Processes of Change in Acceptance and Commitment Therapy and Cognitive Therapy for Depression: A Mediation Reanalysis of Zettle and Rains

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Processes of Change in Acceptance and Commitment Therapy and Cognitive Therapy for Depression: A Mediation Reanalysis of Zettle and Rains

Robert D. Zettle, Jeanetta C. Rains, and Steven C. Hayes

Abstract

Several articles have recently questioned the distinction between acceptance and commitment therapy (ACT) and traditional cognitive therapy (CT). This study presents a reanalysis of data from Zettle and Rains that compared 12 weeks of group CT with group ACT. For theoretical reasons, Zettle and Rains also included a modified form of CT that did not include distancing, and no intent-to-treat analysis was included. Particularly because that unusual third condition did somewhat better than the full CT package, it contaminated the direct comparison of ACT and CT, which has of late become theoretically interesting. In the present study, data from participants in the ACT and

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CT conditions were reanalyzed. ACT was shown to produce greater reductions in levels of self-reported depression using an intent-to-treat analysis. Posttreatment levels of cognitive defusion mediated this effect at follow-up. The occurrence of depressogenic thoughts and level of dysfunctional attitudes did not function as mediators. This study adds additional evidence that ACT works through distinct and theoretically specified processes that are not the same as CT.

Keywords
acceptance and commitment therapy, cognitive therapy, depression, defusion, mediation

Acceptance and commitment therapy (ACT; Hayes, Strosahl, & Wilson, 1999) is a specific model of human suffering and intervention, linked to a specific theory, basic research program (Hayes, Barnes-Holmes, & Roche, 2001), and philosophy of science (Hayes, 1993). Although ACT is one of the modern members of the cognitive-behavior therapy (CBT) family of interventions, models, and theories (Hayes et al., 1999), its model is said by its originators to differ from traditional CBT.

As ACT has become more popular and its empirical base has grown (Hayes, Luoma, Bond, Masuda, & Lillis, 2006), it has attracted commentaries and criticism from CBT theorists. One of the recent themes is that ACT is no different than traditional CBT (Arch & Craske, 2008; Hofmann & Asmundson, 2008) and that it is no better than active treatment comparisons (Powers, Vörding, & Emmelkamp, 2009). For example, Arch and Craske (2008) characterize ACT as a “new therapy” engaging in the “amplification and dichotomization of differences” (p. 263). Similarly, the title of Hofmann and Asmundson’s (2008) criticism asks whether ACT is “old hat.”

Comparing treatments at the level of processes of change is a useful approach in determining whether interventions are distinct from each other (O’Donohue & Yeater, 2003). Both critics and supporters of ACT as a distinct treatment model agree on that point. For example, Arch and Craske (2008) stated that “at the heart of the discussion of similarities and differences between ACT and CBT is the need to investigate the processes or mechanisms by which these two therapies produce change” (p. 271).

There are several possible differences between an ACT model and a traditional CBT or cognitive therapy (CT) model, but perhaps one of the most useful places to focus is on whether changing the function of thoughts and feelings are key to ACT as compared with a change in the form or occurrence
of thoughts and feelings in a traditional CBT model. This is an important area because traditional CBT and CT theorists have emphasized the importance of changes in cognitive content. Beck (1993), for example, has argued that “cognitive therapy is best viewed as the application of the cognitive model of a particular disorder with the use of a variety of techniques designed to modify the dysfunctional beliefs and faulty information processing characteristic of each disorder” (p. 194). Clark (1995) has stated that the core of CBT is to “identify distorted cognitions” and to subject them “to logical analysis and empirical hypothesis-testing which leads individuals to realign their thinking with reality” (p. 155). Conversely, ACT theorists argue that direct attempts to alter and especially to diminish cognitive or emotional content can at times be unhelpful (Hayes et al., 1999) and that it is “often safer to create more flexible responding by diminishing the excessive impact of cognitive events than trying to correct their content” (Hayes, Levin, Plumb, Boulanger, & Pistorello, in press, p. 10).

A small number of studies have examined these differences empirically. Forman and his associates (Forman, Herbert, Moitra, Yeomans, & Geller, 2007) as well as those of Lappalainen (Lappalainen et al., 2007) found differences in processes of change in ACT versus traditional CBT but neither performed a formal mediation analysis. This is important, as critics have noted,

Future research will need to examine whether ACT techniques target different mediators of treatment change than certain CBT techniques. For this purpose it will be important to conduct studies directly comparing ACT and CBT strategies in conjunction with conducting formal mediation analyses. (Hofmann & Asmudson, 2008, p. 284)

Because ACT research has had a process focus from the beginning, the two earliest ACT randomized trials (Zettle & Hayes, 1986; Zettle & Rains, 1989) fit the criteria for Hofmann and Asmudson’s challenge. Mediation analysis was not well known at the time Zettle and Hayes (1986) first published, but a reanalysis of their small randomized trial of individual ACT and CT for depression has been conducted (Hayes et al., 2006). In line with ACT theory, it showed that the follow-up outcome differences between CT and ACT (in favor of ACT) were mediated by earlier reductions in the believability of depressogenic thoughts (used as a measure of cognitive fusion) but not the occurrence of these thoughts.

The second early study that compared ACT (then called “comprehensive distancing”) and CT was that by Zettle and Rains (1989). This study contained
three treatment groups: (a) ACT, (b) CT, and (c) a modified form of CT that included no cognitive-distancing elements. This was a logical step at that time to better understand the results from Zettle and Hayes (1986). If distancing was the component of key applied importance in cognitive-behavior therapy (CBT), and “comprehensive distancing” was nothing more than an extreme form of distancing as used in CT, then one would expect that ACT might have the best outcomes, then CT, and then a form of CT without distancing. That is not what was found. In terms of treatment outcome on the Beck Depression Inventory (BDI; Beck, Ward, Mendelson, Mock, & Erbaugh, 1961), ACT produced the better outcomes, followed by CT without distancing, and then CT, suggesting that ACT was not just distancing as it was conducted in CT because that form of distancing apparently did not add to CT outcomes. The statistical significance of these differences was only marginal ($p = .1$), and BDI outcomes were not interpreted in the original study.

In the context of the present day, of more interest, however, are the differences between ACT and the full CT package. Inclusion of the modified CT condition clouded that comparison. Furthermore, an analysis of covariance was used to evaluate outcomes with dropouts not included in the analysis, thus not meeting the modern expectation for a full intent-to-treat analysis using statistical methods that can properly adjust for the impact of missing data. Finally, mediation analyses were also not conducted.

The present reanalysis corrects these problems by focusing exclusively on the ACT versus CT comparison, using more modern statistical approaches and an intent-to-treat analysis and by considering whether differences in processes of change were functionally related to outcome using formal methods of mediation analysis.

**Method**

**Participants and Assignment**

Volunteer female participants were recruited through announcements in local media. Only women were accepted as participants to ensure more homogeneous groups. Before they began treatment, all participants were required to verify that they were not taking antidepressant and/or tranquilizing medications. A total of 80 women presented themselves as potential participants. A total of 45 women met initial selection criteria; 8 declined to participate or dropped out before they had completed baseline assessments and before
they were aware of their treatment assignments. Participants randomly assigned to a modified CT protocol \((n = 12)\) were not considered in this reanalysis. The remaining 25 participants were randomly assigned either to CT \((n = 13)\) or to ACT \((n = 12)\).

### Selection Criteria

To be included in the study, participants were required to be experiencing moderate to severe depression by meeting all of the following criteria: (a) a score of 20 or above on the BDI (Beck et al., 1961), (b) a \(T\)-score of 70 or greater on the Depression scale of the Minnesota Multiphasic Personality Inventory (Hathaway & McKinley, 1942), and (c) a score of 14 or more on the 21-item version of the Hamilton Rating Scale for Depression (HRS-D; Hamilton, 1960) completed by an independent evaluator blind to treatment condition. One third of the interviews were scored by a second independent evaluator blind to treatment and interrater agreement was good, \(r = .89, p < .001\). More detailed characteristics of the sample are presented in Zettle and Rains (1989).

### Measures

Only the primary outcome and process measures needed for the reanalysis will be examined here (see the original report for other measures). The BDI, a 21-item self-report instrument that is one of the most widely used and best validated measures of depressed symptomatology (Beck, Steer, & Garbin, 1988), was considered the primary outcome measure. An important advantage of the BDI in this relatively small study is that it was obtained at pretreatment, after each treatment session, posttreatment, and at a 2-month follow-up, so that at least some data were obtained after treatment began, thus maximizing the sensitivity of treatment comparisons.

Two measures were used to assess therapeutic processes: the Automatic Thoughts Questionnaire (ATQ) and the Dysfunctional Attitude Scale (DAS). The ATQ is a 30-item questionnaire designed to assess the occurrence of negative thoughts associated with depression in immediate awareness (Hollon & Kendall, 1980). The frequency of occurrence of each thought is rated on a 5-point scale, which yields total scores of 30 to 150. A recent review of mediational studies on CT found that the ATQ has been widely used in the assessment of depressogenic thoughts that are typically targeted in CBT and as a process measure in cognitive-therapy research (Garratt, Ingram, Rand, & Sawalani, 2007).
ACT researchers have long used a second rating for the ATQ, asking participants to rate each thought on its believability given that it occurred, also using a 5-point scale. This measure is termed the ATQ-B and, as in Zettle and Hayes (1986), was the primary ACT process measure used. Psychometric evaluation of the ATQ-B (Zettle, 2010) shows that it has good internal stability in both clinical ($n = 177$) and nonclinical ($n = 249$) populations (Cronbach’s $\alpha = .95$ and .97, respectively). Test–retest reliability over 3 months with a nonclinical sample is .85. The ATQ-B correlates significantly with the BDI for both populations ($r = .53$ and .58, respectively), providing evidence of the measure’s construct validity. Thus, the psychometric properties of the ATQ-B appear to be adequate for use as a process measure.

The DAS consists of 40 attitudes identified by experienced clinicians as most characteristic of depression (Weissman, 1979). Respondents indicate their agreement with each attitude on a 7-point scale, which yields total scores of 40 to 280. In contrast to the ATQ, which surveys depressive thoughts at the level of immediate awareness, the DAS is designed to assess depressogenic beliefs that occur at a higher level of cognitive organization. The DAS has been very widely used as a process measure in CT (e.g., DeRubeis et al., 1990).

**Procedure**

Each group ($n = 4$ to 7 members each; two groups per condition) met for 12 weekly 90-min sessions. The general format of each treatment session included reviewing assigned homework, group presentation and discussion of new treatment material, and assignment of new homework. On average, participants attended between 10.5 (ACT condition) and 11.2 (CT condition) sessions. All participants who did not dropout attended at least 8 of the 12 sessions and verified that they remained free of psychotropic medication for the duration of the study.

The first author was the primary therapist in both conditions. He had previous training in CT as a psychology intern at the Center for Cognitive Therapy in Philadelphia under the supervision of Aaron Beck, originator of cognitive therapy, and also had training in ACT as a graduate student receiving his PhD in clinical psychology under the supervision of Steven C. Hayes, originator of ACT. So far as we know, no one else has received this level of training in both approaches, and thus the present data provide an especially worthwhile data set for reanalysis.

To assess the presence of any potential bias or other nonspecific effects that might favor one condition over others, after each treatment session all
participants completed a questionnaire adapted from Rose (1984; obtained \( \alpha \) in the present study = .85) that asked participants to rate their involvement in the group and their evaluation of the session. A repeated measures analysis of variance showed no significant difference between the two treatment conditions.

**CT package.** This treatment condition followed guidelines outlined by Hollon and Shaw (1979) in presenting a full complement of therapeutic procedures and strategies common to CT (Beck, Rush, Shaw, & Emery, 1979). Distancing, cognitive restructuring, and behavioral hypothesis-testing were incorporated as part of treatment. Distancing procedures included the use of similes, reattribution techniques, and “alternative conceptualizations” as outlined by Beck et al. (1979). Self-monitoring homework was assigned to identify depressive thoughts for cognitive restructuring. Group discussion, in which turns were taken in focusing on individual group members, was used to evaluate both past and present evidence relevant to the validity of depressive beliefs. Behavioral hypothesis-testing focused on low frequency, highly pleasurable activities as assessed by the Pleasant Events Schedule (MacPhillamy & Lewinsohn, 1972). Participants were asked to verbalize thoughts that interfered with their engagement in such activities (e.g., “I wouldn’t have a good time anyway,” “It wouldn’t turn out right,” etc.). Participants were then guided in designing tests of such thoughts and were asked to carry them out for homework.

**ACT.** This treatment condition followed the early ACT protocols (Hayes, 1987), which are very similar to later ones (e.g., Hayes et al., 1999) except that values work was embedded in other themes. A series of didactic and experiential exercises emphasized several major themes. The first theme was that deliberate efforts to control or change depressive thoughts can be counterproductive in that such attempts often evoke the very thoughts to be eliminated. A second theme was that depressive thoughts that participants offer as reasons or explanations for dysfunctional actions are themselves merely behavior and not valid “causes” for such behavior. A final theme was that if participants react to their depressive thoughts and feelings as “behavior in context” by merely describing and identifying them, it becomes possible for them to behave more effectively without struggling with the continued presence of negative thinking. To underscore this point, participants were assigned homework that involved engaging in pleasant activities, as in the CT group. However, the purpose of homework assignments were said to be to provide participants with experience in engaging in valued activities while they were having thoughts that normally would prevent them from doing so.
**Check of Treatment Integrity**

In the original study, audiotapes of one third of the sessions were reviewed by two judges familiar with treatment manuals but blind to each group’s treatment condition. The judges were able to classify correctly 21 of 24 tapes ($p < .001$).

**Results**

Pretreatment, posttreatment, and follow-up means and standard deviations for all measures are shown in Table 1. Pretreatment differences in all measures were examined using $t$ tests, none of which were significant ($p > .1$).

### Outcome Analysis

Although differences in mediation can technically be examined without a significant treatment difference, most mediation analyses are based on a significant

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**Table 1. Means and Standard Deviations of Measures by Treatment Condition**

<table>
<thead>
<tr>
<th>Measure</th>
<th>CT</th>
<th>ACT</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>$M$</td>
<td>$SD$</td>
</tr>
<tr>
<td>BDI</td>
<td>Pretreatment</td>
<td>26.90</td>
</tr>
<tr>
<td></td>
<td>Posttreatment</td>
<td>16.20</td>
</tr>
<tr>
<td></td>
<td>Follow-up</td>
<td>12.90</td>
</tr>
<tr>
<td>ATQ</td>
<td>Pretreatment</td>
<td>87.30</td>
</tr>
<tr>
<td></td>
<td>Posttreatment</td>
<td>72.60</td>
</tr>
<tr>
<td></td>
<td>Follow-up</td>
<td>69.10</td>
</tr>
<tr>
<td>ATQ-B</td>
<td>Pretreatment</td>
<td>87.50</td>
</tr>
<tr>
<td></td>
<td>Posttreatment</td>
<td>70.10</td>
</tr>
<tr>
<td></td>
<td>Follow-up</td>
<td>66.80</td>
</tr>
<tr>
<td>DAS</td>
<td>Pretreatment</td>
<td>140.90</td>
</tr>
<tr>
<td></td>
<td>Posttreatment</td>
<td>127.80</td>
</tr>
<tr>
<td></td>
<td>Follow-up</td>
<td>119.20</td>
</tr>
</tbody>
</table>

Note: CT = cognitive therapy; ACT = acceptance and commitment therapy; BDI = Beck Depression Inventory; ATQ = Automatic Thoughts Questionnaire; ATQ-B = Automatic Thoughts Questionnaire—Believability; DAS = Dysfunctional Attitude Scale.
difference in outcome, and thus attention was first directed toward the primary outcome variable. Hierarchical linear modeling (HLM) was used to investigate treatment outcomes using BDI scores and an intent-to-treat sample (Raudenbush & Bryk, 2001). The standard HLM model involves two levels of analysis that assume that the outcome varies within participants over time as a function of a person-specific growth curve (Level 1) and that these person-specific change parameters vary across participants as a function of the participant’s treatment (Level 2). HLM is a particularly powerful way to conduct an intent-to-treat analysis because unlike normal analysis of variance models, HLM does not assume that missing data are randomly related to outcomes or to the independent variables tested, and available data for all participants can be used.

Four participants dropped out of the study, three in the CT condition (after the 5th, 6th, and 11th sessions) and one in the ACT condition (after the 4th session). Because weekly BDI scores were collected after each session, all participants had several posttreatment data points available. Assessment point was entered as a continuous covariate for all observations. A mixed model assuming a random intercept and a random slope, allowing the two to be correlated, and assuming an unspecified covariance structure produced a convergent model with the best restricted log likelihood and thus was used. This model is also fairly conservative because it makes the fewest assumptions about the underlying covariance matrix. What the analysis tests is the statistical significance of the difference in the slopes of the outcome variable in the two treatment conditions.

Participants were randomly assigned to therapy groups, but the difference between therapy groups within condition was first examined to see if we needed to account for this variance before the primary outcome analysis was conducted. The time by group interaction did not approach significance in either condition ($p > .25$) and thus a two-level model was used.

Results showed no significant effect for treatment condition, $F(1, 23.11) = 0.062, p = .81$, but there was a significant effect for time, $F(1, 18.19) = 41.20, p = .000$, and the interaction of condition and time, $F(1, 18.19) = 6.73, p = .018$ (effect size $= 1.08$). BDI scores improved in the ACT condition to a significantly greater degree across time than in the CT condition. The difference in slopes and the underlying BDI means are shown in Figure 1. No outliers were identified, but to assess the stability of the results given the relatively small $n$, it was repeated with the best responder in the ACT condition and the worst responder in the CT condition removed. The difference in slopes was still marginally significant ($p = .058$). A model that nested individuals within therapy groups and then conditions, which is greatly underpowered given the $n$ in this study, still had a marginally significant time by treatment condition interaction ($p = .071$).
Because it is desirable to examine the effects of mediators assessed before there is a significant difference in outcome (Stice, Presnell, Gau, & Shaw, 2007), the main analysis was reconducted eliminating the follow-up BDI to see whether there were already outcome differences at the point at which the mediator was tested. The interaction of condition and time was still significant, $F(1, 19.24) = 5.33, p = .032$. Thus, it is not possible in this data set to fully meet the conditions for an ideal mediation analysis.

It should be noted that although the HRS-D was reliable and collected by an independent evaluator blind to treatment assignment at pretreatment, posttreatment, and follow-up, it could not be used as the primary outcome measure or for meditational analyses because it failed to show homoscedasticity at follow-up. At follow-up, the BDI and HRS-D correlated very strongly, however, calculated both parametrically and nonparametrically ($r = .85, p = .84, p < .001$), suggesting that the self-reported depression scores that were the primary outcome in this study were objectively valid as compared with a structured clinical interview.
Mediation Analysis

The primary focus of the current discussion about ACT and CT are the possible process differences. The best way to examine this is through mediation. Mediation analysis is most commonly conducted using causal steps (Baron & Kenny, 1986), but there are several problems with that approach. First, the causal-steps approach never directly tests the significance of the difference between the direct and indirect, or mediated, effect. Second, it requires both a significant “a path” (treatment on the mediator) and a significant “b path” (mediator on outcome controlling for treatment), despite the fact that these two values are necessarily related (the larger the “a” path the smaller the “b” and vice versa), which can considerably and artificially reduce power. In part for that reason, testing the significance of the “a” and “b” cross product is recognized as perhaps the best all-around available method to test mediation (MacKinnon, Lockwood, Hoffman, West, & Sheets, 2002). In most finite data sets, the cross product models the difference between the direct (treatment on outcome or c) and the indirect path (treatment on outcome accounting for the mediator or c’) and thus provides a single test of the statistical significance of the mediated effect.

The most common approach to assessing the statistical significance of the cross product of the coefficients is the Sobel test (Sobel, 1982), which uses multivariate delta logic to generate the needed error term for the evaluation. Unfortunately, the Sobel test assumes a normal distribution of the cross product, which has been shown to be generally incorrect (Preacher & Hayes, 2004).

The nonparametric method used in the current study to test the statistical significance of the cross product of the coefficients (Preacher & Hayes, 2004, 2008) solves the distribution problem through bootstrapping, in which k samples of the original size are taken from the obtained data (with replacement after each specific number is selected) and mediational effects are calculated in each sample. In the present set of analyses, parameter estimates were based on 3,000 bootstrap samples. The bias corrected and accelerated 95% confidence intervals were then examined. These confidence intervals are similar to the 2.5 and 97.5 percentile scores of the obtained distribution of the cross products over the k samples, but with z score-based corrections for bias due to the underlying distribution (Preacher & Hayes, 2004, 2008). If the confidence intervals do not contain zero, the point estimate is significant at the level indicated.

Three primary mediation analyses were conducted using the ATQ-B, the ATQ, and the DAS. The ATQ-B was examined as a measure of cognitive defusion, which is emphasized in ACT; the ATQ was examined as a measure...
of the level of occurrence of depressogenic thoughts and the DAS as a measure of dysfunctional attitudes occurring at a higher level of cognitive organization, both of which are emphasized in CT. In each case, posttreatment scores for these mediators were examined for their impact on BDI follow-up scores, using pretreatment BDI scores as a covariate. All possible multiple mediator models were also examined (combining these three mediators in pairs or in a model with all three as mediators), but in no cases was an insignificant individual mediator significant when considered as part of a multiple-mediator model. For that reason, the results of the multiple-mediator analyses add no new theoretical information and they will not be presented here.

In the analyses below, we will describe the results of the nonparametric analysis followed by the presentations of the values for the $a$, $b$, $c$, and $c'$ paths as generated by normal theory (i.e., without bootstrapping). The point estimates of the $a \times b$ cross product (mean and standard error for the 3,000 samples) and the relevant bootstrapped confidence intervals (for $p \leq .05$) are shown in Table 2.

The posttreatment scores on the ATQ-B successfully mediated BDI follow-up outcomes ($p <= .05$). Normal theory tests showed a marginally significant $a$ path, $t = 1.74, p < .1$, and a significant $b$ path, $t = 2.37, p < .03$. A significant $c$ path, $t = 2.24, p < .038$, became nonsignificant when adjusted for the mediator ($t = 1.42, p > .17$), indicating mediation.

Table 2. Bootstrapped Point Estimates and BCa CI for the Total and Specific Indirect Effects on Follow-Up BDI Scores, Using Pretreatment BDI Scores as a Covariate

<table>
<thead>
<tr>
<th>Mediators tested</th>
<th>Product of $ab$ coefficients</th>
<th>BCa 95% CI</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Point estimate</td>
<td>SE</td>
</tr>
<tr>
<td>Cognitive defusion: Post ATQ-B</td>
<td>$-3.22$</td>
<td>$2.67$</td>
</tr>
<tr>
<td>Depressogenic thoughts: Post ATQ</td>
<td>$-1.60$</td>
<td>$1.95$</td>
</tr>
<tr>
<td>Dysfunctional attitudes: Post DAS</td>
<td>$0.21$</td>
<td>$0.89$</td>
</tr>
<tr>
<td>Dysfunctional attitudes: Follow-up DAS</td>
<td>$1.72$</td>
<td>$2.02$</td>
</tr>
</tbody>
</table>

Note: BCa CI = bias corrected and accelerated confidence intervals; ATQ-B = Automatic Thoughts Questionnaire–Believability; ATQ = Automatic Thoughts Questionnaire; DAS = Dysfunctional Attitude Scale.
The posttreatment scores on the ATQ did not successfully mediate BDI follow-up outcomes \((p > .05)\). Additional bootstrapped analyses showed that this remained true at \(p = .1\) but did reach significance at \(p = .15\). Normal theory tests showed a significant \(a\) path, \(t = 2.18, p < .05\), but a nonsignificant \(b\) path, \(t = 0.83, p > .4\). The significant \(c\) path reported above became nonsignificant when adjusted for the mediator, \(t = 1.59, p > .13\).

The posttreatment scores on the DAS did not successfully mediate BDI follow-up outcomes \((p > .05)\). Additional bootstrapped analyses showed that this remained true at \(p = .1\) and \(p = .15\). Normal theory tests showed a nonsignificant \(a\) path, \(t = 0.32, p > .75\), and a nonsignificant \(b\) path, \(t = 0.72, p > .48\). The significant \(c\) path reported above remained significant when adjusted for the mediator, \(t = 2.25, p < .038\).

Because the DAS is commonly associated with a cognitive model of depression, an additional analysis was conducted using the follow-up DAS scores as a mediator, which is analytically a considerably more liberal approach. This also failed to mediate BDI follow-up outcomes \((p > .05)\). Additional bootstrapped analyses showed that this remained true at \(p = .1\) but that it became significant at \(p = .15\). This weak effect was, however, evidence of suppression and not mediation because adjusting the impact of treatment on outcome by the mediator tended to increase the difference between CT and ACT on BDI follow-up outcomes. This overall pattern is shown in the normal theory tests. Both the \(a\) path, \(t = 1.57, p > .14\), and \(b\) path, \(t = 1.28, p > .23\), were nonsignificant; the significant \(c\) path reported above became somewhat more so when adjusted for the follow-up DAS score, \(t = 2.57, p < .02\).

Finally, as was recommended by Baron and Kenny (1986), to deal with the conceptual ambiguity of mediation when outcomes had also already changed, the main mediation analysis was repeated, reversing the relationship of outcome and mediator to see if post-BDI scores mediated follow-up ATQ-B values (using pre ATQ-B scores as a covariate). It was not significant.

**Discussion**

Mediation provides important information regarding the treatment technology and the theoretical model related to specific treatment approaches. For mediation to work, the technology used must impact the mediator and the mediator must impact the outcome, controlling for treatment. It is necessary to control for treatment in the \(b\) path analysis because this prevents mere socialization into a treatment model or irrelevant correlates associated with successful treatments from being improperly categorized as “mediation.”
To date, the published comparisons of ACT to other methods have all indicated differences in processes of change, but this alone does not show the functional relation with outcome that is assessed through mediation analysis. However, a number of formal mediation analyses have been reported. So far, successful ACT mediators include the following: (a) general or specific measures of acceptance and psychological flexibility (Bond & Bunce, 2000; Forman et al., 2007; Gifford et al., 2004; Gregg, Callaghan, Hayes, & Glenn-Lawson, 2007; Lappalainen et al., 2007; Lillis, Hayes, Bunting, & Masuda, 2009; Lundgren, Dahl, & Hayes, 2008), (b) defusion (Gaudiano & Herbert, 2006; Hayes et al., 2004; Lundgren et al., 2008; Varra, Hayes, Roget, & Fisher, 2008; Zettle & Hayes, 1986), (c) values (Lundgren et al., 2008), and (d) mindfulness (Forman et al., 2007). All these processes of change are explicitly included in an ACT model (Hayes et al., 2006).

Only a few studies have directly compared process differences between ACT and CT or CBT (Bond & Bunce, 2000; Forman et al., 2007; Lappalainen et al., 2007; Zettle & Hayes, 1986, as reanalyzed in Hayes et al., 2006). The present reanalysis adds to this set and replicates with group treatment the findings of Zettle and Hayes (1986, as reanalyzed in Hayes et al., 2006) that the ATQ-B mediated differences in depression outcomes between ACT and CT in individual psychotherapy for depression.

It should be noted that the differences in outcome on the BDI were significant at posttreatment, and thus, the present test of mediation does not meet all the criteria that are desirable in mediation analysis (Stice et al., 2007). This is a notable limitation of the present study, although it seems unlikely that outcome per se produced the process differences seen, given that when reversed, the BDI did not mediate the ATQ-B. Furthermore, some ACT studies have shown successful mediation of outcomes by ACT processes even when these processes were assessed before outcomes changed (see, for example, Gifford et al., 2004; Gregg et al., 2007; Zettle & Hayes, 1986, as reanalyzed in Hayes et al., 2006). Outcome differences can come early (in this data set, there was a significant difference between conditions by Session 5; see Figure 1). Thus if mediation is a key focus, very high frequency outcome and process measurement seems wise, although it is admittedly hard to be accomplished without an excessive assessment burden.

To some degree, it is surprising that measures of cognitive defusion can function as mediator when ACT and CT are compared because the believability of cognition is included as a target in a traditional cognitive model (Beck et al., 1979). The difference is that traditional CT seeks to alter changes in believability through logical analysis and empirical hypothesis-testing,
and thus believability is targeted in a different way than ACT. Empirically, that was apparently enough in the present case, but when comparing ACT and traditional CBT in a modern study, a broader range of process targets should be used. Zettle and Rains (1989) did not have the advantage of measures of mindfulness, acceptance, values, and the like that might be included in a modern comparison of these methods.

Although the ATQ and DAS have been regularly used in process studies on CT (Garratt et al., 2007), it is possible there are better mediators to be drawn from cognitive theory. This is a complex area because it depends simultaneously on many factors: specificity and scope of the theory, the availability of measures, and the precision and breadth of the treatment technology among them. A recent review in this area (Longmore & Worrell, 2007) found that the evidence for cognitive mediation is weak and inconsistent in cognitive therapy, but other authors disagree (e.g., Hofmann & Asmudson, 2008), so the question is still in spin. It is also known that other active treatments, such as pharmacotherapy, move cognitive measures (Garratt et al., 2007), which is an example of why studies such as the present one are important: Mediation linked to active treatment comparisons (as opposed to mediation of the effect of an active treatment as compared with a wait list or placebo) provide relatively challenging tests for underlying theories.

It is important not to oversimplify the present results, however. ACT does produce cognitive and emotional change, and indeed the theory suggests that altering such processes as acceptance, defusion, mindfulness, and values should do so. It also seems likely that successful CBT and CT clients will move on ACT processes as well, especially over time. But the present study adds evidence suggesting that early differences in these processes of change do seem to predict ultimate outcomes. In the present study, ACT changed the occurrence of depressogenic thoughts and their believability assuming they had occurred, but the defusion effect accounted for more of the differences in follow-up outcomes between ACT and CT. CT did not change the DAS more so than did ACT, and the DAS did not mediate outcomes. Indeed, the effects indicated that the DAS could be viewed as a weak suppressor. Overall, the present reanalysis provides additional evidence that ACT and traditional CT are theoretically distinct models and methods, despite the fact that both are part of the larger family of interventions called “CBT.”

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1. Copies of the treatment protocols may be obtained by contacting the first author.

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