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The Impact of Treatment Components Suggested by the Psychological Flexibility Model: A Meta-Analysis of Laboratory-Based Component Studies

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Abstract

An important aspect of psychotherapy research is the examination of the theoretical models underlying intervention approaches. Laboratory-based component research is one useful methodology for this endeavor as it provides an experimental means of testing questions related to intervention components and the change process they engage with a high level of control and precision. A meta-analysis was conducted of 66 laboratory-based component studies evaluating treatment elements and processes that are suggested by the psychological flexibility model that underlies Acceptance and Commitment Therapy (acceptance, defusion, self as context, committed action, values, and present moment), but also touches on a variety of contextual forms of cognitive behavior therapy. Significant positive effect sizes were observed for acceptance, defusion, present moment, values, mixed mindfulness components, and values plus mindfulness component conditions compared to inactive comparison conditions. Additional analyses provided further support for the psychological flexibility model, finding larger effect sizes for theoretically-specified outcomes, expected differences between theoretically distinct interventions, and larger effect sizes for component conditions that included experiential methods (e.g., metaphors, exercises) than those with a rationale alone. Effect sizes did not differ between at risk/distressed and convenience samples. Limitations with the meta-analysis and future directions for laboratory-based component research are discussed.

Keywords. Psychological Flexibility; Acceptance and Commitment Therapy; Component Analysis; Mindfulness; Values

The Impact of Treatment Components Suggested by the Psychological Flexibility Model: A Meta-Analysis of Laboratory-Based Component Studies

There is wide spread agreement that the purpose of treatment research is not merely to evaluate the efficacy/effectiveness of interventions, but also to increase our theoretical understanding of how treatments have their effect (Kazdin, 2007; Murphy, Cooper, Hollon, & Fairburn, 2009). That dual commitment has been clear from the earliest days of organized research on psychotherapy outcomes and is reflected in the last five words of Gordon Paul's famous statement of the question empirical treatment research needs to answer: "What treatment, by whom, is most effective for this individual with that specific problem under which set of circumstances, and how does it come about" (1969, p. 44). Thus, a central goal of treatment research is to develop robust theoretical models of human difficulties and their alleviation.

This goal has recently been highlighted in relation to the evidence-based therapy movement, with researchers stressing the need to evaluate the evidence for a treatment's theoretical model, in addition to its efficacy, in determining evidence-based therapy status (e.g., David & Montgomery, 2011; Lohr, 2011). Treatment outcome is per se often taken to indicate the usefulness of the foundational theoretical ideas that were extended into application, but in large packages that is a difficult connection to make. Process of change research, such as mediational analysis, provide one method for obtaining more refined support for underlying theoretical models and principles by examining whether changes in outcome are functionally related to changes in theoretical processes (Kazdin, 2007).

A useful additional method for theory testing is to evaluate the impact of theoretically derived treatment components in laboratory-based experimental research (David & Montgomery,

2011; Hayes, Levin, Plumb, Boulanger & Pistorello, in press; Kazdin, 1978). Such laboratory-based component studies provide a methodology in which relevant sample, intervention, and contextual variables can be carefully controlled and manipulated to test theoretical hypotheses at a level that would be difficult to achieve in treatment outcome research due to pragmatic and external validity issues (Hayes et al., in press; Kazdin, 1978). These studies allow more precise assessment methods than what is feasible in treatment outcome research (e.g., behavioral measures, response to laboratory inductions, moment by moment physiological measures), which can sometimes be necessary in testing refined theoretical hypotheses (Hayes et al., in press). Control conditions can be used in laboratory-based component studies, that would otherwise not be feasible in treatment outcome research due to ethical concerns (e.g., potentially iatrogenic interventions – thought suppression, rumination) or practical limitations (e.g., reliably implementing precise manipulations) (Kazdin, 1978). Finally, because these studies are often smaller and less costly to run, they can be conducted relatively early and throughout theory and treatment development, before models and packages become difficult to change (Hayes et al., in press). This can facilitate an iterative process, where results from laboratory-based research inform further innovation and refinement of the theoretical model and treatment technology, which subsequently guide further laboratory-based research.

If theoretically derived components do not perform as expected in laboratory-based studies, it suggests there is an issue with the theory that underlies a treatment approach, while positive findings help bolster the empirical support for the theory. However, laboratory-based component studies do not demonstrate the efficacy of actual clinical component interventions. That question requires the more elaborate and extensive methodologies of traditional component analyses, dismantling studies, and treatment efficacy research. Yet, such outcome research can

be more theoretically informative by building off of findings in laboratory studies.

The present paper examined laboratory-based component studies focused on the main elements of psychological flexibility, the theoretical model underlying Acceptance and Commitment Therapy (ACT; Hayes, Strosahl, & Wilson, 1999; 2011). Psychological flexibility lends itself to laboratory-based components studies because it represents a small number of fairly well-specified processes organized into a single model and linked to specific intervention methods. Although psychological flexibility is linked to ACT, the various processes within this model are shared with other contextual cognitive behavior therapies that emphasize acceptance and mindfulness (see Hayes, Villatte, Levin & Hildebrandt, 2011 for a comprehensive review of the wide variety of current CBT methods that appear to modify these processes).

The psychological flexibility model is composed of six processes that each have a problematic manifestation and an indicated process of change for treatment: (1) cognitive fusion / defusion, (2) experiential avoidance / acceptance, (3) loss of flexible contact with the now / present moment focus, (4) attachment to a conceptualized self/ self as context, (5) values problems / chosen values, and (6) inaction, impulsivity, or avoidant persistence / committed action. A number of laboratory-based component studies have been published in most areas of the psychological flexibility model, and thus enough data exists to warrant a comprehensive examination.

A review of laboratory-based component studies is of applied importance in the context of evidence that the treatment package informed by the theory is useful. While there are arguments about methodological issues and the appropriate measures or comparisons (e.g., Gaudiano, 2009; Levin & Hayes, 2009), the major meta-analyses published on ACT outcomes agree that ACT is effective, with generally medium to large effect sizes as compared to wait lists

or treatment as usual across a very wide range of problem areas (Hayes, Luoma, Bond, Masuda, & Lillis, 2006; Öst, 2008; Powers, Zum Vörde Sive Vörding, & Emmelkamp, 2009).

This paper presents a meta-analysis of laboratory-based component studies seeking to examine whether psychological flexibility components impact psychological outcomes in ways that are consistent with the theoretical model. Effect sizes were calculated across studies to examine the impact of psychological flexibility components relative to inactive (i.e., inert) and theoretically distinct (i.e., thought suppression, rumination) conditions on a range of outcomes. Additional sub-analyses were conducted to further test specific theoretical predictions.

Rather than directly attempting to change the frequency or intensity of distressing internal experiences (i.e., thoughts, feelings, sensations), the psychological flexibility model focuses on changing how people relate to distressing internal experiences, and on engaging in values-based actions independent of these experiences. Thus, the impact on these psychological flexibility targeted outcomes would be expected to be greater, at least in the short run, than on the intensity/frequency of distressing internal experiences. This was explored by examining differences in effect sizes on these two distinct sets of outcomes.

The psychological flexibility model also posits that experiential methods such as exercises or metaphors are particularly helpful in reducing the dominance of literal verbal rules over experience, as compared to mere rationales (Hayes, Strosahl et al., 2011). This theoretical claim was tested by comparing conditions with experiential exercises, metaphors, or only a rationale/description of what to do.

Methods

Eligibility Criteria

The current meta-analysis included English-language published, peer-reviewed

laboratory-based studies testing single-session component conditions targeting a subset of psychological flexibility components (acceptance, defusion, present moment, self as context, values, committed action) as compared to alternative conditions to which participants were randomly assigned (e.g., distraction, attention control condition). Studies had to include at least one outcome of possible applied or theoretical relevance (e.g., psychological distress, task persistence) and report the information needed to calculate an effect size or have the primary author agree to provide us with this information (five studies were excluded because requested information was not provided). To keep the focus on brief laboratory-based component studies, conditions targeting all the components of psychological flexibility were excluded (e.g., Gregg et al., 2007 and Varra et al., 2008), as were studies that failed to include an experimental manipulation (e.g., correlational studies). Studies with multi-session component interventions were excluded because they blend gradually into entire treatments and raise special issues (e.g., variability in attendance; variability in content due to attendance) that single session studies do not (although studies with multiple assessment time points were not excluded). Contemplative practice conditions (i.e., formal mindfulness meditation practices) were included given their use in ACT (e.g., Hayes & Smith, 2005) and their inclusion in the psychological flexibility model (Hayes, Villatte et al., 2011). Studies on committed action alone, which include behavioral methods used to build patterns of valued activity (i.e., goal setting, contingency management, behavioral activation), were excluded because this literature is vast and well-established.

Article Search and Eligibility Screening

Searches were conducted for relevant studies on PsychInfo and Medline up to February 2011 using the following keyword/text word search terms: “Acceptance and Commitment Therapy” or “Defusion” or “Acceptance-Based” or “Mindfulness” or “Psychological

Flexibility", with articles limited to peer-reviewed journals. This search identified 1,147 articles on PsychInfo and 905 articles on Medline. An additional 46 articles were identified by cross-referencing articles, searching publications listed on the Association for Contextual Behavioral Science website, and requesting in press and published ACT-relevant component studies from an email list serve focused on ACT research and from researchers who had published ACT studies.

An initial screening was conducted by the first author to exclude studies that were clearly ineligible based on criteria that are easy to apply reliably (non-empirical articles, non-intervention studies, studies involving multi-session component interventions), resulting in 89 articles reporting a total of 117 studies as potentially eligible. These studies were examined for eligibility by the first, second and third author. In order to assess the reliability of exclusion and inclusion, half of the 117 studies (n = 58) were double coded for specific inclusion or exclusion relevant features by two of the three raters (Kappa = .77).

Of the 117 screened studies, 66 eligible studies (drawn from 57 articles) were included in the final sample. A total of 51 studies (43.6%) were excluded: 24 did not include a psychological flexibility component; 5 targeted all of the components of psychological flexibility; 7 tested a multi-session component intervention; 11 did not use a randomized between groups design; 7 did not have an appropriate comparison condition; 3 did not include a measure of possible applied or theoretical relevance; 5 lacked data to calculate an effect size; 10 failed to meet eligibility criteria on multiple factors (a list of excluded studies is available from the first author upon request).

System for Categorization

Studies were systematically categorized on several features including targeted components, intervention modality, comparison conditions, sample type, and outcomes. Two advanced clinical graduate students with significant experience in ACT applied the particular

categories (see below). Approximately 50% of the studies were randomly selected and double coded on these categories independently by one of the authors in order to examine inter-rater reliability (Kappa = .89). The categorization results from each study are listed in Table 1.

Component Categorization

Component intervention scripts were obtained from articles when provided, or requested from the study author when not available. Conditions were categorized based on the psychological flexibility components that were targeted. Criteria for acceptance included 1) Instructing participants to sit with, lean into, or otherwise relate to difficult thoughts and feelings in an accepting way, 2) Discussing how to practice acceptance, 3) Discussing and/or helping participants notice the negative effects of suppression, avoidance, or otherwise controlling one's thoughts and feelings, 4) Explicitly targeting letting go of unhelpful experiential avoidance strategies. Criteria for defusion included 1) Helping participants interact with thoughts in a non-literal way (e.g., seeing a thought as just a thought, saying the thought in a funny voice or singing it), 2) Guiding participants to notice and let go of judgments and evaluations, 3) Instructing participants to engage in actions independent of their thoughts in order to reduce the link between thoughts and behaviors. Criteria for self as context included 1) Guiding participants to contact a sense of self as distinct from their thoughts, feelings and other internal experiences, 2) Helping participants to notice their internal experiences from a distinct observer perspective, 3) Evoking and supporting flexibility in perspective taking across frames of I/You, Here/There, and Now/Then. Present moment criteria included instructing participants to actively attend to bodily sensations, thoughts, feelings, and/or other internal experiences in the present moment. Committed action criteria included 1) Discussing making commitments linked to one's values, 2) Guiding participants to make specific commitments related to their personal value(s) and the

laboratory task at hand. Values criteria included 1) Discussing central features of values from a psychological flexibility perspective, 2) Helping participants to clarify their values, 3) Relating personal values to a laboratory task. If a condition attempted to isolate and target a specific component, it was categorized for that component only. Two combined component categories were used: mixed mindfulness to refer to combinations of acceptance, present moment, defusion, and self as context; and values plus mindfulness elements.

Although there were 66 studies in the sample, 2 studies included 2 separate psychological flexibility component conditions - which was used for each effect size calculation depended on the specific comparison being examined (e.g., psychological flexibility component, intervention modality). Of these 68 intervention conditions, 7 were categorized as acceptance, 7 as defusion, 8 as values, 13 as present moment, 25 as mixed mindfulness components, and 8 as values plus mindfulness components. There were no studies using self-as-context or committed action alone (the latter was an exclusion criterion so no studies should have been so categorized). The inter-rater reliability for component coding was Kappa = .80.

Modality Categorization

Scripts were also used to categorize condition modality using three categories: rationale-alone, therapeutic metaphors, and experiential exercises. Conditions were coded as a rationale-alone if they provided instructions to engage in a psychological flexibility process and/or education regarding how or why to engage in one of these processes, but did not include any therapeutic metaphors or experiential exercises. In other words, participants were given a rationale for using a skill or engaging in a process, but without any more intensive or experiential methods to practice or engage in it. Conditions were coded as therapeutic metaphors if they included a metaphor designed to exemplify/elaborate on a particular psychological flexibility

process, but did not include any experiential exercises. Interventions were coded as experiential exercises if they included having participants practice engaging in a psychological flexibility process (e.g., mindfulness meditation, defusion exercises, writing about personal values).

Of the 66 studies (2 of which included an additional psychological flexibility condition), 12 were coded as rationale-alone, 10 as therapeutic metaphors, and 46 were coded as experiential exercises. The inter-rater reliability for modality categorization was Kappa = .68, which is just below the targeted level. Because 80% of the disagreements involved distinguishing therapeutic metaphors and experiential exercises (which can be difficult when metaphors become enacted or highly involved), key examinations of modality effects were followed by supplementary analyses that combined metaphor and experiential exercises as a single category. Kappa when these two categories were combined (i.e., rationale alone vs. metaphor/experiential) was .89.

Comparison Condition Categorization

These laboratory-based component studies typically used comparison conditions belonging to one of a few specific theoretical categories. Thus, comparison conditions were categorized for each study as inactive, control context, fusion, and active comparison conditions.

Inactive conditions refer to conditions merely intended to control for basic factors such as time, attention and demand characteristics. Examples included attention controls (i.e., reading a magazine), conditions intended to be inert (i.e., writing about how your least important value is meaningful to others), or no instructions. Control context conditions refer to conditions that actively encourage strategies designed to control the frequency or intensity of aversive internal experiences (e.g., thoughts, feelings, and sensations). Examples included teaching distraction or suppression techniques. Fusion conditions refer to conditions that actively encourage engaging in maladaptive cognitive patterns in which thoughts are actively focused on and related to as if

literally true. Examples included assigned worry or rumination. Active conditions that did not fit within these categories, such as cognitive reappraisal and imaginal exposure, were categorized as active comparisons. Although the psychological flexibility model can encompass some aspects of such interventions (for example, cognitive reappraisal can blend into cognitive flexibility training), no attempt was made to further categorize them in theoretical terms, due to the complexity and difficulty of that task.

Of the included studies, 44 included an inactive condition, 39 a control context condition, 7 a cognitive fusion condition and 4 an active comparison condition. The inter-rater reliability for comparison condition coding was Kappa = .97.

Sample Categorization

The sample for each study was categorized as either at risk/distressed or convenience sample. Laboratory-based component studies are focused on theory, not evaluating the efficacy of clinical interventions with clinical samples, but this variable provided some indication of the relevance of findings to applied populations. At risk/distressed samples consisted of individuals with current or past psychological disorders (i.e., individuals with panic disorder or a history of depression), elevated symptoms (i.e., depression), other applied problems (i.e., burn patients) or notable risk factors (i.e., high in experiential avoidance or anxiety sensitivity). All others (i.e., university students, members of the community in general) were coded as convenience samples.

Of the included studies, 51 included a convenience sample and 23 included an at risk/distressed sample (some included both). The inter-rater reliability for sample type coding was Kappa = .72.

Outcome Measures

Given the purpose of this meta-analysis, outcome measures were grouped according to

psychological flexibility theory. Outcomes focusing on overt behavior (i.e., directly observable by others as opposed to internal experiences) and its direct by-products, or one's relation to internal experiences (e.g., acceptance of feelings, mindfulness of sensations, believability of thoughts) were categorized as primary "psychological flexibility targeted outcomes." This consisted of the following variables: the ability to persist in a distressing task (e.g., time spent in a cold pressor), willingness to re-engage in a difficult task (e.g., self-reported willingness to return for a second CO_2 exposure session), believability of distressing thoughts, and behavioral outcomes or their direct by-products (e.g., academic grades, cigarette smoking, memory recall, number of errors in a task, engaging in downward social comparisons). A second distinct set of outcomes, the "frequency/intensity of distressing internal experiences," are broadly positive, but are argued by psychological flexibility theory to be less likely to change when they are directly targeted. Examples included frequency of personal intrusive thoughts, self-reported distress, distress from self-critical thoughts, and other self-reported negative reactions to laboratory-based stressors. Psychological flexibility theory does *not* predict that these outcomes should not or will not change with acceptance, mindfulness, and values: "Therapists can readily elaborate cognition, make it more flexible, and indeed make it less likely (ironically one of the best ways to do that is just to make specific thoughts less important, which is an emphasis in ACT)" (Hayes, Strosahl et al., 2011, p. 51). Because they are not directly targeted, however, they were categorized separately. All other positive outcomes, were categorized as "other outcomes," such as positive emotions, physiological arousal, attitudes, acceptance of health messages, and motivation. The inter-rater reliability for outcome coding was Kappa = .83.

Data Collection and Effect Size Calculation

Data was extracted from reports by the first author of this paper; each value was checked

by an additional author. When the necessary data to calculate an effect size was not available, the data was requested from the study authors.

All data were analyzed using Comprehensive Meta-Analysis, a commercially available software program for conducting meta-analyses (Borenstein & Rothstein, 1999). Between group effect sizes were calculated using Hedge's *g* for each outcome. Effect sizes were averaged into a composite score when multiple outcome measures were reported in a study for the same category. When possible, Hedge's *g* was calculated using the relevant means and pooled standard deviation. If these data were not reported, test statistics such as Chi Square or *F* statistics were converted into an effect size using the appropriate formulas (Rosenthal, 1991). Given the variety of sample types, outcome measures, and experimental manipulations included in laboratory-based component studies, heterogeneity in effect sizes is likely, so random effects model were used to estimate aggregated effect sizes (Borenstein, Hedges, Higgins & Rothstein, 2009). When relevant, mixed effects models with *Q*-tests based on analysis of variance were conducted to test for statistically significant differences in effect sizes between subgroups (Borenstein et al., 2009). When studies had multiple subgroups for a given analysis and effect sizes were based on shared estimates (i.e., McMullen et al., 2008 included both a rationale-alone and experiential condition), only one subgroup was selected for the analysis based on whichever subgroup had fewer studies. Effect sizes were discussed using Cohen's (1988) cutoffs for small (.2), medium (.5) and large (.8) effects.

Results

Are Psychological Flexibility Components Psychologically Active?

At the most basic level, it seems important to determine whether psychological flexibility components generally affect outcome variables in laboratory-based component studies, whether

or not these outcomes are targeted theoretically. In order to assess whether components were psychologically active (i.e., have any effect on psychological outcomes), their impact on all outcomes was examined relative to inactive comparison conditions. Effect sizes were calculated for each psychological flexibility component compared to inactive conditions based on all relevant studies and outcomes. Significant medium effect sizes were observed favoring defusion, values, and values plus mindfulness components and significant small effect sizes were observed favoring present moment and mixed mindfulness components (see Table 2). A non-significant small effect size approaching significance was also observed favoring the acceptance component.

Of more theoretical importance, psychological flexibility components were examined to see whether they had an impact on primary theoretically-specified outcomes relative to inactive conditions. When only outcomes targeted by the psychological flexibility model were considered, a significant large effect size was observed favoring acceptance, and values plus mindfulness components; significant medium effect sizes were observed favoring defusion, and present moment components; and significant small effect sizes were observed favoring mixed mindfulness, and values components (see Table 2). Overall, these results suggest the acceptance, defusion, values, and present moment components of psychological flexibility are psychologically active, particularly with primary theoretically-specified targeted outcomes.

Do Psychological Flexibility Components Impact Outcomes in an Expected Way?

Analyses were conducted to examine whether psychological flexibility outcomes have a larger impact on primary theoretically-specified targeted outcomes (i.e., task persistence, willingness, believability of thoughts) than on the frequency/intensity of distressing internal experiences. A significant medium effect size was observed favoring psychological flexibility components taken as a whole, relative to inactive conditions on primary targeted outcomes (g =

.68, 95% CI = .50, .85, z = 7.53, n = 28), while a significant small effect size was observed favoring psychological flexibility components on the frequency/intensity of distressing internal experiences (g = .25, 95% CI = .08, .41, z = 2.91, n = 30). As these two effect sizes were calculated using a combination of shared and distinct samples, we did not directly test the statistical significance of the difference between effect sizes, but it is worth noting that the 95% confidence intervals do not overlap.

Do Psychological Flexibility Components Impact Outcomes Differently Than Theoretically Distinct Conditions?

Several studies included active comparison conditions that were selected so as to directly contrast with psychological flexibility concepts. Most of these could be conceptualized as control context manipulations focusing on strategies such as distraction and suppression. Significant small effect sizes were observed favoring psychological flexibility components over control context conditions on all outcomes (g = .20, 95% CI = .07, .33, z = 2.98, n = 39) and primary targeted outcomes (g = .48, 95% CI = .29, .67, z = 4.90, n = 26). Given that control context manipulations can sometimes work immediately, but with a rebound effect over time (e.g., Abramowitz, Tolin & Street, 2001), we further examined effect sizes for the frequency/intensity of distressing internal experiences during and after a laboratory-based stressor (i.e., mood induction, cold pressor). There was no significant effect size for the frequency/intensity of distressing internal experiences during laboratory-based stressors (g = .11, 95% CI = -.06, .28, z = 1.30, n = 30), but there was a significant small effect size favoring psychological flexibility components when recovering after a laboratory-based stressor (g = .27, 95% CI = .10, .45, z = 3.07, n = 15).

A smaller number of studies compared psychological flexibility components to

conditions designed to support cognitive fusion (e.g., rumination, worry). There was a significant medium effect size favoring psychological flexibility components relative to fusion conditions on all outcomes (g = .50, 95% CI = .21, .79, z = 3.37, n = 7). Only two studies included a targeted outcome measure, so no effect size was calculated.

Overall, these findings suggest psychological flexibility components impact outcomes differently than specific, theoretically distinct conditions (e.g., control context, fusion). This finding needs to be interpreted with caution, however, since these analyses cannot distinguish possible iatrogenic effects of control contexts and cognitive fusion, from beneficial effects of psychological flexibility components or some combination of such factors.

Are There Differences Across At Risk/Distressed and Convenience Samples?

Given the frequent use of convenience samples, one potential concern with laboratory-based component studies is that the findings may not apply to distressed or at risk samples. To examine this issue, mixed effects models with Q-tests based on analysis of variance were used to test for statistically significant differences between the effect sizes for at risk/distressed versus convenience samples.

When comparing psychological flexibility components to inactive conditions on all outcomes, there were similar significant small effect sizes for at risk/distressed samples (g = .41, 95% CI = .24, .58, z = 4.76, n = 18) and convenience samples (g = .39, 95% CI = .20, .57, z = 4.03, n = 30), and no significant difference between the two samples, $Q(1)$ = .04, p = .85. Results with primary targeted outcomes showed significant medium effect sizes for at risk/distressed samples (g = .68, 95% CI = .49, .88, z = 6.78, n = 10) and convenience samples (g = .63, 95% CI = .39, .87, z = 5.14, n = 20), and no significant differences between the two, $Q(1)$ = .10, p = .76.

These analyses were also conducted comparing psychological flexibility components to

control context conditions. Across all outcomes, there were significant small effect sizes for at risk/distressed samples (g = .23, 95% CI = .01, .45, z = 2.04, n = 13) and convenience samples (g = .18, 95% CI = .04, .32, z = 2.55, n = 29), with no significant difference between them, $Q(1)$ = .14, p = .71. For targeted outcomes, there was a significant medium effect size for at risk/distressed samples (g = .67, 95% CI = .28, 1.06, z = 3.33, n = 7) and a significant small effect size for convenience samples (g = .39, 95% CI = .18, .61, z = 3.57, n = 20), but no significant difference between the two samples, $Q(1)$ = 1.44, p = .23. Thus, all analyses suggested that laboratory-based studies evaluating psychological flexibility components produced similar findings with at risk/distressed and convenience samples.

Are There Differences Between Rationale-Alone and More Experiential Conditions?

Analyses were conducted to examine whether conditions using rationale-alone were less impactful than those including more experiential methods (metaphors and experiential exercises). All comparison conditions (e.g., inactive, control context, active, fusion) were included to increase power to detect significant differences between intervention types.

Across all outcomes, there was a significant small effect size for experiential conditions (g = .39, 95% CI = .25, .52, z = 5.60, n = 44) and metaphors alone (g = .20, 95% CI = .00, .40, z = 1.99, n = 10), but not for rationale-alone conditions (g = .01, 95% CI = -.20, .22, z = .11, n = 12). There was an overall significant difference between the three samples, $Q(2)$ = 9.11, p = .01. When comparing effect sizes across the three samples, there was a significant difference between experiential and rationale-alone conditions, $Q(1)$ = 8.67, p < .01, but there was no significant difference between experiential and metaphor or metaphor and rationale-alone conditions (p > .10). Because reliability of the distinction between metaphors and experiential exercises was somewhat low, a final comparison examined rationale alone compared to a collapsed experiential

and metaphor comparison. It too was significant $Q(1) = 8.14$, $p < .01$,

For primary targeted outcomes, there was a significant small effect size for experiential conditions ($g = .48$, 95% CI = .34, .61, $z = 6.81$, $n = 31$) and significant medium effect size for metaphors ($g = .65$, 95% CI = .34, .96, $z = 4.11$, $n = 7$), but no effect for rationale-alone conditions, and indeed the effects were trending in a negative direction ($g = -.15$, 95% CI = -.46, .17, $z = -.92$, $n = 5$). There was an overall significant difference between the three samples, $Q(2) = 15.49$, $p < .001$. When comparing effect sizes across the three samples, there was a significant difference between experiential and rationale-alone, $Q(1) = 12.89$, $p < .001$, and a significant difference between metaphors and rationale-alone conditions, $Q(1) = 12.62$, $p < .001$, but there was no significant difference between experiential conditions and metaphors ($p > .10$). The combined experiential/metaphor condition was also significantly different than rationale alone, $Q(1) = 14.37$, $p < .001$.

Overall, these analyses suggest that psychological flexibility components have a greater impact on psychological outcomes when they include elements such as metaphors and experiential exercises, as compared to verbal explanations and rationale-alone.

Test for Publication Bias

Funnel plots, fail safe N, and trim and fill methods were used to detect potential publication bias based on recommended procedures (Borenstein et al., 2009). Publication bias was examined for the comparison of psychological flexibility components to inactive conditions specifically as well as for all comparison conditions. Both funnel plots indicated a nearly symmetrical distribution and trim and fill analyses did not require any adjustment for either effect size estimate, suggesting no publication bias. The fail safe number of studies with an effect size of 0 necessary to make the effect non-significant was 1,230 across all comparison conditions

and was 1,097 for inactive comparison conditions. Overall, these results suggest there is minimal publication bias for the laboratory-based component studies.

Discussion

In broad terms, the present meta-analysis of laboratory-based component studies provides support for the usefulness and theoretical coherence of components specified by the psychological flexibility model. Significant positive effect sizes were observed for acceptance, defusion, present moment, values, mixed mindfulness, and values plus mindfulness conditions compared to inactive conditions, suggesting these components of psychological flexibility are psychologically active. Larger effect sizes were observed on primary theoretically-specified targeted outcomes than on the intensity/frequency of negative thoughts and feelings. Conditions theoretically contrary to a psychological flexibility model (e.g., control context, cognitive fusion) were less impactful, or possibly iatrogenic, relative to psychological flexibility components. Effect sizes for conditions including experiential methods (e.g., metaphors and exercises) were significantly larger than conditions using a rationale-alone.

The observed effect sizes tended to be medium or small, which makes sense when taking into account the brief and highly refined nature of these component interventions as well as the use of non-treatment seeking participants. The purpose of these studies is not to model normal treatment outcomes, but to focus on key theoretical issues. Thus, what is of most interest is the overall pattern of results from highly controlled component conditions on theoretically-specified outcomes.

A strategic advantage of laboratory-based component studies is that they help move attention away from horse races between treatment packages, toward understanding theoretically-specified components tightly linked to specific theoretical processes and principles.

In principle this should help establish empirically supported principles that can then be used by treatment developers regardless of their preferred treatment label (Rosen & Davison, 2003). This seems scientifically desirable because it should help reduce unnecessary proprietary barriers to effective intervention development. In that vein, it is worth noting that elements suggested by the psychological flexibility model (acceptance, mindfulness, and values interventions) are shared by interventions other than ACT, including not just the many newer contextual forms of CBT (see Hayes, Villatte et al., 2011), but also aspects of traditional CBT (Arch & Craske, 2008) and indeed treatment traditions outside of CBT such as existential, humanistic or analytic approaches. Thus the more principles oriented approach encouraged by laboratory-based studies could hold out hope for better communication among traditions and research programs.

A traditional concern with laboratory-based studies is whether results will generalize to clinical settings and populations (Kazdin, 1978). The applied impact of treatment components can only be assessed by direct clinical studies, not laboratory-based component studies. If the focus is on the applicability of the theoretical concepts, however, more can be said, particularly in this case. The processes composing psychological flexibility (e.g. experiential avoidance / acceptance, cognitive fusion / defusion) have been argued to be based on normal psychological processes (Hayes, Strosahl et al., 2011). Accordingly, while clinical populations would be expected to show greater difficulties, these processes should also be present in and relevant to the general population. The fact that virtually identical effects were seen in at risk or distressed samples as compared to convenience samples in the present analysis comports with that perspective.

This should not be taken to mean that psychological flexibility certainly applies with equal force to those with specific psychiatric syndromes, medical problems, or other problem

areas. Specific questions of that kind require studies with the population of interest and although there were a few studies in the present meta-analysis focused on specific populations (e.g., Campbell-Sills et al., 2006; Levitt et al., 2004), the number was too small for more fine-grained analyses. More laboratory-based component research needs to be done to examine the breadth of applicability of these theoretical concepts for those with specific disorders or problem areas, but without slipping into the mistaken idea that short laboratory-based studies can assess treatment outcomes per se. Expansion of population focus would be most useful if the theory itself suggested population differences. For example, psychological flexibility theory would predict that participants who were both distressed and highly experientially avoidant might respond differently to acceptance, mindfulness, and values based interventions than those who were neither. If the purpose of laboratory-based studies can become more well-understood, there is a vast range of studies that might be usefully done, not just in the area of psychological flexibility, but across CBT and clinical psychology generally.

Although a good number of laboratory-based studies have been conducted on psychological flexibility, there are notable holes in the literature. The self as context component is rarely included in laboratory-based component conditions and has never been tested in isolation in a published laboratory-based component study. Examining interactions between components and the cumulative effects of multiple components is an important aspect in testing a treatment package and theoretical model, and indeed the majority of studies in the present meta-analysis included combined components. However, these combinations generally were not designed to test theoretical predictions of how treatment components interact or can best be sequenced. It is unclear from the current laboratory-based component literature whether and how combining components of psychological flexibility will affect how they function. More

systematic approaches are needed such as comparing combined components to components in isolation and indeed these have begun to appear in the literature (e.g., the Branstetter et al., 2009 comparison of acceptance versus acceptance and values components).

Many of the studies in the present meta-analysis focused on measuring the impact of psychological flexibility components on task persistence and reactivity/distress in the context of various methods to induce distressing thoughts, sensations, and emotions. A broader range of preparations (i.e., laboratory-based procedures and measures) is needed to further evaluate theoretical predictions of how psychological flexibility components impact various outcomes. In addition, there are often no consequences for persisting longer in the laboratory task besides the social demand placed on the participant by the experimenter. While it is reassuring that preparations personally relevant to the population, such as an exposure paradigm for individuals diagnosed with panic disorder (e.g., Levitt et al., 2004), show broadly similar effects, there seem to be few reasons not to use a wider range of preparations of more direct relevance.

Many studies have used reduction in negative emotions and/or sensations as the primary outcome, rather than a change in overt behavior or one's relation to these internal experiences. A psychological flexibility model argues that reduction of the frequency and intensity of distress is most likely to occur over time and in a sustainable way when it is not directly targeted due to dangers of increased focus, unintended suppression, rebound effects and similar issues (Hayes, Strosahl et al., 2011). Thus, these areas are not a primary proximal outcome and a failure to appreciate this issue can lead to comparisons between psychological flexibility components and those drawn from other approaches that are not fully informative regarding the theoretical claims being made. Future research will benefit from testing treatment components with a wider range of theoretically-relevant assessment methods.

An interesting and possibly useful finding was that rationales alone seem to be a weak way to assess the impact of acceptance and mindfulness components. Based on the current meta-analysis, these should generally be considered to be inert. Since this is in itself broadly consistent with the assumptions of experiential methods such as contemplative practice or ACT (Hayes, Strosahl et al., 2011), there seems to be little reason to continue their broad use in laboratory-based component studies in this area. We are unaware of any clinical acceptance and mindfulness protocols that rely on rationales alone. Beginning therapists do often seem to emphasize understanding over experience, however, and the present paper shows why that may be unwise when dealing with these methods.

A major weakness of this meta-analysis is that we did not formally rate the methodological quality of the studies being aggregated. Such rating would be difficult due to the lack of widely promulgated and clear methodological standards for laboratory-based component research, and the resulting lack of relevant detail in methods sections of these studies. In randomized clinical trials, consensus standards such as the CONSORT criteria have evolved but no such consensus yet exists for laboratory-based component studies. ACT researchers have done preliminary work in this area with a list of 19 key methodological issues that should be attended to when doing laboratory-based component studies (e.g., Barnes-Holmes & Hayes, 2003), but these standards have not yet been subjected to formal peer review. Very few of the present set of studies would contain most or all of the suggested methodological controls. The exclusion of non-published studies in the present meta-analysis provided a general method for ensuring at least minimal methodological quality, but also increased the potential for introducing publication biases into the effect size estimates.

Some of the aggregated effect sizes were composed of a small number of studies, which

reduced the power to characterize stable effect sizes for some analyses, particularly when examining components independently. Only gross comparisons could be drawn relative to various comparisons conditions since more fine-grained analyses produced samples too small to be of interest.

The categorization of studies was ad hoc and it is possible that the observed effect sizes for each component may differ somewhat if alternative categorization methods were employed. This is particularly notable with our categorization of metaphor versus experiential conditions, as the inter-rater reliability was somewhat low (.68), requiring supplementary analyses using a combined category as compared to rationale alone. Metaphorical interventions can be very active and the dividing line between that and an experiential process can be difficult to determine. In several studies, the treatment components targeted were not clearly specified in the article, making it more difficult to categorize. When researchers were trying to test a specific component, it was rare that researchers considered whether that component impacted only the expected processes or other processes as well. Sensitive and valid manipulation checks and broader process of change assessment methods are needed to increase the precision and validity of these studies. Many studies presented methods to participants, but did not carefully assess (e.g., through think aloud procedures, Pistorello, 1998) what participants were actually doing as a result. This is a problem since terms like “acceptance” are often not initially clear to participants. More refined comparisons could be made if researchers took more steps to ensure that manipulations did in fact alter the intended processes in participants.

The laboratory-based component strategy is not a call for a return to analog research as a way of testing clinical outcomes. The questions engaged by these research methods are limited to testing theoretical models, rather than the clinical efficacy of interventions. Without clarity on

this point, laboratory-based component studies could easily wander into areas that can only be answered by careful randomized controlled trials of treatment packages with appropriate clinical populations, or by meditational analyses, dismantling studies, and the like. The gap between theoretical principles and applied methods can never be fully closed, and there will always be a need for the test of applied theories to include practical clinical studies. At the same time, given the importance of good theory to scientific progress, theories should be asked to prove themselves across a range of empirical examinations and levels of analysis.

If focused on the limited set of questions for which they are appropriate, the wider use of laboratory-based component studies in CBT seems warranted and should be encouraged. There are myriad questions in the behavioral and cognitive therapy tradition that are deeply theoretical (e.g., how does exposure work; how do thoughts alter the impact of experience; what key cognitive processes are involved in motivation; and so on). In these areas, well-crafted laboratory studies could help refine and test our theoretical ideas. Many theoretically important questions are difficult to ask first in clinical studies because so many variations may need to be examined (e.g., how to combine or sequence components). If a theory does not work in the laboratory, theoretical development is clearly needed. If it does, the positive results of clinical studies can be viewed with more confidence.

The present meta-analysis suggests that the elements thought to contribute to behavior change in contextual ("third wave") CBT work in a way that is broadly consistent with psychological flexibility theory. Many forms of modern CBT, including ACT, use methods that impact these processes (Hayes, Villatte et al., 2011). The evidence reviewed here suggests that the increased attention being given to such processes may be warranted.

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Table 1. *Intervention Coding Results for all Studies*

Article	*N*	Sample Type & Coding	Specific intervention technologies & Modality coding	Component coding	Comparison condition
1) Alberts & Thewissen, 2011	38	University students (C)	Breathing meditation (E)	Mixed Mindfulness	Inactive
2) Arch & Craske, 2006	60	University students (C)	Breathing meditation (E)	Present Moment	Inactive, Fusion
3) Bowen & Marlatt, 2009	123	University student smokers (R/D)	Urge surfing during cue exposure (E)	Mixed Mindfulness	Inactive
4) Branstetter et al., 2009	99	University students (C)	"Tug-of-war with a monster", "Chinese finger trap" (M)	Mixed Mindfulness	Inactive
			& Imagery exercise linking endurance of pain to values (E)	Values + Mindful	
5) Broderick, 2005	177	University students (C)	Mindfulness exercise (E)	Present Moment	Control, Fusion
6) Burns, 2006	205	Univ. students (C) & Pain patients (R/D)	Sensory focus instructions (R)	Present Moment	Inactive, Control
7) Campbell-Sills et al., 2006	60	Current anxiety/mood disorder (R/D)	"Skidding on ice" metaphor (M)	Acceptance	Control
8) Cioffi & Holloway, 1993	63	University students (C)	Sensory focus instructions (R)	Present Moment	Control
9) Cohen et al., 2006	253	Seventh graders (C)	Writing about personally relevant values (E)	Values	Inactive
10) Crocker et al., 2008, Exp. 1	139	University students (C)	Writing about personally relevant values (E)	Values	Inactive
Experiment 2	102	Studt. non- smokers (C) & Smokers (R/D)	Writing about personally relevant values (E)	Values	Inactive
11) De Young et al., 2010	120	University students (C)	Milk, milk, milk exercise (E)	Defusion	Inactive, Active
12) Dunn et al, 2009	89	Community sample (C)	"Emotions like a cloud passing through the sky" metaphor (M)	Mixed Mindfulness	Inactive, Control
13) Eifert & Heffner, 2003	58	Studt. with high anxiety sensitivity (R/D)	Chinese finger trap metaphor (M)	Acceptance	Inactive, Control
14) Erisman & Roemer, 2010	30	Studt. difficulty regulating emotions (R/D)	Breathing meditation, Mindfulness of emotions exercise (E)	Mixed Mindfulness	Inactive
15) Feldman et al., 2010	190	University students (C)	Breathing meditation (E)	Present Moment	Control
16) Feldner et al., 2003	48	Students low in experiential avoidance (C)	Instructions to notice and accept internal experiences related to	Mixed Mindfulness	Control
		& High in experiential avoidance (R/D)	CO2 induction (R)		
17) Forman et al., 2007	98	Students low susceptibility to food (C)	Chocolate cake exercise, "Joe the bum" metaphor,	Mixed Mindfulness	Inactive, Control
		& High susceptibility to food (R/D)	Watching the mind-train exercise (E)		
18) Gutiérrez et al., 2004	40	University students (C)	Contents on cards exercise, "Swamp" metaphor (E)	Values + Mindful	Control
19) Harris & Napper, 2005	82	Univ. Studt. (C) & High drinkers (R/D)	Writing about personally relevant values (E)	Values	Inactive

20) Hayes, Bissett, et al., 1999	32	University students (C)	Mindfulness exercise, Behavioral commitment (E)	Values + Mindful	Inactive, Control
21) Haythornthwaite et al., 2001	42	Adult burn patients (R/D)	Sensory focus instructions (R)	Present Moment	Inactive, Control
22) Heppner et al., 2008	60	University students (C)	Mindfulness raisin eating exercise (E)	Present Moment	Inactive
23) Hofmann et al., 2009	202	University students (C)	Instructions to accept emotions during speech task (R)	Acceptance	Control, Active
24) Hong et al., 2011, Exp 1	21	University students (C)	Mindfulness raisin eating exercise (E)	Mixed Mindfulness	Inactive
Experiment 2	113	University students (C)	Mindfulness raisin eating exercise (E)	Mixed Mindfulness	Inactive
25) Huffziger & Kuehner, 2009	76	Formerly depressed patients (R/D)	Instructions to practice mindful awareness (R)	Mixed Mindfulness	Control, Fusion
26) Kehoe et al., 2007, Exp 1	128	Community sample (C)	“I Cannot Walk” exercise, “Swamp” metaphor (E)	Mixed Mindfulness	Inactive, Control
Experiment 2	39	Community sample (C)	“I Cannot Walk” exercise, “Swamp” metaphor (E)	Mixed Mindfulness	Control
Experiment 3	36	Community sample (C)	Imagery exercise, "Swamp” metaphor (E)	Mixed Mindfulness	Control
Experiment 4	42	Community sample (C)	Imagery exercise (E)	Mixed Mindfulness	Control
27) Kishita & Shimada, 2011	48	University students (C)	"Tug-of-war with a monster", “And/be out”, “Bad cup” (M)	Mixed Mindfulness	Control
28) Kuehner et al., 2009	60	University students (C)	Instructions to practice mindful awareness (R)	Mixed Mindfulness	Control, Fusion
29) Lehmiller et al., 2010	102	University students (C)	Writing about personally relevant values (E)	Values	Inactive
30) Levitt et al., 2004	60	Patients with panic disorder (R/D)	"Tug-of-war with a monster", "Chinese finger trap" (M)	Acceptance	Inactive, Control
31) Logan et al., 1995	164	Dental patients (R/D)	Sensory focus instructions (R)	Present Moment	Inactive
32) Low et al., 2008	51	University students (C)	Instructions to practice mindful awareness (R)	Mixed Mindfulness	Fusion
33) Marcks & Woods, 2005	103	University students (C)	Soldiers in the parade exercise, "Struggling in quicksand" (E)	Mixed Mindfulness	Inactive, Control
34) Marcks & Woods, 2007	79	University students (C)	Soldiers in the parade exercise, "Struggling in quicksand" (E)	Mixed Mindfulness	Control
35) Masedo & Esteve, 2007	218	University students (C)	"Two scales", "Passengers on the bus", Mindfulness exercise (E)	Mixed Mindfulness	Inactive, Control
36) Masuda et al., 2004, Exp 1	8	University students (C)	Milk, milk, milk exercise (E)	Defusion	Inactive
Experiment 2	8	University students (C)	Milk, milk, milk exercise (E)	Defusion	Control
37) Masuda et al., 2010a	103	University students (C) & Students with elevated depression (R/D)	Milk, milk, milk exercise (E)	Defusion	Inactive, Control
38) Masuda et al., 2010b	94	University students (C) &	Milk, milk, milk exercise (E)	Defusion	Inactive, Control

		Students with elevated depression (R/D)			
39) May et al., 2010, Exp 1	48	University students (C)	Breathing meditation (E)	Mixed Mindfulness	Inactive, Control
Experiment 2	49	University students (C)	Body scan mindfulness exercise (E)	Present Moment	Inactive, Control
40) McHugh et al., 2010	24	Normal functioning elderly (C)	Breathing meditation (E)	Present Moment	Inactive
41) McMullen et al., 2008	64	University students/former students (C)	"I Cannot Walk" exercise, "Swamp" metaphor (E) & Instructions to accept pain from shocks (R)	Values + Mindful	Inactive, Control
42) Michael & Burns, 2004	53	Chronic pain patients (R/D)	Sensory focus instructions (R)	Present Moment	Inactive
43) Paez-Blarrina et al., 2008a	30	University students (C)	Linked persisting despite pain to values using examples (M)	Values + Mindful	Inactive, Control
44) Paez-Blarrina et al., 2008b	20	University students (C)	"Swamp" metaphor, Mindfulness exercise (E)	Values + Mindful	Control
45) Roche et al., 2007	20	University students (C)	"Chinese finger trap" metaphor (M)	Acceptance	Control
46) Rogojanski et al., 2011	61	Community sample of smokers (R/D)	Urge surfing during cue exposure (E)	Mixed Mindfulness	Control
47) Sanders & Lam, 2010	60	Previous depressed (R/D) & Never depr. (C)	Mindful self-focus exercise (E)	Present Moment	Fusion
48) Schmeichel, 2009, Exp 1	59	University students (C)	Writing about personally relevant values (E)	Values	Inactive
Experiment 2	72	University students (C)	Writing about personally relevant values (E)	Values	Inactive, Active
49) Sharpe et al., 2010	103	University students (C)	Attention training technique (E)	Present Moment	Control
50) Singer & Dobson, 2007	80	Previously depressed individuals (R/D)	"Thoughts as images on a movie screen" metaphor (M)	Mixed Mindfulness	Inactive,Control,Fusion
51) Spencer et al., 2001	24	University students (C)	Writing about personally relevant values (E)	Values	Inactive
52) Szaz et al., 2011	73	University students (C)	Instructions to accept feelings of anger (R)	Acceptance	Control, Active
53) Takahashi et al., 2002	20	Community sample (C)	Leaves on a stream exercise, Physicalizing exercise (E)	Mixed Mindfulness	Inactive
54) Vernig & Orsillo, 2009	48	Alcohol dependent univ. students (R/D)	"Tug-of-war with a monster", Mindfulness exercise (E)	Mixed Mindfulness	Inactive
55) Vowles et al., 2007	74	Patients with chronic back pain (R/D)	"Running a marathon" metaphor (M)	Acceptance	Inactive, Control
56) Wagener & Zettle, 2011	36	University students (C)	Mindfulness exercise, Defusion exercise (E)	Values + Mindful	Inactive, Control
57) Watson et al., 2010, Exp 1	88	Students with contamination fear (R/D)	Milk, milk, milk exercise (E)	Defusion	Inactive, Active
Experiment 2	100	Students with contamination fear (R/D)	Milk, milk, milk exercise (E)	Defusion	Inactive, Active

R/D = At risk/distressed sample; C = Convenience sample; R = Rationale-alone; M = Metaphor with/without a rationale; E = Experiential exercises with/without metaphors or rationale.

Table 2. *Effect Sizes Comparing ACT Components to Inactive Conditions*

Component	Number of Studies	Effect Size (Hedge's *g*)	95% CI	*z*-Score	Outcomes
All Components	44	.44	.31, .58	6.33***	All outcomes
	28	.68	.50, .85	7.53***	Targeted outcomes
Acceptance	3	.32	-.03, .68	1.80†	All outcomes
	3	.81	.45, 1.18	4.38***	Targeted outcomes
Defusion	6	.74	.37, 1.11	3.90***	All outcomes
	4	.77	.16, 1.37	2.49*	Targeted outcomes
Values	8	.61	.19, 1.04	2.81*	All outcomes
	5	.41	.01, .82	2.00*	Targeted outcomes
Present Moment	8	.22	.03, .40	2.29*	All outcomes
	4	.64	.30, .98	3.74***	Targeted outcomes
Mindfulness Combinations	15	.27	.14, .40	4.04***	All outcomes
	8	.46	.29, .64	5.27***	Targeted outcomes
Mindfulness/Values Combination	5	.78	.19, 1.36	2.59**	All outcomes
	5	1.37	.74, 2.00	4.26***	Targeted outcomes

†$p < .10$, *$p < .05$, **$p < .01$, ***$p < .001$

Highlights

- Meta-analysis of lab-based component studies related to psychological flexibility
- Significant positive effect size for each component compared to inactive conditions
- Larger effect size for theoretically-specified outcomes
- Significant positive effect sizes relative to theoretically distinct interventions
- Larger effect size for conditions with experiential methods than rationale-alone