ABSTRACT. The present set of papers show that leaders in the field of organizational behavior management are grappling with issues of human language and cognition. That is a good and necessary step for the field, but the solutions proposed are worrisome: adopting principles from non-behavioral psychology, adopting principles from introspection that have not been empirically validated, returning to methodological behaviorism, or appealing to non-empirical interpretations using traditional behavioral principles. In this paper I argue that these are the wrong solutions, being taken for the right reasons. There is a need for an analysis of language and cognition, but it will be found neither in other forms of psychology nor in traditional Skinnerian thinking on the topic. I suggest instead that OBM look at the data that exists in contemporary basic behavior analysis on the topic, particularly in the area of Relational Frame Theory. That work is a vigorous area of research, and its applied implications are significant, growing, and known to be relevant to organizations. The solution to the malaise these papers reflect is most likely to be found in post-Skinnerian behavior analysis itself. [Article copies available for a fee from The Haworth Document Delivery Service: 1-800-HAWORTH. E-mail address: <docdelivery@haworthpress.com> Website: <http://www.HaworthPress.com> © 2005 by The Haworth Press, Inc. All rights reserved.]
B. F. Skinner conceived of behavior analysis as a field that “began with simple organisms in simple situations and moved on, but only as its growing power permitted, to the complexities of the world at large” (1966, p. 14) and that derived its importance “from the possibility of an eventual extension to human affairs” (Skinner, 1938, p. 441). In the middle part of the last century, enough basic behavioral principles had been developed that they could begin to be applied to applied problems. The initial successes suggested that Skinner’s bottom-up strategy had worked. New applied behavioral techniques and procedures flourished in applications across a broad range of human problems. In the world of psychology, circa 1972, the future of applied behavior analysis was bright, and it was broad. No one knew how many human problems would yield to a bottom-up behavior analytic approach, but few behavior analysts doubted that it would be a very long list indeed.

Over thirty years later progress in applied behavior analysis has continued and deepened in some areas, but it takes only minutes with a current issue of the Journal of Applied Behavior Analysis to confirm that it is no longer broad. When “behavioral principles” are taken to refer simply to direct contingency principles, there is an obvious pattern of uneven success in their application. Direct contingency principles work with all complex living organisms, but in general they work less well and less predictably whenever we are dealing with verbally competent people, particularly if their environments are relatively uncontrolled. Practitioners dealing with human adults in particular, whether they are psychotherapists or organizational psychologists, find over time that direct contingency principles provide less and less point to point guidance in the development of new and innovative technologies.

Applied behavior analysis in general has become dominated by the study of developmental disabilities, where direct contingency principles continue to provide relatively adequate guidance for technological development. In areas dealing with human adults the story is very different. Behavior therapy largely “mined the direct contingency vein” within a decade, and few new major behavior therapy approaches over the last twenty years have arisen that are based purely on direct contingency principles. Organizational behavior management has taken longer, but if some of the present articles are any indication it too is now
facing the stark reality that something more is needed in the theoretical armamentarium. A brief review of these articles is instructive.

Noting the limited and waning interest in organizational behavior management, a former editor of the *Journal of Applied Behavior Analysis* and his colleague (Wiegand & Geller, this issue), propose to expand organizational behavior management to include ideas and data from various areas of psychology that have historically been viewed as almost the anti-thesis of behavior analysis, including achievement motivation, self-worth theory, self-efficacy, and personality typology. These are old areas, some older than applied behavior analysis itself, and they have long been used as the basis of traditional industrial/organizational psychology. Almost no attempt is made by Wiegand and Geller to account for these areas and the data they contain by an analysis based on behavior analytic principles. Indeed, that appears to be the whole point of their article: new principles are needed and they can be found in mentalistic psychology. It seems to be true that new principles are needed, but this proposal represents a clear abandonment of the bottom-up behavior analytic tradition. All behavioral data (using the term “behavioral” in its broadest sense) must ultimately be understood by an adequate behavioral science, but as is reflected in the Skinner quote above, the whole idea of behavior analysis was to develop behavioral principles (meaning descriptions of functional relations that allow behavioral interactions to be predicted and influenced with precision, scope, and depth) and to use these to understand increasingly complex phenomena so as to construct a comprehensive and coherent discipline. If ideas drawn directly from achievement motivation, self-worth theory, self-efficacy, and personality typology are treated as if they are behavioral principles, even though they have not been subjected to the same criteria as behavioral principles, then behavior analysis as a field simply no longer exists. The authors are correct that the form of organizational behavior management defined by such an approach would then be part of the psychological mainstream, but only in the sense that water dropped into a river is part of the river. Industrial/organizational psychology already uses contingency interventions on occasion, but its philosophy of science is not contextual, its principles are minimally behavioral, and the creation of an integrated science is not its aspiration. If OBM needs IOP to survive, it might as well just become IOP, or (the functional equivalent) simply cease to exist all together.

Malott (this issue) argues that organizational behavior management will be more effective, or at least more correct, if it is based on ideas derived not just from direct contingency principles but also from the re-
sults of his introspection and philosophizing. Almost no attempt is made to link these recommendations to direct experimental tests of his ideas, basic or applied. Malott has had these ideas for a long time. Variants of the “Jewish Mother” argument were being made by Malott in his “Notes from a Radical Behaviorist” newsletter decades ago. If these ideas are scientifically valuable, surely their value can be shown in direct, experimental analyses. So far as I can tell, little or no such data have been produced. If behavioral introspection is to become the basis for applied behavior analysis, then once again behavior analysis itself has been abandoned. There was a reason that the field was called the experimental analysis of behavior. This reflected not just a commitment to data of any kind, but also a commitment to the empirically based development of behavioral principles as defined above. It is true of course that behavior analysis includes philosophical and conceptual areas, and when complex phenomena (applied or basic) cannot yet be analyzed experimentally, armchair extensions of behavioral principles might be useful, either to help provide a coherent account or to orient the field toward ways in which complex phenomena might ultimately be analyzed empirically. But cautious and helpful extensions are not the same as decades-long introspective speculations. Jewish mothers may declare the results of such an exercise to be kosher, but behavior analysts must not or the field itself will no longer have credibility as an experimental science.

In addition to Michael’s well known discussion of positive and negative reinforcement, Michael (this issue) properly cautions against the use of mentalistic concepts, and provides a very brief behavioral interpretation of self-efficacy. That is fine so far as it goes, but it does not go very far. Some of Michael’s analysis would apply as well to non-verbal organisms (where few would argue that “self-efficacy” is a relevant concept). In places where verbal concepts are used (e.g., goal-setting) no technical account is provided.

McSween and Matthews (this issue) praise OBM and argue that its permanent success will come only from the ability of practitioners to consistently implement interventions that demonstrate lasting results. Behavioral safety practitioners are praised in that regard, but no note is made of the ironic fact that one of the very practitioners being praised (Geller) is arguing for the inadequacy of the current set of behavioral principles. Indeed, McSween and Matthews also agree that the world of OBM needs more than principles derived from “a simple Skinner box analogy.” Their solution is fundamentally different than Wiegand and Geller’s, however, and in broad terms is more in keeping with the be-
havioral tradition. For example, they point to Komaki’s (2000, as cited in McSween and Matthews, this issue) observation that supervisors make workers angry with rhetorical questions and argue that this “is an important observation that would not be easily predicted from basic behavioral concepts.” When behavior analysis is unable to predict and influence behavioral phenomena in a given domain, by definition either additional behavioral principles are needed, or more creative applications of behavioral principles to that domain need to be developed. The authors suggest the latter alternative, appealing to the 35-year-old work of Daryl Bem, a social psychologist who dabbled in the use of behavioral principles in the early “brave new days” of applied behavior analysis. While this general strategy makes sense, the authors make no attempt to explain how Bem’s simple idea that attitudes are verbal descriptions of past behavior even bears on Komaki’s observation, nevermind explaining it. The authors correctly state that the data of social psychology should be embraced by behavior analysis (a point that could be expanded to include all behavioral data as noted earlier) but the analyses should be behavioral. There is a certain irony in this, however, since the behavior analytic aspect of Bem’s ideas have largely disappeared in Bem’s own work, and social psychology itself never found these aspects of his ideas useful.

Like Wiegand and Geller, Crowell and Anderson (this issue) chaff against the bonds of behavioral principles, and are looking for what they call a more “eclectic account.” They reject Wiegand and Geller’s embrace of cognitive causes, and restate several very traditional methodological behavioral objections to them. Their solution is to refashion OBM simply as an empirical, objective, and valid area that embraces theories provided they are not mentalistic. This is a traditional methodological behavioral solution, more in keeping with S-R tradition than with behavior analysis, and indeed Hull and Spence are cited in support of their argument. But methodological behaviorism already has had its day in traditional IOP. It is not clear why OBM needs to pursue a path long abandoned by others and indeed one that behavior analysis itself turned away from. Perhaps they have other ideas in mind, but they are not explicated in the article.

To the extent that it is addressed, all of these papers acknowledge that OBM faces major challenges, although different solutions are provided. Most of them seem to agree that OBM is in a weakened competitive and perhaps technological position because it is currently difficult to address a full range of complex problems of adult human beings in organizational settings. Some propose expansion of principles to include those
drawn from traditional, non-behavioral areas of psychology; others look for such expansion provided it is not mentalistic; others propose expansion based on behavioral introspection and analyses that are not based in careful experimental research; others propose a better use of existing behavioral principles but point to analyses that are either cursory or that are decades old and never led anywhere. Some of the alternatives being advocated involve a de facto abandonment of behavior analysis itself, either in its experimental and functional tradition, its aspiration to be a coherent discipline, or in the standards applied to new principles. From the point of view of behavior analysis as a progressive, coherent approach, it is not a pretty picture.

I intend to make a rather simple argument, organized into six specific claims. The first five statements, I believe, are empirically supported and I will provide evidence for each in the sections that follow. The last flows logically from the first five. I will consider each of these points in turn:

1. In order to predict and influence the full range of complex human behavior of applied importance, behavioral psychology will need to develop an adequate analysis of human language and cognition.
2. Traditional Skinnerian accounts did not lead to an adequate basic analysis of human language and cognition and is not relied upon significantly in applied areas that deal with human adults.
3. There currently exists a relatively well-developed post-Skinnerian behavior analytic approach to human language and cognition, Relational Frame Theory (RFT), that has dealt experimentally with an unusually broad range of verbal and cognitive phenomena.
4. RFT has already led to successful applied interventions that deal with the problems of human adults.
5. These interventions include a small number of organizational studies, but the results there are similarly positive.
6. If organizational behavior management is to remain true to its roots but grow conceptually currently the best way to do so is to embrace post-Skinnerian behavior analytic science rather than to embrace foreign philosophies, theories, and principles.

The Need for an Adequate Behavioral Analysis of Human Language and Cognition

The field of behavior analysis does now not feel able to address issues of human language and cognition in a detailed and effective man-
ner. The evidence for that is widespread, but these very papers themselves are an indication of the extent of the problem. Nearly every example used of problems that the field has a hard time explaining, and every non-behavioral principle that someone is wishing to embrace, obviously revolves around that human language and cognition. It is the elephant that is filling the room. Such issues as reactions to rhetorical questions, the role of personal beliefs, the impact of predictions about future success or failure, and so on, are obviously issues of human verbal behavior. It is just as obvious, however, that the field is not sure what to do with such issues. In general, these papers reflect a desire to flee from this elephant, to minimize its presence and accept cursory explanations, or to abandon traditional behavior analytic approaches in the face of it, rather than to face it head on and advance a coherent and effective behavior analytic account.

The Traditional Skinnerian Account of Verbal Behavior Has Failed OBM

It hardly needs to be said, but if in order to be effective, applied behavior analysts (including even a past editor of its flagship journal) feel the need to abandon traditional behavioral accounts in favor of achievement motivation, analysis of personal beliefs, self-efficacy, and the like, then these traditional behavioral accounts have failed the field. Not one of these papers cites Skinner’s *Verbal Behavior* (1957). That is not surprising, since leading behavior analysts have long noted that Skinner’s account of verbal behavior never lead to a robust experimental program, or indeed to data that differed significantly from those evidenced in typical operant studies performed with non-human subjects (Michael, 1984). There is another way to say this: Skinner’s analysis adds little to direct contingency accounts of human behavior. In other venues, I and my colleagues have tried to explain why that happened (Hayes, Blackledge, & Barnes-Holmes, 2001) but the most important step toward solving these problems is to recognize that it happened. If Skinner’s analysis is not working, nearly 50 years later, it is time to say “enough is enough” and get on about creating an analysis that will work. The understated core of all of the papers in the current set that are arguing for an expansion or modification of existing analyses or principles is that the analysis is not working, since it is not being used and the non-behavioral alternatives being suggested involve a dramatic abandonment of the behavioral tradition.
The Growing Lack of Reliance on Contemporary Basic Behavior Analysis

When applied behavior analysis is in need of basic guidance on issues of behavioral theory and principles, it has historically turned to basic behavior analysis, not to mentalistic concepts, social psychology, introspection, or methodological behaviorism. But basic behavior analysis moves and changes. If that field is alive and well, applied behavior analysts should need to keep abreast of it.

What would we want of a relatively adequate basic behavioral account of human verbal behavior? There may be several features, but ideally we would want an account that is thoroughly grounded in existing behavioral principles and is coherent and logical, is empirical and ideally experimental, deals with a wide range of relevant verbal and cognitive phenomena, simplifies these complex phenomena without trivializing them, and leads to application. At the present time, I believe that there is only one behavioral account that comes close to those criteria: Relational Frame Theory (RFT; Hayes, Barnes-Holmes, & Roche, 2001). It is, so far as I know, the only comprehensive, book length contemporary behavioral account of language and cognition other than Skinner’s. Given that, it seems worth considering each of the criteria mentioned.

Based in Behavioral Principles and Is Coherent and Logical. The core of RFT is about as behavioral as one can get: relating is an operant. In the 20 years since RFT was first put forward (Hayes & Brownstein, 1985) I am unaware of a single article (of the dozen or more criticisms and commentaries that have been published by others about RFT) that has attempted to make the case that RFT is not in touch with what is known about existing behavioral principles, or that the theory is illogical. Perhaps the closest to such a claim is Sidman’s view that “A linguistically naive organism’s abstractions among commonalities from a set of exemplars that share no physical feature requires more of an explanation than just a history of experience with the exemplars” (1994, p. 557) but that very claim was backed up by no data, seems behaviorally incoherent (it in essence decrees a priori that some responses cannot be operants), and is directly contradicted empirically not just by the RFT literature (e.g., Barnes-Holmes, Barnes-Holmes, Roche, & Smeets, 2001) but also by a large empirical literature on the operant nature of imitation, randomness, novelty, creativity, and similar responses.

Empirical and Ideally Experimental. There are currently over 60 published empirical articles on RFT, the great majority of which is ex-
perimental. About another dozen are in press. This does not count the vast literature on derived stimulus relations more generally that is not directly linked to RFT. Almost all of this is published in major behavioral journals, such as the *Journal of the Experimental Analysis of Behavior* and *The Psychological Record*. Basic RFT research already considerably exceeds basic research on Skinner’s *Verbal Behavior*, although the applied literature on the latter is probably still largely due to the amount of work done on tacts and mands in developmental disabilities. The basic ideas central to the theory that have been tested include:

- the establishment of multiple stimulus relations (e.g., Steele & Hayes, 1991; Roche & Barnes, 1996);
- the regulation of relational responding by consequences (e.g., Healy, Barnes-Holmes, & Smeets, 1998; 2000);
- the role of multiple exemplar training (e.g., Barnes-Holmes, Barnes-Holmes, Roche, & Smeets, 2001);
- the regulation of relational responding by contextual stimuli (e.g., Barnes, Browne, Smeets, & Roche, 1995; Wulfert, & Hayes, 1988);
- the role of relational responding in the transfer and transformation of stimulus functions (e.g., Dymond, & Barnes, 1996; Hayes, Kohlenberg, & Hayes, 1991);
- the development of relational responding in infants (e.g., Lipkens, Hayes, & Hayes, 1993); and
- the correlation between relational responding and language abilities and exposure (e.g., Barnes, McCullagh, & Keenan, 1990; Devany, Hayes, & Nelson, 1986).

Virtually all of the data on these questions so far has been supportive of the theory.

**Range of Phenomena.** In the basic laboratory RFT has been studied in relation to a wide range of complex issues, many of which are explicitly mentioned in the present papers in this journal. These include:

- the regulation of emotions (e.g., Roche, & Barnes, 1997; Roche, Barnes-Holmes, Smeets, Barnes-Holmes, & McGeady, 2000);
- the generation of self descriptions and self-knowledge (e.g., Barnes, Lawlor, Smeets, & Roche, 1996; Dymond & Barnes, 1995);
- social categorization and stereotyping (e.g., Kohlenberg, Hayes, & Hayes, 1991; Watt, Keenan, Barnes, & Cairns, 1991);
- the role of relational responding in the establishment of rule and rule following (e.g., Hayes, Thompson, & Hayes, 1989; Luciano,
Herruzo, & Barnes-Holmes, 2001; O’Hora, Barnes-Holmes, & Roche, 2001);
• the nature of rule following and its impact on contingencies (e.g.,
  Hayes, Brownstein, Haas, & Greenway, 1986; Hayes, Brownstein,
  Zettle, Rosenfarb, & Korn, 1986);
• relational responding as an explanation for findings in traditional
  cognitive psychology (e.g., Hayes & Bissett, 1998);
• the role of relational responding in human perspective taking (e.g.,
  McHugh, Barnes-Holmes, Barnes-Holmes, 2004); and
• relational responding in analogy and metaphor (e.g., Stewart,
  Barnes-Holmes, Roche, & Smeets, 2001; 2002).

Again, all of these studies are supportive. Understanding human lan-
guage and cognition will involve a much longer list, of course, but this
list is already quite substantial and is much longer that lists that could be
created for less well-developed research programs in the area.

* Simplifies These Complex Phenomena Without Trivializing Them *

RFT is based on a very few central claims, all of which have already
been successfully tested at least to a degree. The fact that such a short
list of ideas can lead to such a long list of successful studies on complex
topics provide empirical evidence that the analysis simplifies complex
topics, but does it trivialize them? This is harder to assess, but perhaps
one way is to look at the views of non-behavioral theorists. Skinner’s
analysis of language was almost universally rejected by non-behavioral
researchers in the area. That is not happening to RFT. Indeed, on the
back of the RFT volume Michael Eysenck (a major cognitive psycholo-
gist, who had previously declared behavioral psychology to be dead)
said that the RFT book indicated that “a behavioral account can shed
light on many more of the complexities of language and cognition than
had previously been supposed . . . it is abundantly clear from this book
that the behavioral approach is alive and kicking, and full of vitality.”
Similarly, the well-known cognitive therapist Albert Ellis said “this
book gives more plausible explanations of why people behave the way
that they do, and particularly why they are frequently dysfunctional,
than any other I can think of.” In the eyes of major leaders outside of the
behavioral psychology, RFT does not appear to be trivializing the area.
The remaining criterion is application, which I will address shortly.

None of this need be determinative. I am not arguing that RFT is the
only possible base for extension from recent basic behavior analysis, al-
though I do think a fair examination of the literature with a reasonable
set of criteria in hand leads to the conclusion that RFT is demonstrably
farther along than competing alternatives. My point is rather more gen-
eral. In this present set of articles we see applied behavior analysts
struggling mightily with complexities of human language and cogni-
tion, and running off in all directions as they do so. Somehow no one is
turning to contemporary empirical developments in basic behavior
analysis itself for a possible solution to these problems. If these papers
had appealed to stimulus equivalence, or to recent work in joint control,
or even to the older empirical literature on rule-governance, one could
say “well, they are not considering RFT but at least they are looking to
contemporary empirical work in basic behavior analysis.” But that is
not what we see here either. It cannot be that this contemporary empiri-
cal work is not known. In the preceding two sections I have cited 27
RFT related articles spanning nearly 20 years of empirical work, 12 of
which were published in JEAB and 12 in The Psychological Record.
And this is hardly an exhaustive list. If the work in other labs on equiva-
rence, exclusion, joint control, and the like is added, there are many
more such studies. My only conclusion is either that applied behavior
analysts have lost touch with or faith in basic behavior analysis, or that
they believe basic behavior analysis to be only principles of direct con-
tingency control, regardless of developments in the basic literature and
thus when the limits of direct contingency principles are contacted, it is
time to run to theories like self-efficacy. Either conclusion is grim, as
measured against Skinner’s bright vision quoted at the beginning of this
article.

To me, this pattern suggests that if behavior analysis does not some-
how solve the problem of language and cognition, it seems doomed to
narrowness and increasing irrelevance. Unfortunately, basic behavior
analysis in the United States is currently so committed to the examina-
tion of direct contingency principles in non-human preparations, that
even if a way forward presented itself, the field is not currently orga-
nized well to take advantage of the opportunity. Perhaps that is why so
much of the basic RFT literature has come from overseas or from
clinical laboratories that also do basic research.

RFT and Application

It is one of the hallmarks of behavior analysis that we expect advan-
ces in basic knowledge to give rise to applications. I quoted Skinner
on that point in the first sentence of this article. Contemporary develop-
ments in a behavioral analysis of language and cognition have done just
that. This includes the RFT work (which I will briefly discuss here), but
it is characteristic of the work on derived stimulus relations more generally (e.g., de Rose, de Souza, Rossito, & de Rose, 1992).

The biggest current impact of RFT is in the area of clinical behavior analysis. Controlled studies have shown that RFT based clinical methods reduce drug use in polysubstance abusing individuals better than methadone and drug counseling alone (Hayes, Wilson, Gifford, Bissett, Piasecki, Batten, Byrd, & Gregg, 2004), help keep psychotic persons from being rehospitalized better than existing case management approaches (Bach & Hayes, 2002), reduce the impact of negative self-referential thoughts better than cognitive techniques (Masuda, Hayes, Sackett, & Twohig, 2004), help with smoking cessation more than nicotine replacement (Gifford, Kohlenberg, Hayes, Antonuccio, Piasecki, Rasmussen-Hall, & Palm, 2004), and reduce the impact and frequency of socially stigmatizing attitudes more than multicultural training (Hayes, Bissett, Roget, Padilla, Kohlenberg, Fisher, Masuda, Pistorello, Rye, Berry, & Nicolls, 2004), among a much longer list of positive outcomes (see Hayes, Masuda, Bissett, Luoma, & Guerrero, 2004 for a recent review). Thus there can be no doubt that RFT is linked to powerful new applied methods.

RFT and Organizations

Let’s consider an example from the current papers: success seeking motivation. Skinner always claimed that “operant behavior is the very field of purpose and intention” (1974, p. 55) but he was not talking about purpose in the sense of being able to construct a verbal future that is then sought. Skinner meant reinforcement. Reinforcement, however, is not about the future: it is about the past. When a non-verbal organism responds “for” food by pressing a bar given a light, it is only because the light–bar press–food relation has been experienced in the past. Purpose is not the same thing in the context of relational framing. Given that repertoire, and cues to bring it into the current situation, it is quite possible to relate events in an “if . . . then” fashion that have never been experienced directly to have been related. The “future” that is verbally constructed in this way has functions, because verbal events have functions.

Suppose a person formulates a rule of the sort “I want to have X in my life. If I do Z then I am moving toward X.” That kind of formulation does not have a behavioral impact mechanically. For an RFT point of view, both the fact that it occurred at all and whether it has behavioral regulatory functions is controlled by one’s past history and current context. This is superficially like the light–bar press–food relation situa-
tion, except that what one is placing in that future verbally is not anything that has to have been experienced previously.

The problem with the kind of assessment typologies that Wiegand and Geller talk about is that these theories do not explain the source of “motivation” nor its role in behavior change. They allow the analyst to predict behavior, but not to influence it. RFT provides an account of future oriented talk, and its role in the regulation of behavior in the form of manipulable contextual events. It also explains how to avoid the problems that verbal formulations create for human beings, even while using these methods. A detailed account of that takes us beyond this paper, but it has been put into writing in several places (e.g., Hayes et al., 1999; 2001) and a brief list of examples seems warranted.

RFT leads to the idea that reducing the negative impact of private events can usually best be done by changing the social/verbal context of these events rather than their content. One method is to attack the contexts that maintain powerful transformations of stimulus functions through derived relational networks: so called “cognitive defusion” methods. An example comes from Titchener (1916): take a word and say it over and over again very quickly. When you do so, the aversive and behavior regulatory functions of that work plummet (Masuda et al., 2004), even though the word continues to occur and its form did not change. Another method is to attack the rule-based system maintained by the culture that indicates that aversive private events have to “go away” in order for effective behavior to occur (i.e., if you feel good you will then live well): what we call “acceptance” interventions. While diminishing these language functions, RFT suggests that we might want to augment language functions related to long term goals and values. Positive concrete goals are things that can be obtained as an object. We use these in our applied methods, but only when they are linked to life directions that can only be instantiated, never achieved as an object. For example, the goal of getting a degree might be linked to the value of contributing to the welfare of others. By doing so, the “post reinforcement pause” of concrete goals is avoided: no matter how much of a contribution one has made, there is always more to make.

This is a powerful way of constructing a future. The combination of all three methods is also helpful. For example, “fear of failure” is usually actually a fear of what one will feel in association with failure. Acceptance methods can greatly and quickly reduce the behavior regulatory impact of such a fear. Without acceptance and defusion, values work is more fearful because there is more at stake, so there is a tendency to avoid even thinking about what one values. Thus, RFT helps
explain how and why you need to both build and diminish language functions to create effective behavior. A wide variety of acceptance, defusion, and values interventions (among other interventions that also flow from RFT, such as a focus on the present moment, or the development of a transcendent sense of self) have been developed and packaged under the name “Acceptance and Commitment Therapy” (Act: Hayes, Strosahl, & Wilson, 1999).

For present purposes, it might be more useful just to look at the outcome of such efforts in organizational settings. There is a growing body of evidence that RFT can be brought into organizational settings in a way that is helpful to OBM and acceptance, defusion, and values interventions have all been evaluated in organizational contexts. Bond and Bunce (2000) showed that interventions based on RFT reduced stress and increase behavioral innovations in the workplace more so than either a wait list control or a previously validated behavioral intervention specifically designed to increase innovations in the workplace. Furthermore, they found that this improvement occurred because acceptance and defusion interventions actually increased these processes. Bond and Bunce (2003) found that RFT derived measures of acceptance, defusion, and valued action correlated with objectively measured behavioral outcomes (key-stroke errors by call center employees working for a large financial institution) measured a year later, and did so better than measures of job satisfaction, or job control alone.

Dahl, Wilson, and Nilsson (2004) showed that these RFT based interventions have a dramatic impact on worker disability. After only a 4 hour intervention, over the next six months workers at risk for permanent disablement missed only three or four days of work cumulatively, versus nearly 70 days in the treatment as usual condition. Folke and Parling (2004) found a similar result for workers on sick leave due to depression.

Hayes, Bissett et al. (2004) found that RFT-based intervention with substance and alcohol abuse counselors resulted in significant decreases in their negative stigmatizing attitudes toward clients, and lower job burnout three months later. Importantly, this included higher levels of “sense of personal accomplishment” at work: an outcome measure that has been shown to be notoriously difficult to move in previous studies of employee burnout. Strosahl, Hayes, Bergan, and Romano (1998) showed that training in RFT based methods produced behavioral health workers who were objectively more effective at work as measured by the cost, amount of time spent working with clients, and ratings of clients of their degree of positive outcomes.
A comprehensive review of this work goes beyond the scope of the present paper, but these organizational studies have been published in major IOP journals such as the Journal of Applied Psychology, as well as in major interventions journals such as the Journal of Consulting and Clinical Psychology or Behavior Therapy. Yet all of these studies are clearly behavior analytic and they are explicitly based on contemporary basic behavior analysis in the form of Relational Frame Theory.

**Post-Skinnerian OBM**

If behavior analysis is not busy being born, it is busy dying. If organizational behavior management is to remain true to its roots but grow conceptually it must develop technologies based on contemporary work in basic behavior analysis itself. The work we have reviewed seems to show that behavior analysts do not need to join the mainstream to impact on the mainstream. The authors in this series who claim that direct contingency principles are not enough to guide OBM into the future are surely correct. But what is needed is what has been needed right along: more effective development and use of behavior analysis. This does not mean behavior analysis circa 1965; it means attending to what has actually been shown empirically in the laboratory over the last decade or two. What is there is a new form of behavior analysis, but it is behavior analysis nonetheless.

RFT is not the only alternative. Let all flowers bloom. And it is admittedly difficult to master contemporary basic behavior analytic accounts like RFT (did someone promise that science would be easy?). But it seems very sad and even self-destructive to have behavioral psychologists argue for the value of hoary non-behavioral field like achievement motivation and personality typologies before spending the effort to master contemporary work in their own basic field.

The sub-title of the RFT book is “A post-Skinnerian account of human language and cognition.” We called it post-Skinnerian both to recognize it as a form of behavior analysis, the field he created, and to indicate that we believed that this field needs to develop beyond the specific ideas of its founder. It needs to do so in a coherent, careful way that accords with the philosophy of science that underlies behavior analysis. Without that development we cannot achieve the visions of the founder of behavior analysis as expressed by the quotes in the first sentence of this paper. Said in another way, we cannot be true to the Skinnerian vision unless we become post-Skinnerian behavior analysts. I think these
papers as a set could be entered as exhibit one in an argument of that kind. OBM’s future as a behavior analytic enterprise will require linkage to ideas and principles that B. F. Skinner never dreamed of. This is as it should be. But those ideas and principles need to come from a coherent base rather than foreign philosophies, theories, and principles. They need to come from contemporary behavior analysis itself.

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