

Climbing Our Hills: A Beginning Conversation on the Comparison of Acceptance and Commitment Therapy and Traditional Cognitive Behavioral Therapy

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The history and developmental program of acceptance and commitment therapy (ACT) and relational frame theory (RFT) is described, and against that backdrop the target article is considered. In the authors' comparison of ACT and traditional cognitive behavioral therapy (CBT), traditional CBT does not refer to specific processes, principles, or theories but to a tribal tradition. Framed in that way, comparisons of ACT and CBT cannot succeed intellectually, because CBT cannot be pinned down. At the level of theory, change processes, and outcomes, ACT/RFT seems to be progressing as measured against its own goals.

Key words: acceptance and commitment therapy, cognitive behavioral therapy, contextualism, mediational analysis, processes of change, relational frame theory. [*Clin Psychol Sci Prac* 15: 286–295, 2008]

Progressive scientific fields are defined by a demonstrably increasing ability to systematize knowledge over time, producing predictably beneficial outcomes through understood processes, and encompassing new and previously unanalyzed areas within existing theoretical approaches. In other words, progressive fields develop a body of observations, principles, concepts, and theories that are precise, broad in scope, useful, and systematic.

In empirical clinical psychology, the attempt to create a progressive field has been dominated over the last few decades by the construction of defined methods applied to defined syndromes in carefully controlled studies. Although that produces large amounts of data and supports the normal science efforts of an army of students and researchers, it does not necessarily lead to systematization of knowledge, successful principles of change, or

the ability to extend either into new areas. Tested techniques alone are precise, but they are neither broad in scope nor systematic. Without specific theory and principles, techniques can only be organized on the basis of assumptions, topography, purported focus, or brand names within the sociology of science and application.

Acceptance and commitment therapy (ACT; said as a single word) and its underlying basic research program in language and cognition, relational frame theory (RFT; Hayes, Barnes-Holmes, & Roche, 2001), have always been a part of cognitive behavioral therapy (CBT) writ large (Hayes, 1987; Hayes, Strosahl, & Wilson, 1999, p. 79) provided that CBT is cast so as to include modern behavior analysis. In order to avoid comparing CBT to itself, we will use “CBT” in this article to refer only to the more traditional wing the target article is discussing.

Acceptance and commitment therapy has recently gained sufficient visibility as to be the focus of articles, such as the target article (for another example, see Hofmann & Asmundson, 2008), which attempt to understand ACT in more traditional terms. So far, these efforts reveal more about the characteristics of the dominant paradigm within CBT than they do about ACT/RFT.

Although it may be true, as the target article notes, that “group comparisons tend toward amplification and dichotomization of differences between one’s own group and an outside group” (Arch & Craske, 2008, p. 263), the history of science also shows that dominant paradigms often make it difficult to perceive intellectually important distinctions that arise (Kuhn, 1962). The CBT tradition itself has had rich contact with that process. In the earliest days of behavior therapy, psychoanalysts claimed that behavior therapy was nothing new except that it was psychotherapy done badly (Schraml & Selg, 1966). In the earliest days of the rise of traditional CBT, traditional behavior therapists often claimed that a cognitive model added nothing because behavior therapy already included an adequate analysis of cognition (Wolpe, 1980).

The reason this occurs is the almost invisible attitude that comes along with intellectually dominant paradigms, namely, that the purposes, categories, assumptions, and strategies of the dominant paradigm provide the best metric by which to best evaluate events within a discipline. When fundamental changes occur, however, usually what is changing are these self-same purposes,

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categories, assumptions, and strategies—precisely the things that cannot be seen inside that presumption. Metaphorically, it is like a team playing “king of the mountain” that is used to maintaining its position atop the seemingly most important hill, now trying to deal with those climbing another hill that is unexpectedly thought to be important.

The core of traditional behavior therapy was the assumption that the most important clinical task was direct behavioral and emotional change created through the application of direct contingencies or associative behavioral principles. With the advent of traditional cognitive models, CBT reorganized itself around the assumption that it was necessary to change thoughts and feelings in order to change behavior. Within this wing, psychopathology is still thought to be “maintained or exacerbated by exaggerated or biased ways of thinking. The therapist’s role is to help the patient recognize his or her idiosyncratic style of thinking and modify it through the application of evidence and logic” (Leahy, 2003, p. 1). Over 25 years ago, ACT theorists rejected both of these assumptions, adopting a view that thoughts and feelings do not cause behavior, but that they were nevertheless critically important in the context of a social/verbal community that linked these private events to overt action (Hayes, 1987; Hayes & Brownstein, 1986).

We conducted a series of empirical pieces on cognitive methods, finding that they do indeed work, but only in particular social and verbal contexts. Eight such studies were published from my research team between 1983 and 1985. For example, we found that coping statements work, but only if the client knows that the therapist knows the statement (e.g., Rosenfarb & Hayes, 1984). We found in a component analysis of cognitive therapy that distancing was a key feature, but as we suspected it did not work through cognitive change but rather through the behavioral process of helping patients “make more effective contact with the natural consequences surrounding pleasant activities” (Zettle & Hayes, 1987, p. 951). We constructed ACT to see what would happen if we focused on changing the social and verbal contexts that we believed linked thoughts and feelings to overt action, and then to integrate behavioral methods into that new context. ACT was initially called comprehensive distancing¹ to reflect that focus, but a review of early

ACT protocols (e.g., Zettle, 1984) shows that a variety of acceptance, defusion, and mindfulness methods were employed that went far beyond Beck’s (1976) specific approach to distancing.

In addition to an open trial with anxiety disorders (Hayes, 1987), we conducted three very small studies comparing ACT to CBT protocols of the day, in the areas of depression (Beck, Rush, Shaw, & Emery, 1979), pain (Turk, 1978), and weight control (Brownell, Heckerman, Westlake, Hayes, & Monti, 1978). All three studies showed different processes of change, and two showed better outcomes. Having convinced ourselves that an alternative model might succeed, we described it (Hayes, 1984, 1987), and stopped doing outcome research. Parts of the depression study were published quickly (Zettle & Hayes, 1986); the pain study was published 16 years later, after our development work was finished (Hayes, Bissett, et al., 1999); the weight protocol was never published but, instead, was revised; and data regarding it are currently under review (Lillis, Hayes, Bunting, & Masuda, under review).

We did not rush these data out the door because challenging CBT was not the point. ACT was being compared to the best available approaches to reassure ourselves that we were on a useful path, but our purpose was to create a scientifically progressive program. We spent 15 years attending to that task before returning to outcome research.

THE ACT/RFT DEVELOPMENT PROGRAM

The development strategy we pursued is an elaboration of the inductive, functional approach that characterizes behavior analysis. We term it a “contextual behavioral science” approach, because it deviates in some ways from traditional behavior analysis. Very briefly, it involved these steps.

Explication of Philosophical Assumptions

We believed that it was critical to be clear about our assumptions. ACT is based on a pragmatic philosophy called *functional contextualism* (Biglan & Hayes, 1996; Hayes, 1993; Hayes, Hayes, & Reese, 1988). Much of what is unique about ACT can be traced back to its philosophical assumptions, which are more similar to constructivist approaches, such as feminist psychology, hermeneutics, dramaturgy, or social constructivism, than

to the realistic and, at times, mechanistic assumptions of the psychological and CBT mainstream, but with a different goal (Hayes, 1993) than most other contextualistic approaches (Hayes, Hayes, Reese, & Sarbin, 1993). Functional contextualists view truth as the incremental achievement of prediction-and-influence with precision, scope, and depth, and they assess any act of partitioning the one “real” world against that criterion (Hayes, 1993). As a result, all ACT concepts need to be linked to context, defined both historically and situationally, because only contextual variables can be directly manipulated and lead directly to both prediction and influence (Hayes & Brownstein, 1986). The ACT rejection of cognitive and emotional causality, the clinical emphasis on values, deemphasis of an interest in literal truth, and the emphasis on workability all flow from the radical pragmatism of its underlying philosophy. The assumptions of the dominant paradigms are different, which can at times make ACT hard to understand, especially if these different philosophical assumptions have not been owned and explicated.

Create a Basic Account That Continuously Informs Treatment and Vice Versa

Every hard-won and well-established behavioral principle is part of the theoretical armamentarium of ACT, but because no empirically adequate behavioral account existed of human language and cognition, one had to be created. RFT (Hayes et al., 2001) was the ultimate result. Over the last decade, RFT has been the most active area of basic human research in behavior analysis, and its findings have implications that go far beyond ACT. Indeed, if a CBT researcher were interested in producing cognitive change, RFT suggests some interesting ways to do it (see Hayes et al., 2001, pp. 228–230). The ACT emphasis on acceptance, defusion, contact with the present moment, and self in the sense of perspective taking can all be readily derived from RFT.

Create a Model of Psychopathology and Intervention Linked to These Principles

A set of abstractive theoretical terms (acceptance, defusion, a transcendent sense of self, experiential avoidance, values, psychological flexibility, and so on) was created and systematized into a formal model of psychopathology and treatment (e.g., Hayes, Bond,

Luoma, Masuda, & Lillis, 2006). There are now myriad books and articles on ACT that explain the model (e.g., Luoma, Hayes, & Walser, 2007). Much as a user-friendly operating system is, in turn, based on programming languages and machine code,² these middle-level concepts stand on the technical language of RFT and behavioral principles and make it possible for ACT clinicians to apply the analysis without being expert in the arcane world of basic contextual behavioral science.

Measure Processes of Change

Measures of ACT-related processes have received attention from the very beginning. For example, a measure of cognitive defusion was used in the first ACT study (Zettle & Hayes, 1986). Over two dozen measures have been developed and used (Ciarrochi & Bilich, 2007), from ratings of in-session interactions, to general and specific measure of acceptance or psychological flexibility, to values assessment.

Test Components Linked to Processes

A practical theory needs to tell clinicians which components are likely to manipulate specific processes. Instead of waiting for dismantling studies that may occur long after mistakes are well ingrained (e.g., Dimidjian et al., 2006; Jacobson et al., 1996), ACT components and processes have been tested in experimental psychopathology studies from the earliest stages of empirical development. This literature is now large, encompassing dozens of component studies on acceptance (e.g., Hayes, Bissett, et al., 1999), defusion (e.g., Masuda, Hayes, Sackett, & Twohig, 2004), values (Paez-Blarrinaa et al., 2008), self as context (Williams, 2007), and other ACT elements (Levin, Yadaivaia, Hildebrandt, & Hayes, 2007). So far, the individual methods and processes in ACT are psychologically active and work in ways that comport with ACT theory.

Show That Dissemination and Training Work

It is critical to show that concepts and methods are broadly useful because that is a key measure of “truth” within this perspective. Three effectiveness studies show that ACT training creates positive outcomes, as good as (Forman, Herbert, Moitra, Yeomans, & Geller, 2007) or better than CBT methods (Lappalainen et al., 2007), and through distinct processes of change.

Care About Mediation and Moderation as Much as or More Than Outcome

ACT researchers have embraced this goal from the beginning, as will be shown below.

Broad Outcomes

The first books on ACT (Hayes, Strosahl, et al., 1999) and RFT (Hayes et al., 2001) appeared after 15–20 years of development. Dozens of controlled outcome studies have appeared since, and in a strikingly broad number of areas (Hayes et al., 2006). Successful ACT outcome studies range from psychosis to smoking; from diabetes management to professional burn out; from prejudice to being more able to learn empirically supported treatments. Applied RFT studies are also now appearing (e.g., Berens & Hayes, 2007), expanding the overall research program.

WHAT IS CBT ANYWAY?

The strength of one method is not increased one iota by the weaknesses of another. ACT/RFT has its own hill to climb regardless of the success or failure of traditional CBT. But as ACT/RFT has progressed, authors within other parts of the CBT tradition have begun to argue that maybe ACT/RFT is nothing new, nothing different, or at least nothing better (Hofmann & Asmundson, 2008). The brief history that I have described leaves little doubt that the tradition, principles, basic science, model of scientific development, assumptions, breadth of application, and techniques of ACT/RFT are distinctive. That does not mean that the processes of change are unique or that this approach is “better” than anything else. These are empirical matters. Thus, I welcome the present article in its basic instincts, and I consider it a turn taken in what seems likely to be a multiyear conversation between two strands of the behavioral and cognitive therapies.

There is excellent work on ACT for anxiety (Eifert & Forsyth, 2005), but it does not seem wise to frame this beginning conversation there. The reason, offered by the authors for doing so, is that the work in CBT is well established there as compared to this “new approach” called *ACT*. *ACT* is not new, but the authors are correct to want to work out from what is known. Unfortunately, little is known about how traditional CBT works (Longmore & Worrell, 2007) and, in almost every area,

the target article speculates or refers to new findings to explain CBT change processes. Conversely, the processes underlying ACT are well specified, have been relatively unchanged for a decade or even two, and have received consistent empirical support. Because the firmer ground seems to be these data, and most of that lies outside of anxiety, I will answer more broadly.

Unfortunately, the ambiguities in traditional CBT make it difficult to frame a comparison regardless of the ground for it. You cannot compare a specific model to a scientific bowl of Jell-O. The target article never defines CBT, lists its putatively essential components or techniques, or notes long-established and well-understood theoretical processes underlying them. As presented in this article, CBT is not a specific theory, model, or even a specific set of methods, but a vast tradition or even a tribe, and tribal members are apparently free to propose new processes and then to hope that these too are now instantly part of the dominant paradigm. As a result, despite its even-toned approach, the target article (as with similar articles, such as Hofmann & Asmundson, 2008) is surprisingly speculative, almost as if it is traditional CBT that is newly on the scene.

The problem can be seen clearly in the authors’ discussion of cognitive restructuring. From an ACT perspective, cognitive restructuring in the sense of cognitive challenges has always been thought to be a risky overextension of a cognitive model (Hayes, 1987; Hayes, Strosahl, et al., 1999). We worried that people are so prone toward emotional and cognitive avoidance that almost any method designed to be helpful could evoke avoidance and risk paradoxical effects. The scientific issue was not that cognition cannot change, nor that cognitive change cannot be helpful, nor that cognitive therapy is the same as thought suppression. It was that cognitive disputation and direct efforts at cognitive change could inadvertently strengthen the literal, reasoning, and avoidant social/verbal contexts that surround private events. We preferred, instead, to focus therapeutically on contexts that would weaken unhelpful cognitive and emotional regulation of behavior.

The authors note correctly that acceptance and defusion could also evoke suppressive and avoidant reactions. This is precisely why ACT has so many methods designed to prevent and to detect this possible process (e.g., the “creative hopelessness” phase of ACT; learning to detect

experiential avoidance and cognitive fusion in session—see Luoma et al., 2007, for detailed examples). If that is true with acceptance or defusion, however, consider how much more of a worry it might be with direct cognitive challenges. Given that cognitive methods can sometimes produce poorer outcomes than alternatives (e.g., Dimidjian et al., 2006), this worry is not empty speculation.

In the authors' defense of cognitive restructuring, the authors first point to some data on the positive impact of cognitive reframing and then use this evidence to speak to the supposed utility of "monitoring, stating, and *challenging* threat-related cognitions" (Arch & Craske, 2008, p. 266; emphasis added). This is not an adequate defense and it sidesteps the essence of the traditional ACT concern. ACT includes a large number of elaborative cognitive methods, including many of its defusion and acceptance techniques. Such methods can be helpful if they encourage psychological flexibility and not avoidance (e.g., Kashdan, Barrios, Forsyth, & Steger, 2006). Disputing and challenging cognition requires much more of an empirical and conceptual defense. The authors provide one, but it is a shocker: These cognitive methods are defensible, because they "may function as a form of exposure" (Arch & Craske, 2008, p. 266).

Used in that way, exposure is not a principle, process, or method. It is just a word for contact with something. So viewed, every psychotherapeutic method involves exposure. Anything that applies to everything explains nothing. You cannot explain the effects of cognitive challenges by exposure until exposure itself is delineated and explained. The authors sense the problem and later try to explain exposure, but, instead, they show how little the initials "CBT" mean anymore. Noting that they have recently discovered that fear reduction within exposure trials is not predictive of overall outcome, they reach the conclusion that fear reduction is not the central process in exposure therapy. Citing two articles published within the last year, the authors state that the new "CBT" explanation is that exposure is about "optimizing learning . . . based on increasing tolerance for fear and anxiety" (Arch & Craske, 2008, p. 269).

Acceptance and commitment therapy will not differ from CBT accounts if the latter are changed to agree with decades-old ACT positions. Compare this explanation of exposure to the first presentation of ACT (Hayes, 1987), which said that it utilizes "traditional behavioral

techniques" that are adapted to fit into "a contextual approach to private experience" (p. 365), adding that the "exposure work, however, is not designed to reduce anxiety. Instead, exposure gives people an opportunity to practice experiencing anxiety without also struggling with anxiety" (p. 365).

I am not saying that ACT is the specific source of these particular changes, although to some degree ACT probably helped move the CBT Zeitgeist over the decades. Instead, these are generational changes occurring in every wing of CBT, which is precisely what justifies calling them "third wave" (Hayes, 2004) or any of the other euphemisms that have arisen ("acceptance or mindfulness-based CBT," and so on). But my colleagues do not seem to appreciate the implications of the changes they are proposing for traditional CBT, and the intellectual incoherence that can come if any new idea is instantly part of CBT merely because an important tribal member utters it.

I picked up the most recent comprehensive presentation of CBT that I could find (Farmer & Chapman, 2008; an excellent book, by the way, and one that gives ACT and other third wave interventions extensive coverage). It defines exposure as "a term used for several related procedures that reduce unwanted emotional responses by exposing the client to the stimuli that elicit these responses" (p. 229). Let us put aside the circularity of the definition ("exposure" is defined as "exposing"), and the inclusion of a desired effect of a procedure in the definition of that procedure, thus making its impact a tautology. It is hard to criticize authors for poorly defining a term that no one else has adequately defined either. Focus instead on the fact that emotional reduction is in the definition of exposure within the CBT tradition, circa 2008—the very effect the authors of the target article now say is not particularly important to the therapeutic impact of "exposure." Most CBT practitioners would be surprised to read of the authors' new "CBT" interpretation of exposure. ACT practitioners would find the formulation familiar to the point of boredom. Nevertheless, the authors are placing their ideas inside traditional CBT and are then comparing them to ACT. The questions fairly scream from the page: If this is traditional CBT, what the heck is traditional CBT? Is it a theory or a tribe? If it is a theory, what is that theory? If it is a tribe, why is this article being written?

The problem is not one of who should get credit, or who was there first. Such matters are trivial. It is also not an issue of labels. If others want to tauntingly call ACT “Another Cognitive Therapy” or “old hat” or “Uncle Fred the Wonder Slug,” it has no impact on the real issue, which is how to create the kinds of theories and models that lead to a more progressive scientific discipline.

In the history of science, all theories are ultimately shown to be incorrect. So far, this is without exception, and there is no reason to suppose that will be different in the future. It is best to reach this stage quickly and with precision so that the field can move ahead, but for that to occur, theories need to be clear, systematic, and linked to manipulable basic processes. ACT/RFT has tried to do that. For sociological purposes, it is fine to allow loose collections of methods disconnected from processes and theories to live under a tribal label, at least for a time. But it is not fine to allow traditions and tribes to substitute for careful, systematic theorizing and the development of knowledge about manipulable psychological processes linked to specific methods.

That appears to be what is occurring here. Consider the ripple effects from the authors’ new approach to exposure. Many researchers in CBT have an interest in emotional arousal, emotional regulation, physiological measures of emotion, emotion centers of the brain, and so on, as key features of psychopathology. All of these now need to be addressed in a new way if the focus is shifted from the occurrence, frequency, or intensity of emotions, such as fear to “tolerance for fear.” ACT theorists took the time to learn how to walk that path with integrity. Traditional CBT has not yet done so. The vast army of CBT colleagues and students will take many years (if ever) to adjust to the new, official position on exposure that these authors propose. Meanwhile, the inconsistencies that this change produces will need to be detected and weeded out one by one.

Let me give an example. Elsewhere in the article the authors emphasize that “physiological measures may be highly useful” in the comparison of ACT concepts and CBT concepts. Why, if the real action is not fear but “tolerance for fear”? Do they suppose that the now key process of “tolerance” can be measured by directly examining physiological measures? They could mean that it is useful to look for the desynchrony between physiological, attitudinal, behavioral, and cognitive

measures that can occur when the social/verbal contexts that produce synchrony are changed through acceptance, defusion, and mindfulness (e.g., see Levitt et al., 2004). But then, why raise skepticism about self-report in the same paragraph where physiological measures are called for? It seems what was meant was that real emotion, objectively measured, will best distinguish methods that treat “emotional disorders.” That is, indeed, a traditional CBT assumption, but it directly conflicts with the idea that tolerance of emotion is the key process. I believe that this section simply shows that the implications of abandoning fear reduction have not yet been fully faced by the authors. They are vast.

Consider other such inconsistencies. If it is the client’s relationship to emotion, not emotion itself, that is now the key process issue, why is it not also the client’s relationship to cognition and not cognition itself that is the key in that area? And if all of that is embraced, how is that still traditional CBT and not the very essence of the third-generation CBT, which has been aptly characterized this way, as it applies to Mindfulness-based Cognitive Therapy (MBCT) but could be said as well of all third-generation treatments: “Unlike CBT, there is little emphasis in MBCT on changing the content of thoughts; rather, the emphasis is on changing awareness of and relationship to thoughts, feelings, and bodily sensations” (Segal, Teasdale, & Williams, 2004, p. 54). Should “traditional” CBT make these moves, and throw in a dash of mindfulness and a dollop of values work, technologically, as Darth Vader would say, “the transformation will be complete.”

ACT AND CBT PROCESSES

The authors walk through a number of comparisons of ACT and CBT. Because of how I have chosen to structure my reply, I have little room to speak to the specific issues beyond the few I have chosen already. There are some good ideas in here for ACT researchers, though to be honest, as with the risk of suppression following acceptance interventions, most are already on the table. There are a number of misunderstandings of ACT/RFT and comparisons that do not seem apt as well, but I feel confident that there will be years ahead to address this list as part of an ongoing conversation within the field. That is especially true given the most common word in these comparisons: the word “may.” An example (one of many) is “cognitive restructuring and cognitive defusion both . . . may serve

to reduce ‘experiential avoidance’” (Arch & Craske, 2008, p. 267). Who would want to argue with that word “may”?

It might also be useful, however, to start with what we know. We know that acceptance, defusion, and mindfulness methods reduce experiential avoidance (Hayes et al., 2006), and we have some indication that lower experiential avoidance empowers exposure (Levitt et al., 2004). We also know that meta-analyses have consistently found that “exposure with and without cognitive modification are equally effective” (Feske & Chambless, 1995, p. 712). I have a hard time putting these facts together in a way that suggests to me that challenging cognitions is a powerful method of reducing experiential avoidance.

We also know that self-described ACT and traditional CBT clinicians recommend different techniques (Storaasli et al., 2007). We know that ACT and CBT sessions are rated as fairly different by objective observers (Forman, Herbert, & McGrath, 2007). We know that politically liberal clients, looking at ACT and CBT tapes, prefer ACT and think it will help; political conservatives prefer CBT and think it will not help (Meyer & Chow, 2003).

We know that acceptance, defusion, and values mediate ACT outcomes quite consistently (Hayes et al., 2006). In a recent presentation on this topic (Hayes, Levin, Yadavaia, & Vilardaga, 2007), my colleagues and I were able to locate 12 studies of ACT outcomes with mediational analyses that were either published, in press, or being written, and we could get the dataset. Pre-to-post changes in ACT processes (or for a few studies with no post data, pre to follow-up changes) accounted for nearly half of the pre to follow-up changes in outcome produced by ACT. The comparison conditions in the set included CBT, pharmacotherapy, psychoeducation, supportive treatment, and wait list controls.

We know that ACT has so far been compared to traditional methods nine times in the published literature (Bond & Bunce, 2000; Forman, Herbert, et al., 2007; Forman, Hoffman, et al., 2007; Hayes, Bissett, et al., 1999; Lappalainen et al., 2007; Masedoa & Esteve, 2006; Zettle, 2003; Zettle & Hayes, 1986; Zettle & Rains, 1989) and all have found differences in processes of change. All but one showed a difference in outcome (and in all but one of those were in favor of ACT) and some (e.g., Zettle & Hayes, 1986, as reanalyzed in Hayes et al.,

2006) show formal differences in the mediators of outcome using mediators assessed before outcomes are significantly different.

Since Hayes, Strosahl, et al. (1999), there have been over 40 outcome or component studies published on ACT, and more than that on RFT. Considering the body of evidence as a whole, ACT/RFT seems to be progressing well as measured against its own goals, which in themselves seem both bold and somewhat distinct. The actual evidence, so far, suggests that ACT is not the same as comparison treatments, even though there are many areas of overlap with other treatments (including not just CBT, but also humanistic, existential, and analytic traditions as well). The overlap with the “B” part of CBT is obvious and should be so given that ACT/RFT is part of behavioral psychology and that “during the latter portions . . . ACT takes on the character of traditional behavior therapy, and virtually any behavior change technique is acceptable” (Hayes, Strosahl, et al., 1999, p. 258).

CONCLUSION

The claims of intellectual and empirical progress that ACT/RFT theorists make may be correct or incorrect, but they were anything but hasty and divisive. I welcome the comparisons that are emerging, but I also caution to others that ACT/RFT has become a substantial area that takes time to explore and that can readily be mischaracterized because it comes from a stream of thought that many long ago presumed to be dead or at least moribund. If ACT/RFT continues to progress, there will be time to work through this conversation within the field in a careful and considered way.

In that regard, the tone of the target article is especially welcomed. In order to promote scientific progress, ACT has taken a strong, clear stance that can be proven right or wrong. But that strong tone is not animus. Twenty years ago, I tried to cast ACT as an approach that “transcends the distinctions between behavior therapy and cognitive therapy” (Hayes, 1987, p. 342), rather than an attack on either. In my Association for Behavioral and Cognitive Therapies (ABCT) presidential address (Hayes, 2004), I stated that this new wave of treatments in CBT more generally should not be seen as hostile to CBT because this change “reformulates and synthesizes previous generations of behavioral and cognitive therapy and carries them forward into questions, issues, and domains

previously addressed primarily by other traditions, in hopes of improving both understanding and outcomes” (p. 658). ACT theorists can disagree about the core conceptions of traditional CBT and still stay in a working alliance that honors CBT and the empirical clinical tradition. Each generation in a progressive scientific field stands on the shoulders of giants, but precisely in order to do justice to their sacrifices and contributions, each generation should be emboldened to see how far they can reach and how much they can grasp.

NOTES

1. Everything in comprehensive distancing remains in ACT today. What has been added was added early and clearly did not come from traditional CBT, so despite the name change this early work has always been viewed just as early ACT. The history of ACT/RFT (Zettle, 2005) shows that we decided to take the time to develop an alternative path only after determining that it seemed different than what was then present, so the use of “distancing” as a name was never meant to indicate that ACT was merely a component of traditional CBT.

2. I thank my colleague Takashi Muto for the analogy.

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Received December 10, 2007; accepted December 20, 2007.