hayes, 1998). The former demands that applied psychologists know basic literature better than most basic psychologists, while the latter demands that basic psychologists be able to distinguish good clinical ideas from poor ones. Furthermore, both ideas suggest that basic psychologists either have or will develop the knowledge needed for application when in reality basic issues often become popular merely because they are intellectually interesting. As a result, the

issues being researched at any one time hardly encompass a complete set of issues that might be useful to application.

RFT was developed on the basis of the "mutual interest model" (Hayes, 1998), namely, that a productive relationship between applied and basic areas requires a shared interest in specific theoretical issues, and sets of researchers who can pass that interest back in forth in a fashion that fits with the sociology of each branch of science. Verbal behavior is an ideal topic for such a model. It is natural for psychotherapists to be interested in the impact of human language since much of what they do is to talk to clients. The nature and impact of verbal behavior is also an area of interest in basic psychology. However, there is little to ensure that this commonality of interest alone will lead to the kind of basic analyses that are practically useful and when the development of useful applications is halted there is nothing in the phenomenon of verbal behavior itself that will automatically overcome that impasse.

If an adequate set of principles does not exist to guide applied psychology, applied psychologists need to follow their interest across the sub-disciplinary divide. Thus, they must develop possible approaches, and then place the analysis that results in the basic field. When this is done it cannot be justified on applied terms but rather it needs to be justified by basic concerns, in the hopes that this will make it more likely that basic psychologists will pursue the issue further.

This process is exactly what happened with RFT. Our early attempts to understand verbal regulation lead to basic work on rule-governance (see Hayes, 1989 for a book length treatment), but this in turn led to an interest in the nature of verbal stimuli, and thus to an interest in stimulus equivalence. Working with a basic behavior analyst, the late Aaron Brownstein, an operant approach to derived stimulus relations was developed (Hayes, 1984; Hayes & Brownstein, 1985). A detailed account appeared a few years later (Hayes & Hayes, 1989), along with a workable experimental preparation (Steele & Hayes, 1991). When this line of thinking was embraced by a remarkably creative and productive experimental psychologist, Dermot Barnes-Holmes, RFT became a full-fledged research program. The clinical lab that helped start the process then began to feed off of its results –using them in various applied areas. Interestingly, since the mutual interest model works in both directions, Barnes-Homes began to show new applied uses of these basic data.

The mutual interest approach is much more easily accomplished within behavior analysis than in other psychological disciplines. What made RFT possible is the ability in behavior analysis for applied researchers to explore a basic research paradigm, and publish in basic research journals, without a significant change in language, method, or approach. In fact, many of the researchers who have published important work in the area of derived stimulus relations over the years are applied psychologists.

IMPLICATIONS OF RFT FOR THE RELATIONSHIP BETWEEN APPLIED AND BASIC BEHAVIOR ANALYSIS

RFT has more long-term implications for the relationship between basic and applied behavioral psychology, however. To the extent that RFT is successful, behavior

analysis itself will change, and do so in a fairly dramatic way. There are several reasons why this is so. First, RFT claims that arbitrarily applicable relational responding is at the core of language and cognition. Second, it suggests that framing events relationally alters how direct contingencies operate, which suggests that much of our hard won knowledge about behavioral principles needs to be reexamined. Third, many of the issues of importance in RFT can only be studied readily in the context of complex human behavior. Fourth, basic behavior analysis is not currently well organized to study these phenomena, in part because of an excessive emphasis on animal learning preparations, and thus reorganization is necessary to pursue the analytic leads RFT presents. Finally, the more important relational phenomena occur most dominantly in the context of applied work. For all of these reasons, RFT seems likely to alter the relationship between applied and basic behavioral psychology to the extent that it is successful.

Relational operants and human language

RFT claims that language *is* framing events relationally. This definitional isomophism carries with it several empirical predictions, all of which have some supportive data. First, since it is clear that humans are far more able in symbolic language than are non-humans, it should be difficult though not necessarily impossible to establish relational frames in non-humans –and it is. After nearly 30 years of trying there are only a very few cases in which arbitrarily applicable relational responding *may* have been established in non-humans (e.g., Schusterman & Kastak, 1993) and in these few cases the training was both arduous and in line with the kinds of training regimens RFT suggests for establishing relational operants. Relational operants in children, conversely, are readily apparent even in infancy (e.g., Lipkens, Hayes, & Hayes, 1993). Second, children with absent or delayed language repertoires should show absent or delayed relational abilities, and they do (e.g., Barnes, McCullagh, & Keenan, 1990; Devany, Hayes & Nelson 1986). Third, fluency with relational frames should predict good performance on a variety of verbal or cognitive tasks, and it does (e.g., O'Hora, Pelaez-Nogueras, Barnes-Holmes, & Amesty, in press). Fourth, basic language skills should be trainable using derived stimulus relation procedures, and it is (e.g., deRose et al., 1992). Fifth, you should be able to model complex language performances using RFT preparations, and you can (e.g., Barnes-Holmes, Barnes-Holmes, & Cullinan, 2000; Wulfert & Hayes, 1988). Sixth, psychological disorders that are obviously in part cognitive should respond to RFT-based treatments and they do (e.g., Bach & Hayes, 2002). Seventh, brain activity during derived relational tasks should look like such activity during the language tasks being modeled and it does (e.g., Dickins, Singh, Roberts, Burns, Downes, Jimmieson, & Bentall, 2001).

If relational operants are indeed at the core of language and higher cognition then to the extent that behavioral psychology aspires to "an eventual extension to human affairs" (Skinner, 1938, p. 441) it must deal with these behaviors, since most forms of complex human behavior (e.g., thinking, problem-solving, planning, persuading) obviously are in part language-based events. What behavioral psychology needs is a

comprehensive, coherent, progressive, experimental, monistic, contextual, and practically useful behavioral analysis of language and cognition. That is precisely the prize RFT seeks as well. To the extent that RFT is a good avenue toward it, major changes for behavioral psychology are implied as we will explain.

Relational frames alter how direct contingencies operate

In one sense, arbitrarily applicable derived relational responding can be predicted from an operant conceptualization. The core of RFT is as basic a claim as can be made in behavioral psychology: that a particular kind of responding is an operant. RFT does not appeal to new principles to explain this operant: it appeals only to contacted consistencies in contingencies across multiple exemplars. But in another sense, arbitrarily applicable derived relational responding presents a real challenge to operant psychology for this reason:

> the instrumental behavior of relational framing alters the functions of behavioral processes... If Relational Frame Theory is correct, the alteration of these behavioral processes was itself a learned process. Said another way, relational framing is operant behavior that affects the process of operant learning itself. We know of no term for such an effect (Hayes, Barnes-Holmes, & Roche, 2001a, p. 45).

In the RFT book we go on to given an extended example that explains how derived relational responses involve the alteration of direct contingency processes:

> A discriminative stimulus is a stimulus in the presence of which there has been a greater probability of reinforcement for a given behavior than in its absence. Suppose a child is rewarded for waving when the word "dog" is heard. The word "dog" is a discriminative stimulus. Suppose, however, that the child is now taught to say "dog" given the word D-O-G, and to point at actual dogs given D-O-G. Suppose that as a result of this training the child now waves upon seeing a dog. Such an outcome has repeatedly been seen in the literature (e.g., Hayes, Brownstein, Devany, Kohlenberg, & Shelby, 1987). The dog cannot be a discriminative stimulus because the child has no history of greater reinforcement for waving in the presence of dogs than in the absence of dogs. The effects cannot be stimulus generalization because there are no formal properties that are shared between the word and actual dogs. The effect cannot be due to classical conditioning because it would require an appeal to backward conditioning. The effect cannot be due to compounding because "dog" and dogs have not even occurred together.
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> Relational Frame Theory suggests that the performance is due to a learned process that transformed these discriminative functions. In normal discriminative control, the stimulus function is learned, but not the process itself. In contrast, the derived performance is discriminative-like, but it is not discriminative. These discriminative-like effects seem to depend on a learned process of altering behavioral processes, and that is something that is not covered by an existing technical term. Despite the conservatism of an RFT approach, therefore, ... in our analysis, verbal events (and relational frames) instantiate a newly identified behavioral process (Hayes & Hayes, 1992). (Hayes, Fox, Gifford, Wilson, Barnes-Holmes, & Healy,

2001, p. 46).

There are already quite a number of empirical examples of effects of this kind. Relational frames can alter reinforcement functions (Hayes, Kohlenberg & Hayes, 1991), discriminative functions (Hayes et al., 1987), classically conditioned functions (Dougher, Auguston, Markham, Greenway, & Wulfert, 1994), and establishing operations (Ju, 2000). It is important to realize, however, that this "new process" is a way of speaking about an empirical *result* of relational operants. No new principle is needed to explain how relational operants themselves come about in the first place.

The implications for basic behavior analysis are sobering. The whole purpose of behavior analysis (analytic, abstractive theories of behavior) requires a bottom up approach. If relational frames are both a core of human language and can alter how direct contingencies operate, then we must do the difficult work to understand exactly how direct contingencies are altered by verbal contingencies because without that knowledge we cannot safely and directly extend our principles of behavior derived from non-human organisms to language able humans. We now need to know how they operate in the context of a repertoire of relational operants. This research agenda puts tremendous pressure on basic behavior analysis.

Basic behavior analysis and complex human behavior

The reason the pressure is great is that understanding how relational operants alter basic learning processes will require scores of difficult studies on the complex behavior of human beings. There are only a relatively few important RFT questions that might be asked with non-humans. One might ask "can you get stimulus equivalence," for example. So far, the studies that might have shown such results (e.g., Schusterman & Kastak, 1993), contained *extensive* relational training of the sort RFT supposes to be important to the development of relational operants. Suppose, just for the sake of argument, we agree that with sufficient training non-humans show relational operants. What next? In order to understand how relational operants alter direct contingencies we need to bring multiple forms of this repertoire into well crafted experimental instances of other behavioral processes. We need to consider how relational operants interact, how relational networks are constructed, and topics of that kind. So far as we can tell, these questions cannot be examined if we have to spend years working with non-humans only to arrive, at best, at the most primitive form of relational responding.

The solution is to examine these issues where they occur: with verbal human beings. But that solution presents enormous practical, tactical, and political problems. As it is currently constituted, basic behavioral psychology may not be up to the task.

Basic behavior analysis is not currently well organized to do this work

The "bottom up" strategy of behavior analysis was built on animal learning, but if the arguments just provided are correct, animal learning must become a much smaller aspect of basic behavior analysis. It appears that the animal learning tradition is declining

generally. The shrinkage has been facilitated in the United States particularly by changes in funding patterns, and the conviction that animal learning models do not provide a full range of principles needed to explain complex human behavior. Yet, it remains the case that the vast majority of the articles in JEAB are done with non-humans. The relatively few studies that appear in JEAB on derived stimulus relations tend to be done with humans with minimal verbal abilities. Much of the best work on RFT, especially in the last several years, has appeared elsewhere.

Perhaps the largest part of this overrepresentation of animal studies in JEAB is an issue of student training. Few basic behavioral laboratories are equipped to do work of this kind. It is not a central part of the tradition and the required preparations are not part of the training that occurs in these basic laboratories. Faculty positions for basic behavioral researchers who work only with non-humans are scarce. A self-amplifying loop is created in which the narrowness of basic behavior analysis feeds leads to a smaller and even more narrow field.

We are left with a troubling question. Who then will do the needed work and where will it appear? In some places this "Catch 22" has largely been solved (e.g., in Ireland) because basic behavioral laboratories that have embraced RFT research are placing their graduates in doctoral departments of psychology in "cognitive science" programs, leading to more graduates, who also can be similarly placed. That solution is still so unusual, however, that it may not be enough given the need for research of this kind: scores of laboratories are needed and in many countries. Fortunately there is another way forward.

Where does one see key questions about relational phenomena? The importance of relational frames is perhaps most obvious in applied fields, such as education, social processes, psychopathology, and the like. Moreover, the basic analyses that are needed are most obvious in these situations, and the ability to work on those issues is often most readily accomplished in such situations.

Thus, in addition to growing basic behavioral laboratories that are researching such questions, applied behavioral laboratories could do so as well. Applied psychologists have the most intimate contact with the kinds of psychological events that are presumed by RFT to be important to language. If some of these researchers conducted both basic and applied analysis, a rapid expansion of research on relational operants could result. If our model is correct then some these analyses will be sufficiently interesting for basic researchers to refine them in order to come to a better understanding of the processes involved.

For example, in the context of educational programming researchers are likely to realize that RFT itself suggests that some task arrangements should provide more relational training than others. To give a concrete instance, it might be better to train a new word using exclusion rather than equivalence, because the procedure relies on more relational operants in each trial (novel comparison is different than known comparison; known comparison is in frame of coordination with other unseen sample; novel sample is in frame of coordination with novel comparison). This can be cast as a very basic issue in RFT, but it would probably never occur if the applied environment did not provide the lead in the first place.

What this means is that a *basic* analysis of language could be vitalized by a tight link to an *applied* analysis of language. As such, RFT research seems likely to make applied behavior analysis a more basic endeavor, and make the experimental analysis of behavior more dependent on applied results, thus fundamentally altering the relationship between applied and basic fields in behavioral psychology.

Already this pattern is obvious in RFT research. Basic RFT studies often are supposedly "too applied" for JEAB and "too basic" for JABA. In fact, these basic studies are not too applied, but when one is dealing with metaphor, reasoning, training, persuasion, and so on they *look* applied to many basic behavioral researchers because the phenomena themselves are complex human behavior.

In the present situation it seems probable that applied behavior analysts are needed to take RFT to the next level within behavioral psychology, in the form of language training programs and the like. In the not too distant future cognitive researchers may discover the methodological advantages RFT can give them, which may further build the basic support for RFT research. It is an open question whether RFT will prosper within basic behavioral psychology, because it presents a real challenge to the status quo, but that too seems to be on the horizon. Once the wrenching transition from an animal learning focus takes place, basic behavioral psychology will be positioned to see if behavioral ideas in this area will be vigorously and broadly supported by psychology as a whole. Until then, the answer almost certainly will no.

Conclusions

When we wrote the RFT book (Hayes et al., 2001a) we deliberately wrote it to appeal to behavioral psychologists first, and non-behavioral psychologists second. We did so for this reason: if RFT cannot find a way to succeed on its home turf, it will be unlikely to produce enough research on enough topics to show the kind of analytic breadth and precision we believe it affords the field.

As this process of assimilation of the RFT research agenda into basic behavior analysis proceeds, however, the depth of the challenge RFT presents to certain aspects of behavioral psychology is more evident. This much seems certain. If RFT succeeds as a behavioral research program, behavior analysis will never be the same.

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