A Cross-Cultural Incremental Effects Meta-Analysis of Acceptance and Commitment Therapy for Depression: Does Targeting Depression Even Matter?

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Abstract

Acceptance and commitment therapy (ACT) is a transdiagnostic psychotherapy intervention. We sought to demonstrate the degree to which ACT interventions incrementally reduce depression symptoms when they are primary targets, as well as secondary features of other conditions. We reviewed randomized controlled trials (RCTs) for ACT interventions with depression outcome measures. Studies were identified from the comprehensive repository of ACT RCTs. Analyses included data from 21,830 participants (*k* = 263 studies). Overall, ACT interventions resulted in a significant reduction in depression symptoms (*g* = 0.88). ACT interventions targeting depression against passive controls (waitlists) demonstrated strong incremental effects (*g*= 1.10). ACT interventions against passive controls where depression was a secondary outcome demonstrated moderate incremental effects (*g*= 0.61). ACT interventions that targeted depression against active controls demonstrated weak effects (*g*= 0.35). Publication year did not influence outcomes. ACT was only modestly superior to controls in trials conducted in the United States/Canada, relative to trials from other world regions. Higher quality studies were associated with weaker effects. Altogether, ACT is associated with a significant reduction in depression regardless of whether it is targeted. The degree to which ACT is superior to other interventions is small and likely dependent on contextual factors.

Keywords: *Acceptance and Commitment Therapy; Depression; Meta Analysis; Review; International*

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Depression is arguably the most common mental health problem. The World Health Organization (2022) estimates over 280 million people suffer from depression with millions more likely suffering at sub-diagnostic levels (Bertha & Balázs, 2013; Cuijpers et al., 2007; Lee et al., 2019; Wesseihoeft et al., 2013). In the United States alone, the economic cost associated with depression is greater than $210 billion and is increasing (Greenberg et al., 2015, 2021). Depressive disorders, broadly defined as those cataloged in current diagnostic systems, such as major depressive disorder (MDD) and persistent depressive disorder, are also frequently comorbid with other psychiatric problems (Avenevoli et al., 2015; Gorman, 1996; Kaufman & Charney, 2000; Li et al., 2020; Swendsen & Merikangas, 2000) and medical diseases (Gold et al., 2020). For the purposes of the current review, we will refer to all depressive disorders as “depression.”

The high prevalence, yet heterogenous nature, has led to speculation that equifinalic etiologies lead to depression (Borgogna et al., 2024; Cicchetti, 2016; Nusslock & Alloy, 2017; Whitton et al., 2015). That is, multiple paths contribute to a diverse array of depression presentations (Borgogna et al., 2024; Juhasz et al., 2012). For example, both recent divorce (environmental) and taking a new medication (biological) might both lead to depression. Similarly, sometimes weight changes may be associated with depression, and other times attention problems. Presentations might also differ across cultures (Juhasz et al., 2012). As such, transdiagnostic interventions that can account for these diverse etiological paths/presentations are needed.

Acceptance and commitment therapy (ACT; Hayes et al., 2012) is a prime example of such an intervention. ACT is a contemporary cognitive-behavioral psychotherapy with over 1,000 published randomized controlled trials (RCTs; Hayes & King, 2024). ACT is based on a functional contextualist philosophy that links psychological problems to empirically derived interventions via a behavioral account of human cognition known as relation frame theory (RFT; Barnes-Holmes et al., 2000; Fletcher & Hayes, 2005; Guinther & Dougher, 2015; Hayes, 1994, 2004; Hayes et al., 2001, 2012). RFT posits that psychological distress emerges as a byproduct of the human ability to form relations among a near-infinite range of arbitrary stimuli via language (Barnes-Holmes et al., 2000; Fletcher & Hayes, 2005; Hayes, 1994, 2004; Hayes et al., 2001, 2012; Montoya-Rodríguez et al., 2017). For example, from an RFT perspective, depression may be perpetuated by the formation of symbolic relations between an individual's sense of self and thoughts of unlovability or worthlessness, with the literality and rigidity of such relations contributing to ongoing suffering (Fletcher & Hayes, 2005; Hayes, 2004; Hayes et al., 2001; Zettle, 2025). More commonly, ACT is described as a “third wave” cognitive-behavioral intervention (Hayes, 2004; Hayes & Linehan, 2018; McCracken, 2022) that can be contrasted with traditional cognitive-behavioral therapy (CBT or “second wave” behavioral interventions; Hayes, 2004; Hofmann & Asmundson, 2008; McCracken, 2022). From an ACT perspective, human suffering is ameliorated via acceptance and values-consistent action (Hayes et al., 2012), rather than by overt psychological modifications common in other forms of CBT (e.g., challenging “cognitive distortions,” Beck, 2021).

Building upon RFT, the ACT theory of psychopathology, termed the psychological inflexibility model (Hayes et al., 2006), proposes a series of six transdiagnostic psychological inflexibility processes (e.g., experiential avoidance; Boulanger et al., 2010; Hayes et al., 1996; Monestès et al., 2016) thought to underly most general psychiatric problems. It follows that from an RFT/psychological inflexibility perspective, depression might arise from various contexts and comprise individual-specific features (Borgogna & Aita, 2025; Hayes et al., 2012). As such, ACT uses a theoretical framework that attempts to flexibly account for the psychological inflexibility-related mechanisms underlying the diversity of depressive etiologies/presentations rather than applying diagnosis-specific interventions (Zettle, 2025).

Several meta-analyses have demonstrated the efficacy of ACT for reducing various forms of psychological distress (A-Tjak et al., 2015; Fang & Ding, 2023; Gloster et al., 2020; Powers et al., 2009), with robust effects for depressive symptoms (Bai et al., 2020; Williams et al., 2023; Zhao et al., 2023). However, a key limitation across previous ACT-for-depression meta-analyses is that they only examined trials for studies that targeted depression as a latent disease (e.g., MDD; Bai et al., 2020; Williams et al., 2023; Zhao et al., 2023). Current shifts towards dimensional conceptualizations of mental illness, including the Research Domain Criteria (RDoC; Insel et al., 2010), suggest that categorical diagnoses based on syndromal criteria outlined in the *DSM* and *International Classification of Diseases* have poor validity due to the lack of specific disease markers and failure to account for etiological pathways (among many other problems, Insel et al., 2010; Ross & Margolis, 2019). Accordingly, prior reviews that only

evaluate clinical trials for “depression” as defined by specific latent diseases (e.g., “MDD”) represent a narrow proportion of existing evidence.

Not surprisingly, depression symptoms are often assessed as secondary outcomes, even when depression diagnosis was not the primary target (Abdallah et al., 2022; Greer et al., 2012; Simpson et al., 2008). Moreover, there are many different forms of ACT intervention, including individual therapy, group therapy, workbooks, apps, workshops, and many context-specific ACT interventions. Rather than examining one discrete form of intervention, we were interested in examining the entire body of ACT interventions. We believe the research body has evolved enough with >1,000 RCTs (Hayes & King, 2024) that such an approach is appropriate.

Accordingly, we conducted a meta-analysis on the impact of all ACT interventions on all available depression outcomes across published RCTs. We specifically assessed RCTs as we were curious about the degree to which any generalized effect would be specific to ACT. That is, CBT (and other interventions) could also be associated with depression reductions even if depression was not the target by way of common factors (Wampold, 2015) or other potential mechanisms. Specifically, we sought to contextualize the overall ACT intervention effect by comparing it to active (e.g., CBT) and passive (waitlist) controls, referred to as the “incremental effect” (Aita et al., 2023). Based on prior literature (A-Tjak et al., 2015; Fang & Ding, 2023; Gloster et al., 2020; Powers et al., 2009), we assumed ACT would likely be associated with a significant reduction in depressive symptoms when only examining ACT treatment outcomes or when ACT is compared against passive controls. However, we also wanted to know the degree to which the ACT intervention program is associated with an incremental effect

that is unique beyond other treatments. Prior work in this area suggests outcomes are much more modest (Gloster et al. 2020).

We also examined key programmatic moderators that might influence aggregated effects. As ACT is now over 40 years old (Hayes et al., 2022; Hayes & King, 2024; Zettle & Hayes, 1986), publication year was selected as a potential moderator. Similarly, ACT is a popular intervention internationally, with a strong presence in developing nations (Sahdra et al., 2024). As such, we also examined the impact of world region. Finally, as there have been various criticisms of the ACT literature on methodological grounds (Öst, 2008, 2014), we also examined how quality coding moderated treatment effects. These moderators were chosen for their contextual emphasis, which is something valued within the ACT community (Hayes et al., 2012). Understanding how effects change as a function of year, will help us observe historical trends in the effects. Moderation analyses will help us understand if RCTs are demonstrating better effects in certain areas relative to others. Concurrently, an analysis of study quality will help us understand how much we can trust the effects.

Altogether our study can be broken into the following research questions:

1: How effective is ACT as an intervention program at reducing depression? We hypothesized that ACT generally would be associated with a significant reduction in depressive symptoms.

2: How effective is ACT as intervention program at reducing depression when it is not the target problem? Given the transdiagnostic nature of ACT interventions based on RFT/contextual behavior science (Hayes et al. 2012), we hypothesized ACT would be

associated with a significant reduction in depressive symptoms in trials that targeted depression and those that were designed to target other problems.

3: How effective is ACT as intervention program at reducing depression when compared to other active interventions? We hypothesized that ACT would yield significant reductions in depressive symptoms relative to passive controls, but that the effects would be significantly attenuated against active controls. We did not posit a hypothesis regarding whether a significant incremental effect would be observed.

4: How does publication year, trial location, and study quality rating influence ACT's effect on depression? We did not posit specific hypotheses for the moderations analyses, with exception to the general assumption that lower quality studies would likely be associated with better treatment effects.

Methods

Search Strategy

The Association for Contextual Behavioral Science (ACBS) is the formal professional organization associated with ACT/RFT research (contextualscience.org). ACBS provides a citation list of all published RCTs using ACT-based interventions (accounting for secondary publications). The list is comprehensive and has been used for past reviews of ACT literature (Hayes & King, 2024; Sahdra et al., 2024) and can be openly accessed on the ACBS website, which is where we identified all the RCTs for the present study. We reviewed all RCTs available on the list from 1986 through August 2024. Because the ACBS list is comprehensive, we opted against formal search engine methodologies because they would likely miss non-English and/or non-indexed journals.

Additionally, such an approach was not needed, as the ACBS list is recognized as comprehensive (Hayes & King, 2024).

Inclusion/Exclusion

Inclusion criteria included 1) ACT intervention, 2) Comparator (non-ACT) condition, 3) Depression measure included, and 4) Manuscript availability. Exclusion criteria 1) Publication language that was **not** English, Spanish, French, Russian, German, Chinese, Farsi, Korean, or Japanese (this was due to our team's limited proficiency in other languages), 2) Failure to report or provide necessary statistical information (pre/post means, n's, deviation estimates, and/or effect sizes), 3) Non-intervention designs, such as basic science related to ACT constructs, 4) Dismantling studies, and 5) ACT interventions compared to other forms of ACT intervention. Notably, we included all ACT RCTs that were intervention based (e.g., individual, group, etc.) that met inclusion/exclusion criteria. In total, 263 studies were included for analyses. See Figure 1 for a PRISMA flowchart detailing study screening and selection.

Data Extraction

Trained coders extracted 10 articles and then were evaluated to ensure correct estimates were received, 25% of articles were then randomly checked for accuracy and corrected as needed. Basic demographic information (e.g., title, authors) were extracted for descriptive purposes. A second team of investigators then coded each study regarding whether depression was considered the primary target (e.g., "ACT for Depression") or whether depression was considered a secondary target (e.g., "ACT for Pain" with a depression measure included in the study). Coders made this determination upon review of study title, abstract, aims, and methods. Studies the

explicitly identified depression as a primary target were coded “primary”. If the study did not explicate why a depression measure was included or explicitly labeled depression as a secondary target, it was coded “secondary”. We then extracted every available depression measure mean and deviation estimate for ACT and control conditions across all assessed time points. Primary texts and supplemental files were reviewed for statistical extractions. When necessary, authors were emailed in effort to obtain appropriate statistics. For the purposes of the current paper, we only report parametric outcomes specific to depression measures.

Quality Coding

We used an eight-item methodological quality rating scale based on select items from a 22-item psychotherapy outcome methodology rating scale (POMRS) developed by Öst (2008) that was refined by Spencer and colleagues (Spencer et al., 2025). We chose this rating scale over others to facilitate benchmarking with previous ACT meta-analyses that employed the POMRS (Öst, 2008, 2014). See supplemental file 1 for a description of each item. Each item was evaluated using the original rating scale specified by Öst, with 0=poor or missing, 1=fair, and 2=good, with the exception of items #3 (random assignment across conditions) and #8 (clinical significance), which were dichotomized in our modified version (0=poor or missing and 2=good). Quality ratings were coded by research assistants trained in the use of the modified POMRS by the second and third authors, with discrepancies resolved via discussion. Quality coding was done by native language reviewers. Consistent with Öst’s (2008) original instructions for use of the scale, only data provided in the original peer reviewed articles were used to evaluate items.

Analytic Plan

We pooled effect sizes using Comprehensive Meta-Analysis (v4) software (Borenstein et al., 2013). All analyses were modeled under random effects, with subgroup set as the unit of analysis. Hedges' *g* was selected as the effect size to adjust for small sample sizes. Indicators were coded such that positive values represented greater intervention effects for the ACT condition (greater depression *reduction*). All depression measures (e.g., the Patient Health Questionnaire-9; Kroenke et al., 2001) and time points (e.g., seven days, 21 days) were nested within subgroups (set as the unit of analysis). Publication bias was assessed using Egger's regression test. Duval and Tweedie's (2000) trim-and-fill method was used to assess the possibility of missing studies due to publication bias. Tau (τ) estimates, I^2, and Cochran's *Q* were also calculated to assess heterogeneity.

We initially conducted an omnibus meta-analysis across ACT-only conditions to provide a general estimate of ACT efficacy in isolation. During this stage, we conducted a single moderator analysis effect of ACT on whether depression was a primary or secondary target. We then calculated the *incremental* effect sizes of the strength of ACT intervention relative to controls by comparing the change in the ACT condition to the change in the comparator condition from baseline to timepoint of evaluation (i.e., strength of ACT relative to control conditions in reducing depression). Consistent with prior meta-analyses (Borgogna et al., 2025; Pizer et al., 2024), we adopted Ferguson's (2009) recommendation of Hedges' $g \geq 0.41$ as the threshold for minimal "practical" significance. We then conducted incremental effect subgroup analyses based on whether ACT was a primary target. Finally, we conducted secondary subgroup analyses

to examine the strength of ACT intervention relative to active (non-ACT intervention) and passive (waitlist) controls, and meta-regressions examining the effects of publication year, country/region, and summed quality code score.

Results

Study Characteristics

In total, 321 subgroups across 263 studies had sufficient data for analyses. Across conditions, *n*=21,830 baseline participants are represented (48% were assigned to ACT condition). Where applicable, we entered statistical estimates from intent-to-treat analyses. Otherwise, *n*'s were fixed to the number of reported participants at post-treatment and respective follow-up time points. Studies were spread throughout the world, but a plurality were in Europe (k=67, 25%), with Iran having the most for any single country (k=65, 25%).

Total Effect of ACT on Depressive Symptoms

Our initial meta-analysis of the ACT intervention omnibus effect included *k*=605 effect sizes'. Results indicated that ACT interventions were associated with a significant and strong (albeit highly heterogenous) reduction in depression: *g*=0.88, 95% CI: 0.82, 0.95, 95% prediction interval: -0.09, 1.86, τ=0.50, I^2=87%. Egger's test indicated the presence of publication bias (b_0=3.40, *p*<.001) such that smaller studies tended to produce larger effects suggesting a treatment effect. However, the random effects trim and fill procedure did not recommend trimming any studies. When analyzing just studies that targeted depression (*k*=156) depression was significantly reduced by: *g*=1.18, 95% CI: 1.07, 1.29, 95% prediction interval: -0.05, 2.41, τ=0.62, I^2=90%. Egger's test indicated the presence of publication bias (b_0=3.97, *p*<.001) favoring ACT. A significantly

weaker (but still statistically significant) effect was observed for studies that did not target depression (*k*=171): *g*=0.67, 95% CI: 0.61, 0.74, 95% prediction interval: -0.09, 1.44, τ=0.38, I^2=82%. Egger's test indicated the presence of publication bias (b_0=2.58, *p*<.001) favoring ACT.

Incremental Effect of ACT on Depressive Symptoms

Incremental meta-analyses indicated ACT was significantly superior (though highly heterogenous) at reducing depression relative to control conditions: *g*= 0.58, 95% CI: 0.51, 0.67, 95% prediction interval: -0.60, 1.77, τ=0.60, *Q*(320)=2,255, *p*<.001, I^2=86%. Egger's indicated smaller studies tended to produce larger effects favoring ACT, b_0=2.18, *p*<.001, consistent with the corresponding funnel plot (see Table 1). The trim-and-fill procedure did not identify any studies to trim. Fail-Safe *N* analysis estimated an additional 9,680 studies with null incremental effects would be needed to change the effect size to non-significance. When only examining studies that targeted depression (*k*=151), ACT demonstrated a significant incremental effect: *g*= 0.81, 95% CI: 0.67, 0.94, 95% prediction interval: -0.76, 2.40, τ=0.78, *Q*(150)=1,432, *p*<.001, I^2=90%. When only examining studies that targeted non-depression issues (*k*=170), the effect shrank, but remained statistically significant: *g*= 0.41, 95% CI: 0.33, 0.49, 95% prediction interval: -0.45, 1.28, τ=0.44, *Q*(169)=789, *p*<.001, I^2=79%.

Next, subgroup (*k*=157 comparisons) analyses indicated that ACT interventions were superior to passive controls (e.g., waitlists), although this is also qualified by large heterogeneity: *g*=0.89, 95% CI: 0.78, 1.00, 95% prediction interval: -0.36, 2.13, τ=.63, *Q*(156)=1,117, *p*<.001, I^2=86%. Egger's test (b_0=3.08, *p*<.001) and the trim-and-fill procedure were consistent with the omnibus model presented above and visual

inspection of the funnel plot (see supplementary file 2). When compared to active controls (*k*=164 comparisons), ACT continued to evidence a statistically better effect, though it was substantially reduced relative to the passive control model and remained heterogenous: *g*=0.31, 95% CI: 0.22, 0.40, 95% prediction interval: -0.74, 1.36, τ=0.53, *Q*(163)=971, *p*<.001, I^2=83%. The Egger's test (b_0=0.89, *p*=.03), trim-and-fill procedure, and funnel plot (see supplementary file 3) were comparable to the prior omnibus and subgroup analyses.

Next, we conducted a series of nuanced analyses examining the incremental effects of ACT interventions with control type (active vs passive) interacting with depression target status (primary vs secondary). The initial interaction model suggested that ACT intervention had a significant incremental effect relative to passive controls when depression was the target intervention, b_0=.96, p<.001. However, the incremental effect weakened when depression was not targeted (b=-.21, p=.005), and when compared to active controls (b=-.51, p<.001), though still statistically significant in both cases. The R^2 analog accounted for 8% of the additional variance.

Sub analyses were conducted to give exact estimates. Specifically, ACT interventions that targeted depression against passive controls (*k*=92) demonstrated strong significant heterogenous incremental effects *g*= 1.10, 95% CI: 0.93, 1.27, 95% prediction interval: -0.40, 2.59, τ=0.75, *Q*(91)=770, *p*<.001, I^2=88%. ACT interventions that did not target depression against passive controls (*k*=65) demonstrated moderate significant heterogenous incremental effects *g*= .61, 95% CI: 0.48, .75, 95% prediction interval: -0.30, 1.53, τ=0.45, *Q*(64)=308, *p*<.001, I^2=79%. ACT interventions that targeted depression against active controls (*k*=59) demonstrated weak significant

heterogenous incremental effects *g*=.35, 95% CI: 0.14, .56, 95% prediction interval: -1.16, 1.86, τ=0.75, *Q*(58)=518, *p*<.001, I^2=89%. ACT interventions that did not target depression against active controls (*k*=105) demonstrated the weakest (though still significant) heterogenous incremental effects *g*=.30, 95% CI: 0.20, .39, 95% prediction interval: -0.53, 1.12, τ=0.41, *Q*(104)=449, *p*<.001, I^2=77%.

Meta-Regressions

Our first meta-regression examined the relation between publication year and effect sizes. Studies were coded so that 1986 was the anchor (i.e., date of the first ACT RCT; Zettle & Hayes, 1986). Results indicated that publication year was not associated with overall incremental effect, b_0=.34, p=.170, *b*=.01, *p*=.33. A scatter plot of the null effect is available in Table 2. The effect was similarly non-significant when reviewing ACT comparisons against passive controls (b_0=.83, p=.087, *b*=.00, *p*=.91) and active controls (b_0=.09, p=.73, *b*=.01, *p*=.42; see supplemental files 4 and 5 for respective scatter plots). For each model, a heteroscedastic trend was observed such that more recent publications demonstrate more extreme effect sizes', with a slight bias towards effects favoring ACT.

We then examined the role of country/region (see supplemental file 6). United States/Canda was set as the referent. All effects are available on Table 3. Notably, the overall model demonstrated that the incremental effect in the United States/Canada was small (b_0=.29, *p*<.001). However, incremental effects were significantly stronger for studies conducted in East Asia (*b*=.75, *p*<.001), Iran (*b*=.62, *p*<.001), the Middle East/Africa (*b*=.68, *p*=.018), and the British Isles (*b*=.40, *p*=.03). Regions that did not significantly differ from the United States/Canda included Australia/New Zealand, South

America (likely due to power), and studies conducted on the European continent. Despite these differences, country/region accounted for little heterogeneity in model (analog R^2=.01). When comparing ACT to passive controls only, the overall trends mainly held, but all significant effects strengthened, with exception to the British Isles which became non-significant compared to United State/Canada (likely due to power). Moreover, the proportion of heterogeneity accounted for increased (analog R^2=.13). When ACT was compared to active controls, the incremental effects in the United States/Canada became small and non-significant (b_0=.15, p=.106), with only studies in East Asia (b=.70, p<.001) and the British Isles (b=.50, p=.018) demonstrating superior effects. Though, analog R^2 shrank to <01.

We then examined the role of overall quality score. A significant, albeit small, inverse effect was evident such that higher quality scores were associated with lower incremental effects (b_0=1.16, p<.001, b=-.08, p<.001, analog R^2=.03). Visual inspection of the scatter plot revealed notable heteroscedasticity as well, such that lower quality rating is associated with more erratic and extreme effect sizes (see Table 4). The pattern remained at approximately the same strength, though the intercept moved, when observing ACT interventions against passive controls (b_0=1.41, p<.001, b=-.08, p<.001, analog R^2<.01; supplemental file 7) and active controls (b_0=.67, p<.001, b=-.05, p=.006, analog R^2=.02; supplemental file 8).

Discussion

To date, this is the largest meta-analysis examining ACT RCT outcomes for depression. Our findings advance previous meta-analytic work by including all depression outcomes, even from studies that did not target depression. Similar to

previous meta-analyses (Bai et al., 2020; Ferreira et al., 2022; Sun et al., 2022), we observed that ACT was associated with a robust omnibus effect on depression symptom reduction, meaning individuals suffering from depression problems (broadly defined) would likely benefit from ACT intervention (also broadly defined). ACT was associated with significant decreases in depression symptoms even if it was not the target condition. Though notably, the effect was much stronger when depression was the target condition. This observation makes sense in that targeting depression should naturally be associated with symptom reduction. Further, interventions that targeted depression, largely also recruited patients experiencing significant depression problems, thereby giving a larger range for symptoms to be reduced. In studies where depression was not the target, not all participants may have had high enough depression scores for robust symptom reduction to occur.

Not surprisingly, the strength of the ACT intervention effect was stronger than the baseline effect when compared to passive controls (e.g., waitlists) indicating that ACT is preferable to no intervention, and that lack of intervention is associated with a worsening of symptoms. That said, the superiority of the ACT incremental effect was markedly attenuated when compared to active controls. The attenuation worsened when considering incremental depression reductions when depression was not the target problem. While all effects still favored ACT, this effect was below our a priori effect size threshold for clinically meaningful differences. Together these effects suggest ACT is effective, if not slightly better, than most other active interventions, a finding that comports with previous meta-analyses (A-Tjak et al., 2015; Fang & Ding, 2023; Gloster et al., 2020; Powers et al., 2009).

One of the most important qualifications to our interpretations is that variance was high across analyses. Our 95% prediction intervals all included negative values, and in some cases strongly negative *and* positive values. The *Q*, τ, and I^2 heterogeneity statistics were high, suggesting ACT efficacy varies according to study contextual factors. That is, while ACT interventions generally appear to be efficacious, not every ACT intervention will be efficacious in every study or superior to every control.

Moreover, funnel plots and Egger's tests suggest significant publication bias such that smaller studies with strong effects likely bias the overall results towards favoring ACT. This is likely because ACT research is frequently conducted by those identifying with the ACBS (the primary ACT professional community), and is often conducted with limited funds preventing larger samples. This does not completely undermine the overall value of ACT as an evidence-based intervention for depression, but taken with our incremental effect analyses, does suggest that ACT is likely not measurably better than other forms of active intervention (e.g., CBT). Publication bias is ubiquitous in the treatment outcome literature (Van Aert et al., 2019) and not unique to ACT per se, but still an important factor in terms of determining efficacy and expectations for any given patient. Most evidence-based intervention programs face similar challenges. We recommend federal funders from across the globe to support large scale ACT intervention clinical trials (n's >200).

The rise of process-based therapy (another approach led by members of the ACBS community; Hofmann & Hayes, 2019) has brought attention to the problem that most interventions yield similar effects when rigorously controlled. From this perspective, ACT should be studied idionomically. That is, eschewing traditional group

level comparisons associated with RCT methodologies in favor of personalized designs that employ nomothetic statistics post-hoc. For instance, searching for significant mechanism trends across participants, even if individual processes, conceptualizations, intervention kernels, and treatment structure differ drastically patient-to-patient within a clinical trial. ACT, as a therapeutic model, has been employed using a process-based approach (Ong et al., 2024). It is possible, such adaptations will help us better understand how ACT works and which theoretical pieces are therapeutically superior to mechanisms from other intervention frameworks (Borgogna & Aita, 2025).

Our moderation analyses clarified some aspects of the heterogeneity. Publication year was not significantly associated with effect size. The increasing popularity for ACT is reflected in the finding that the majority of RCTs were published relatively recently. However, the scatter plots were heteroskedastic such that effect sizes from recent RCTs varied widely, with several outliers trending towards stronger positive effects. There are likely many reasons for the observed heterogeneity, including small sample designs and weak methodological consistency. Researchers should take steps to identify why certain ACT trials demonstrate extremely strong effects, while others null.

One potential (likely partial) explanation has to do with our next set of analyses. We observed that studies conducted in the US/Canda (where ACT was founded and home to many established ACT labs) demonstrated weak incremental effects. Conversely, publications from most other world regions, particularly the Middle East, Iran, and Asia, yielded significantly stronger effects. These effects were demonstrable even against active controls for East Asia-based publications. Recent research has demonstrated that ACT RCTs from developing countries are less likely to appear in

citation engines, and that altogether little is known about the outcomes of these trials in Western clinical science (Sahdra et al., 2024). Our results show that researchers in these countries are demonstrating successful ACT interventions that appear effective, and perhaps more effective than interventions conducted in Western countries. Learning why these trials demonstrate such robust effects is an important direction for future research.

However, since many of these studies were published in non-indexed journals, there could be concerns about the peer review rigor. Indeed, our quality score moderator analyses suggested that higher quality was associated with a reduction in effect size across all analyses. Further, publications coming from non-Western regions, particularly Iran and East Asia, had much lower quality scores. Thus, the strong effects coming from studies conducted in non-Western regions need to be interpreted with some caution. Discerning the extent to which the favorable incremental effects exist beyond potential methodological explanations is an area worthy of further study. We recommend researchers employ rigorous standards when conducting clinical trials, such as pre-registering aims/hypotheses, making data open-access, and conducting appropriate well-powered statistics. We encourage researchers from non-Western countries to increase the rigor and clarity of their published works, such that it enhances favorable findings opposed to detracting.

Limitations

Our study demonstrates a high degree of heterogeneity in ACT effectiveness. While we explored several moderators, there are many others. For example, different forms of ACT might show differing effects (e.g., group-based vs individual). Future meta-

analyses should consider moderating variables such as session count, outcome measure used, therapeutic modality, among other clinically relevant indicators. ACT effectiveness is also highly contingent upon therapeutic relationships. This factor was not assessed in most studies but could explain why participants have differing reactions. Like all meta-analyses, our results reflect the studies included. Thus, our quality is contingent upon the results of others. We also only examined published studies, which means we did not control for file drawer effects (e.g., unpublished trials, unpublished dissertations, etc.).

Conclusions

ACT interventions are efficacious for depression regardless of whether it is a primary target. However, variation from average estimates were large. ACT researchers in non-Western countries tend to report ACT trials that are more efficacious, but have worse quality scores. Researchers should continue to identify sources of heterogeneity in ACT effects for depression. Clinical scientists are encouraged to conduct more rigorous and larger ACT RCTs employing the principles of transparency and open science in their disseminations.

See Supplemental File 9 for a list of all reviewed studies.

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Figure 1
Study Inclusion Flow Chart

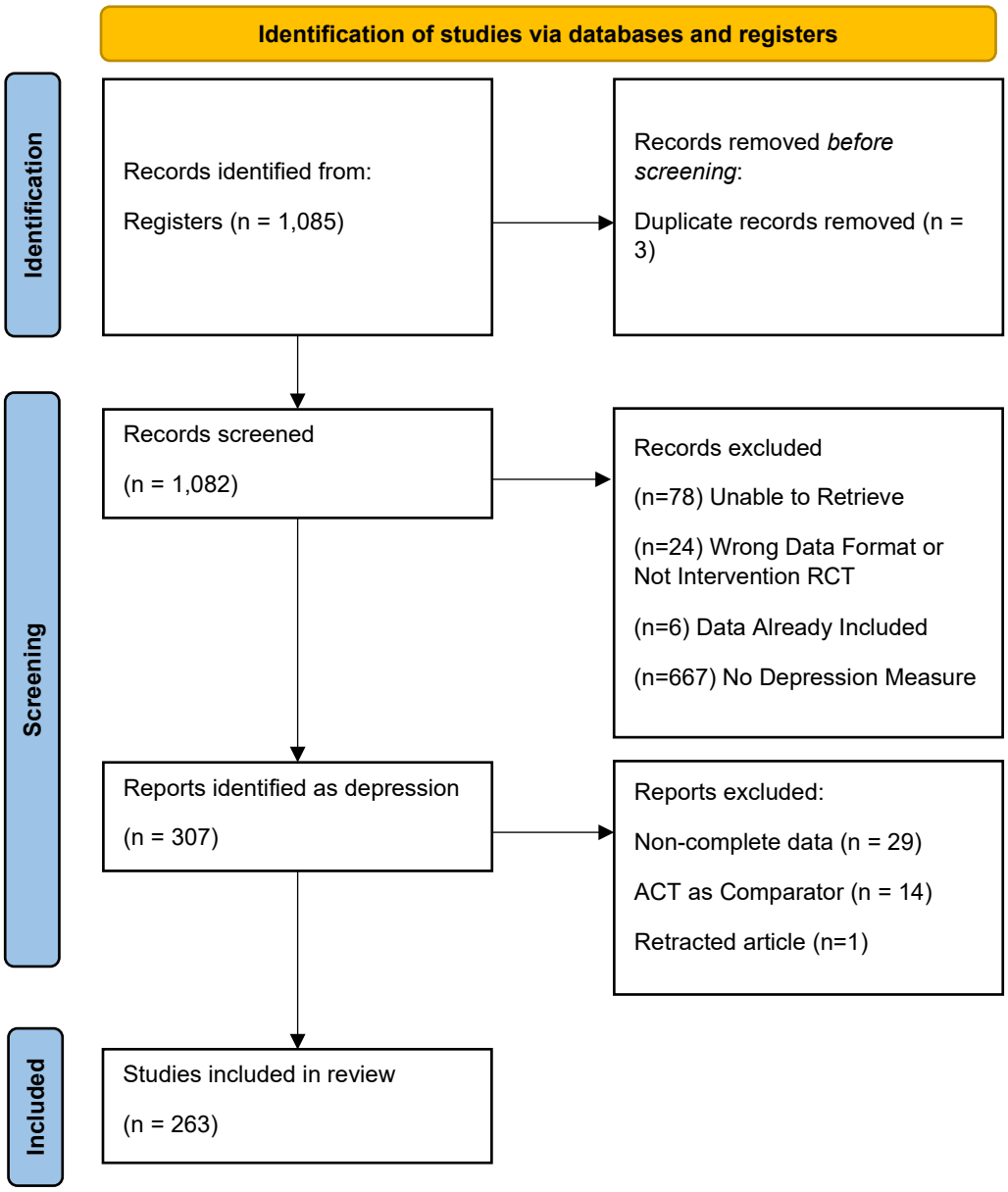

Table 1

Effect of ACT Interventions Compared To All Controls

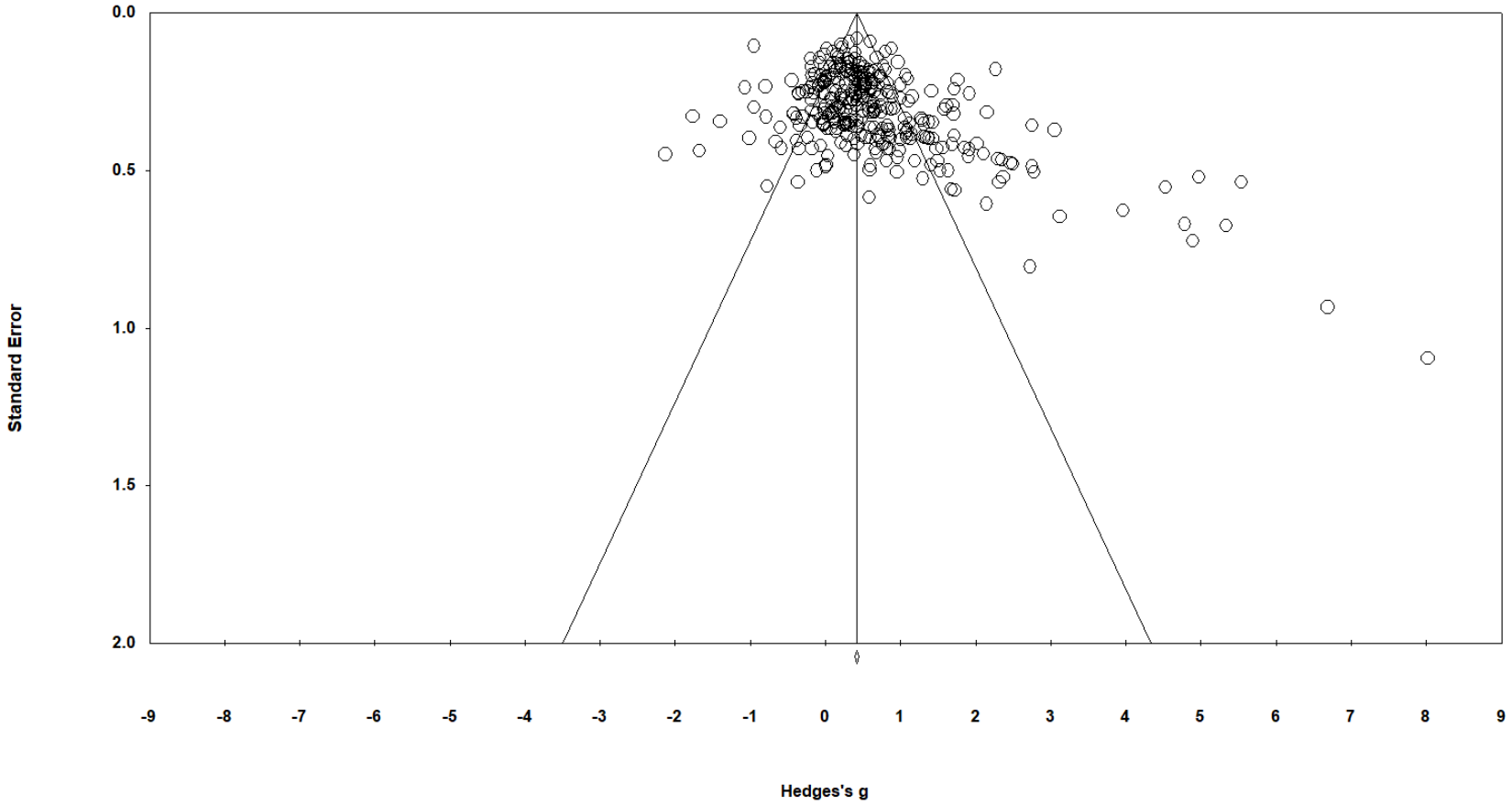

Table 2

Scatterplot of Overall Incremental Effects in Relation to Study Year (1986 coded at 0)

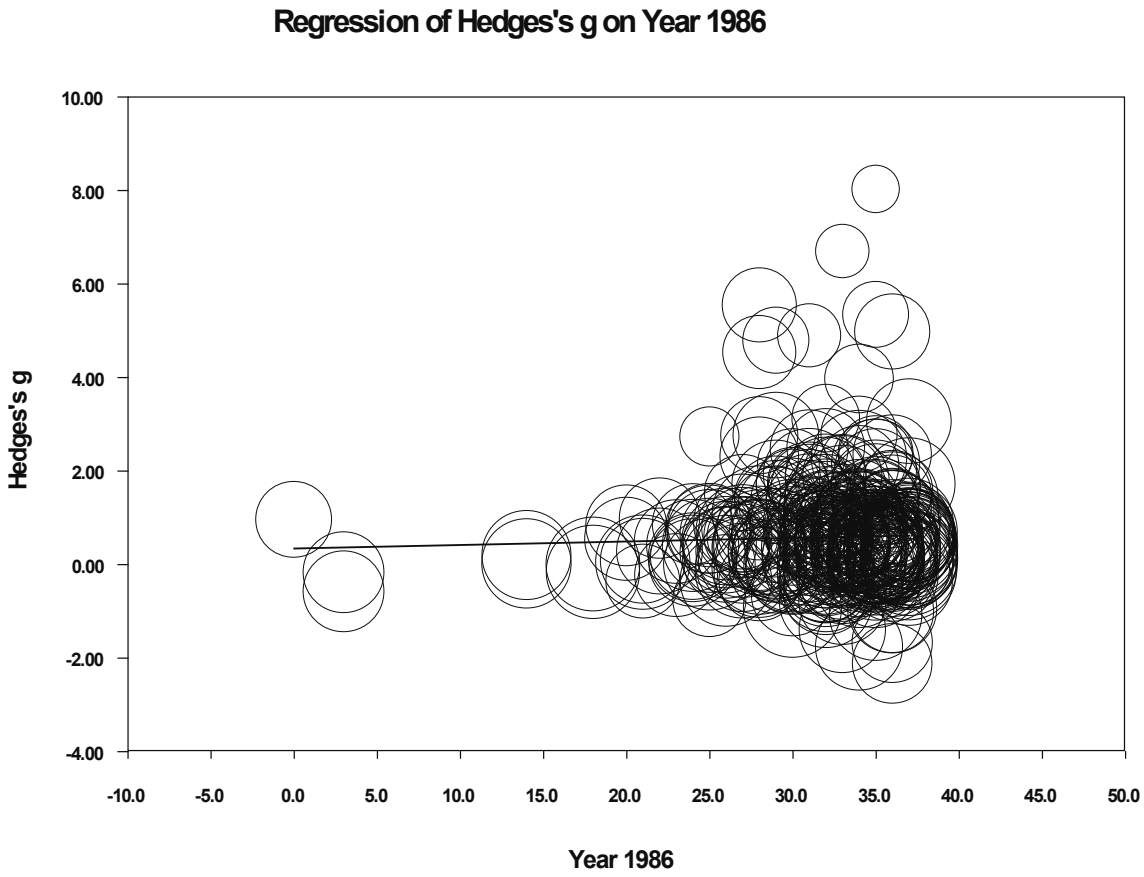

Table 3								
Meta-Regression by Region								
	Overall		Passive Controls		Active Controls		Quality Score	
	B	95% CI	B	95% CI	B	95% CI	Mean	SD
Intercept (United States/Canada)	.29*	.13, .46	.53**	.26, .80	.15	-.03, .34	7.17	2.83
Australia/New Zealand	.01	-.33, .35	-.19	-.63, .25	.04	-.48, .56	7.76	2.84
East Asia	.75**	.49, 1.03	.83**	.39, 1.27	.70**	.39, 1.00	5.89	2.19
Europe	.06	-.16, .28	-.02	-.36, .31	.06	-.19, .32	7.88	2.65
Iran	.62**	.39, .84	.87**	.54, 1.20	-.03	-.33, .26	4.90	2.10
Middle East/Africa	.68*	.12, 1.24	.85*	.03, 1.67	.46	-.23, 1.14	8	3.29
South America	.03	-.65, .71	.34	-.60, 1.27	-.43	-1.33, .46	8.75	2.22
British Isles	.40*	.04, .76	.23	-.35, .80	.50*	.09, .92	6.29	3.20
Note: *p<.05, **p<.001								

Table 4

Scatterplot of Incremental Effects in Relation to Summed Quality Rating