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# Single-Session Acceptance and Commitment Therapy (ACT) Interventions for Patients with Chronic Health Conditions: A Systematic Review and Meta-Analysis

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## Abstract

**Rationale:** Chronic health conditions (CHCs) are costly and difficult to manage. Patients often struggle with behavioral adherence to complex treatment regimens and experience psychiatric distress. Acceptance and Commitment Therapy (ACT) is a transdiagnostic behavioral approach that aims to improve functioning and quality of life (QoL), which are important treatment outcomes for this population. Preliminary efficacy of multi-session ACT in patients with CHCs has been demonstrated, and single-session ACT interventions have since been developed to increase feasibility, acceptability, and accessibility. The purpose of this systematic review and meta-analysis was to describe the literature on single-session ACT intervention studies in CHC populations with regards to (1) study design and methodology, (2) patient characteristics and conditions targeted, and (3) efficacy for outcomes across various domains, using narrative and quantitative methods.

**Methods:** PsycINFO, PubMed, and Web of Science were systematically searched in August 2020. Studies of single-session ACT interventions in adult patients with CHCs that reported quantitative outcomes in any of the following domains were included: (a) functioning and related domains (e.g., disability, QoL, well-being); (b) mental health; (c) physical health; (d) ACT processes. Both controlled and uncontrolled studies were included. Study quality was assessed using the Psychotherapy Outcome Study Methodology Rating Scale (POMRF). Between-group random effects meta-analysis was conducted on general functioning outcomes.

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**Results:** Fourteen manuscripts reporting outcomes from 13 studies ($N$ = 793) met inclusion criteria. Ten studies were identified by their authors as pilot or feasibility trials. Eight studies used comparison or control groups. Twelve studies delivered the ACT content in workshop format. Studies recruited for a variety of conditions. Narrative review found that between- and within-group effect sizes showed generally positive results favoring single-session ACT overall (69%), especially for measures of functioning and related domains (88%), mental health (67%), and ACT processes (73%). Meta-analysis found that ACT did not significantly outperform comparison groups on measures of general functioning (Hedges' $g$: −0.51, 95% confidence interval: [−1.19, 0.16]; $I^2$ = 86%; $K$ = 5) despite a medium-sized pooled effect.

**Discussion:** Use of single-session ACT interventions in CHC populations is an emergent field. There is preliminary evidence for the acceptability, feasibility, and efficacy of these interventions, which provides support for further testing in fully-powered RCTs. Additional RCTs will enable larger meta-analyses and stronger conclusions about efficacy. Recommendations for future trials are provided.

## Keywords

ACT; acceptance and commitment therapy; brief; systematic review; chronic health conditions; workshop; meta-analysis

Physical illness is part of the human experience. While some illnesses are transient and curable, many become chronic and present ongoing challenges to the individual, their families and communities, and the healthcare system. An estimated 60% of Americans have at least one chronic health condition (CHC; Hartzler, Castle, Lewis, & Zakaria, 2020). These conditions account for approximately 75% of healthcare expenditures and can lead to hospitalization, long-term disability, reduced quality of life (QoL), and death (Raghupathi & Raghupathi, 2018). Treatment for CHCs often requires a shift from trying to get rid of the condition to managing it and preventing complications. CHC management often requires health behavior change (e.g., diet, exercise, medication adherence) and ongoing engagement with the healthcare system (e.g., in-office treatments, routine checkups, specialist visits).

Many individuals living with CHCs also experience psychiatric symptoms. Psychiatric distress might be pre-existing and become exacerbated by the condition, appear following diagnosis or CHC-related impairment, or occur as a response to difficulties managing the condition (e.g., diabetes distress; Skinner, Joensen, & Parkin, 2019). For example, the prevalence of comorbid depression ranges from 30–60% in chronic pain (Bair, Robinson, Katon, & Kroenke, 2003), 20–40% in cardiovascular disease (Davidson et al., 2006), 11–30% in diabetes (Anderson, Freedland, Clouse, & Lustman, 2001), and 16–30% in irritable bowel syndrome (Mykletun et al., 2010; Van Oudenhove et al., 2016). Co-occurring depression is associated with more severe medical pathology, poorer prognosis, and higher healthcare utilization compared to having a CHC only (Arnow et al., 2006; Celano & Huffman, 2011; Jeong et al., 2017; Löwe et al., 2008; Merikangas et al., 2007). Similarly, the prevalence of anxiety disorders in CHC is higher than in the general population, including reports of 25–29% in chronic pain (Asmundson & Katz, 2009), 11–14% in cardiovascular disease (Celano, Daunis, Lokko, Campbell, & Huffman, 2016), 14% in diabetes (Grigsby, Anderson, Freedland, Clouse, & Lustman, 2002), and 37% in irritable

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bowel syndrome (Grover et al., 2021). Anxiety is associated with greater pain severity (Csupak, Sommer, Jacobsohn, & El-Gabalawy, 2018), an elevated risk of cardiovascular mortality (Emdin et al., 2016), suboptimal diabetes control (Anderson et al., 2002), and greater impairment in irritable bowel syndrome (Lee et al., 2009).

There is a bidirectional relationship between physical and psychiatric symptoms (Evans et al., 2005). Comorbid psychiatric symptoms and disruptions to daily living contribute to poor QoL in people with CHCs (Megari, 2013). Interventions that maximize self-management behaviors and behavioral activation in the face of complex treatment regimens and accompanying mental health challenges hold promise for maximizing patients' QoL while decreasing healthcare burden.

Acceptance and Commitment Therapy (ACT), a third-wave cognitive-behavioral therapy, is a promising adjunctive treatment for people with CHCs (Dindo, Van Liew, & Arch, 2017). ACT's primary treatment target is not amelioration of symptoms, but rather improved functioning-related outcomes specifically aligned with an individual's values. Though symptom reduction often does occur following ACT treatment (A-Tjak et al., 2015), ACT is particularly well-suited for patients with CHCs, for whom symptom remission may not be a reasonable treatment outcome. ACT is hypothesized to improve functioning and QoL via increases in psychological flexibility. Psychological flexibility is the synthesis of six interrelated processes, which include: (1) acceptance (rather than avoidance) of any and all valanced internal and external experiences; (2) cognitive defusion (that is, de-identification with thoughts and feelings, rather than fusion with or attachment to one's thoughts and feelings as absolute truth); (3) present-moment awareness (rather than maladaptive past- or future-focus); (4) self-as-context (rather than personal identification with one's thoughts or attachment to one's imagined self); (5) defining personally-relevant valued life directions (rather than lack of values clarity); and (6) committed action consistent with those values (rather than inaction or values-incongruent action) (Luoma, Hayes, & Walser, 2007).

ACT is considered a transdiagnostic intervention, as processes can be applied flexibly to address multiple overlapping issues. For example, patients with CHCs may attempt to avoid the disappointment and discomfort associated with having their condition by not taking medication, avoiding activities where modifications must be made or that are related to symptom increase (e.g., physical activity in chronic pain), and engaging in unhealthy coping behaviors (e.g., substance use, overeating). While these behaviors may serve short-term goals (e.g., decreased stress, enjoyment of unhealthy foods, etc.), in the long-term these behaviors paradoxically increase distress and illness severity, and often lower QoL. Personal identification with and attachment to thoughts and feelings about one's condition can be targeted with ACT (e.g., using cognitive defusion and self-as-context exercises) to disrupt these behavioral repertoires. Ruminating thoughts about life before the condition, worries about future complications, or concerns about symptom flare-ups can be addressed using acceptance and present moment awareness exercises. Values identification and committed action exercises can then be used to help patients identify and commit to behaviors that would make their lives personally meaningful given the unique context of their diagnosis and prognosis.

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A prior systematic review of full-length ACT interventions for CHCs found preliminary evidence for efficacy (Graham, Gouick, Krahe, & Gillanders, 2016). However, across the 18 studies reviewed, mean number of treatment sessions was 6.5, with some interventions delivering as many as 11 or 12 sessions. For patients with CHCs, briefer interventions may be more feasible, acceptable, and appropriate, as they can be delivered in traditional healthcare settings and require less resource investment from patients and providers. Many patients live in remote areas, making it logistically and financially challenging to attend multiple sessions. Additionally, the modal number of treatment sessions attended in psychotherapy interventions is one (Gibbons et al., 2011). Thus, using single-session protocols specifically may increase adherence and potentially effectiveness because they ensure every participant receives all intervention components. Although a small narrative review ($k = 6$) of single-session ACT interventions for CHCs found preliminary support (Dindo, 2015), that review was neither systematic nor did it include a meta-analytic component.

The overarching purpose of the present review is to bolster the literature by providing a current, systematic review of single-session ACT for CHC interventions, including preliminary meta-analysis of efficacy. The scope of the present review is broader than prior reviews in its inclusion of both peer-reviewed and grey literature publications. While five included studies were previously reported in Graham et al. (2016) and Dindo (2015), several relevant studies have been published since, including nine studies newly reported here. Primary aims of the present manuscript are to describe the state of the literature of single-session ACT interventions for CHCs with regards to (1) study design and methodology, (2) patient characteristics and conditions targeted, and (3) efficacy for outcomes across various domains, using narrative and quantitative summary methods.

## Methods

This systematic review with meta-analysis was conducted in accordance with the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) guidelines (Moher, Liberati, Tetzlaff, Altman, & Prisma Group, 2009).

### Eligibility Criteria

We used the five PICOS components (participants, interventions, comparators, outcomes, and study design) to design our research question and eligibility criteria (Moher et al., 2009). Studies were required to meet the following inclusion criteria: (P) sample was adults, 18 years and older, with physical illness(es); (I) delivered a single-session intervention based on the ACT model; (C) due to the nascent stage of the literature, both controlled and uncontrolled studies were included; (O) assessed any of the following outcomes quantitatively: functioning and related domains (e.g., disability, well-being, QoL, health behaviors), mental health (e.g., symptom severity, diagnoses changes), physical health (e.g., symptom severity, biomarkers), or ACT processes (e.g., experiential avoidance, psychological flexibility); and (S) used a prospective design. Studies were excluded if: (P) the condition, population, or sole outcome of interest was a health behavior outside the context of a CHC (e.g., physical activity or eating behavior in adults without a discrete

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physical illness) or the intervention was designed to treat caregivers of individuals with a CHC exclusively; (I) a self-help or web-based design was used with no substantive therapist interaction; (O) only qualitative outcomes were reported; or (C/S) study design was cross-sectional, case study, or case series. Only manuscripts published in English were included.

## Information Sources and Search Strategy

The online databases of PsycINFO, PubMed, and Web of Science were systematically searched in August 2020. No search limitations or filters were imposed. We reviewed both peer-reviewed manuscripts and grey literature (i.e., theses and dissertations) for inclusion. No lower limit to year of publication was imposed. Manuscripts were identified using a pre-defined search strategy developed with the assistance of a research librarian. The following search terms were entered, joined by the operator "AND":

1. "acceptance and commitment therapy" OR "acceptance"
2. "brief" OR "single-session" OR "One-day" OR "focused" OR "workshop" OR "FACT" OR "adjunct*" OR "single-session" OR "Pilot" OR "short"
3. "chronic" OR "disease*" OR "health" OR "physical" OR "medical" OR "illness*" OR "condition*" OR "diabetes" OR "cancer" OR "pain" OR "migraine*" OR "HIV"

An example search is included in Supplemental material. Reference lists of included manuscripts, as well as Dindo (2015) and Dindo et al. (2017), were hand-searched. We also searched the Association for Contextual Behavioral Science (http://contextualscience.org) website and contacted the email listserv of that professional organization for peer review of the final inclusion list.

## Study Selection

Study selection proceeded in three stages. In stage 1 (screening), all manuscripts returned from database searches were imported into reference management software. These manuscripts received title/abstract review. Studies that clearly failed to meet inclusion criteria or met exclusion criteria were removed. In stage 2 (selection), remaining studies received a full-text review. Ineligible studies were removed and categorized according to exclusion reason (see Figure 1). In stage 3 (hand-searching), the reference section of included studies as well as Dindo (2015) and Dindo et al. (2017) were reviewed to identify relevant manuscripts not previously identified through database searches. Two independent reviewers (C.D. and M.S.H.) conducted stages 1–3. Disagreements about final study inclusion ($k = 2$; 2%) were discussed in a consensus meeting with other co-authors.

## Data Extraction and Management

EndNote X8 was used to store results from database and hand searches, and to categorize manuscripts according to inclusion and exclusion decisions. Duplicates were removed using the EndNote X8 "remove duplicates" feature and by hand. Study coding and data extraction occurred in Excel. Data extraction was performed by the primary author (C.D.) and verified by co-authors (J.S.W. and M.W.L.).

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Data regarding sociodemographic characteristics (employment, gender, race/ethnicity, education), study population/condition, study design (e.g., pilot, randomized controlled trial), setting, intervention duration, assessment schedule, and intervention and comparator conditions were extracted. Data from functioning and related outcomes, mental health outcomes, physical health outcomes, and ACT process outcomes were extracted. Results from intention-to-treat (ITT) analyses and analyses controlling for relevant covariates were extracted when reported. Where two measures were used to examine the same construct, results from the measure identified by the study authors as primary were extracted. Where outcomes were assessed at multiple follow-up intervals, results from three-month (12-week) follow-up were reported, as this was the most common interval across studies ($k = 10$; 77%). Otherwise, the follow-up interval closest to three months was selected. Authors of included studies were contacted for additional information as needed. Effect size information were calculated by the primary author (C.D.) and confirmed by a co-author (J.S.W.) when not reported. A Cohen's $d$ or Hedges' $g$ value of .2 is considered small, .5 is considered medium, and .8 is considered large. A phi value of .1 is considered small, .3 is considered medium, and .5 is considered large.

## Meta-analysis

Quantitative analysis of treatment efficacy in controlled studies for functioning-related outcomes was conducted using random effects meta-analysis. Functioning-related outcomes were chosen as they are central transdiagnostic treatment targets of ACT. To avoid dependence among multiple effect sizes from the same study, one outcome per controlled study was selected. Where studies reported more than one relevant outcome, the general (rather than condition-specific) measure was selected to reduce heterogeneity.

Analysis was conducted in R version 3.6.1 (R Core Team, 2013) using the metafor and dmetar packages (Harrer, Cuijpers, Furukawa, & Ebert, 2019), using the inverse variance method and Hedges' $g$ as the standardized mean difference. Random effects modeling was used due to considerable statistical heterogeneity among the analyzed effects (i.e., $I^2 > 75\%$, per Cochrane guidelines). All measures included in the meta-analysis were scored such that higher scores indicated poorer results, except for one (the World Health Organization Quality of Life-BREF psychological subscale reported in Dindo et al., 2015), which was reverse scored for consistency of interpretation. Given concerns about inflation in pre-post effect size estimates (Cuijpers, Weitz, Cristea, & Twisk, 2017), within-group effect sizes are depicted graphically but not meta-analyzed.

Statistical heterogeneity was assessed using $I^2$, Cochran's Q-statistic, and $\tau^2$ (using DerSimonian-Laird estimator) (Higgins, Thompson, Deeks, & Altman, 2003). $I^2$ is the percentage of variability in effect sizes due to heterogeneity rather than sampling error. The Q-statistic is the weighted sum of squared differences between individual study effects and the pooled effect, from which $I^2$ is derived. The Q-statistic chi-squared significance test is known to be low-powered for analyses with few studies and should be interpreted with caution (Higgins et al., 2003). $\tau^2$ is another metric of between-study variance in effect sizes (Deeks, Higgins, Altman, & Cochrane Statistical Methods Group, 2019). A prediction interval, which accounts for between-study variance and is less sensitive to number of

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studies than standard heterogeneity estimates, was also calculated (Harrer et al., 2019). Prediction intervals provide a range in which future study effects are predicted to fall based on present evidence in the meta-analysis.

### Individual Study Quality Assessment

Study quality was assessed using the Psychotherapy Outcome Study Methodology Rating Form (POMRF; Öst, 2008). The POMRF is a 22-item measure that assesses methodology and reporting for psychotherapy trials. Each item is rated on a 3-point scale where 0 = *poor*, 1 = *fair*, 2 = *good*. Given that this review includes studies focused on CHC populations rather than strictly psychiatric populations, POMRF items #2 (severity/chronicity of the disorder), #4 (reliability of the diagnosis in question), and #21 (clinical significance) were deemed not directly applicable and were not assessed. For pilot/feasibility studies, item #11 (power analysis) was not assessed. For uncontrolled studies, items #10 (design) and #22 (equality of therapy) were not assessed. Because some items were not assessed and because it is unclear if the total score is a reliable and valid measure of study quality (Liberati et al., 2009), total POMRF scores are not reported. Two reviewers (C.D. and J.S.W.) independently assessed quality of each study and met to reach consensus. Risk of bias was not formally assessed but was approximated at the study level based on whether or not the trial was pre-registered.

## Results

Figure 1 shows the number of manuscripts identified throughout the screening, hand-searching, and selection phases. Half of the manuscripts excluded during the selection phase were ACT interventions for CHC that delivered more than one session ($k = 37$). Fourteen manuscripts reporting results from 13 unique studies met inclusion criteria. Two manuscripts (Dindo, Recober, Marchman, O'Hara, & Turvey, 2014; Dindo, Recober, Marchman, Turvey, & O'Hara, 2012) report separate outcomes from a single study. Five of the 14 included manuscripts (36%; Gregg, Callaghan, Hayes, & Glenn-Lawson, 2007; Sheppard, Forsyth, Hickling, & Bianchi, 2010; Dindo et al., 2012; Dindo et al., 2014; Dindo, Marchman, Gindes, & Fiedorowicz, 2015) were included in prior reviews by Dindo (2015) and Graham et al. (2016). Of the nine newly reported studies, two were RCTs, one was a dissertation, and six were pilot/feasibility trials.

### Quality Assessment

POMRF item scores for each manuscript are presented in Table 1. Overall, studies ensured inclusion of representative samples, used valid and reliable outcome measures specific to the condition and treatment targets, delivered the intervention by clinical psychologists with advanced training, and used appropriate analytic methods with thorough reporting. Most manuscripts presented ITT analysis. Three studies reported both ITT and dropout analysis (Dindo et al., 2018; Gregg, Callaghan, Hayes, & Glenn-Lawson, 2007; Pedersen et al., 2019). All studies reported attrition rates. Rates of attrition for intervention completion (i.e., % of participants that were consented but did not complete the 1-day ACT intervention) ranged from 0% to 35%. The highest attrition rates were in patients with diabetes (35%),

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irritable bowel syndrome (28%), Veterans with chronic pain and traumatic brain injury (26%), and patients with comorbid headache and depression (21%).

The majority of studies with comparison conditions implemented random assignment to treatment and one also randomly assigned to therapist within condition. Two studies reported power analyses (Gregg et al., 2007; Pedersen et al., 2019). Equality of therapy hours in studies with treatment as usual (TAU) control conditions was difficult to assess. Many studies did not provide descriptions of assessor training and blinding, checks for treatment adherence or therapist supervision, or attempts to control for concomitant treatment (particularly other psychotherapy interventions). Many studies used two or more therapists, but none controlled for therapist effects in statistical analyses. Only one study was pre-registered (Dindo et al., 2020b).

## Study Design and Methodology

The design and methodology of each study is described in detail in Table 2 and the specific content of intervention and control conditions is shown in Supplement Table 1. Sample sizes ranged from 15–136 participants ($M = 61$, $SD = 36$). The range of female participant inclusion was 0–100% of the sample ($M = 61\%$, $SD = 32\%$); the range of participants identifying as white was 24–92% of the sample ($M = 61\%$, $SD = 24\%$). Mean sample ages ranged from 33–63 years (median: 46.5; $M = 46$, $SD = 8.5$). Employment rates ranged from 19–89% and unemployment or disability rates ranged from 11–62%.

**Study design.—**Eight of the 13 studies (62%) included a control or comparison condition, all of which used random assignment except one that assigned participants based on availability (Dindo et al., 2014; Dindo et al., 2012). Four controlled studies used medical TAU (Dindo, Marchman, Gindes, & Fiedorowicz, 2015; Dindo et al., 2018; Hadlandsmyth et al., 2019), one used waitlist/medical TAU (Dindo et al., 2014; Dindo et al., 2012), and three provided an active control condition (disease management education in Gregg et al., 2007; enhanced care in Pedersen et al., 2019; support and migraine education in Dindo et al., 2020b). The remaining five studies were single-arm trials.

Ten studies (77%) were identified by their authors as pilot or feasibility studies (Dindo et al., 2015; Dindo et al., 2014; Dindo et al., 2012; Dindo et al., 2018; Dindo et al., 2020a; Ferreira, Gillanders, Morris, & Eugenicos, 2018; Hadlandsmyth et al., 2019; Hou et al., 2017; Huddleston, Martin, Woods, & Dindo, 2018; Sheppard et al., 2010; Welch, 2014), suggesting that assessing feasibility and acceptability were primary aims for those trials. Five of 10 (50%) pilot/feasibility studies reported qualitative feedback regarding feasibility, acceptability, or treatment satisfaction, with generally positive feedback (Dindo et al., 2015; Hadlandsmyth et al., 2019; Hou et al., 2017; Huddleston et al., 2018; Welch, 2014). Among those studies, common reasons for eligible individuals declining participation included practical constraints such as time and distance (47% in Hou et al., 2017; 18% in Hadlandsmyth et al., 2019) and a general sense of overwhelm (33% in Hadlandsmyth et al., 2019).

**Study methodology.—**Twelve studies (92%) delivered the ACT intervention in workshop format and one delivered it in an individualized session (Hadlandsmyth et al., 2019). Two

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studies provided individualized follow-up calls after the workshop as part of the intervention (Dindo et al., 2018; Ferreira et al., 2018), and one included a self-guided bibliotherapy component (Ferreira et al., 2018). Across all studies, average single-session ACT intervention length was 5.5 hours, with a range of two to eight hours. Median and modal length was five hours. Likely due to the short duration of the intervention, only one study (8%; Welch, 2014) assessed outcomes at 'post-treatment' (i.e., directly after the single-session intervention). The modal follow-up period for assessment of post-treatment outcomes was three months ($k$ = 10, 77%). Four studies (31%) assessed outcomes beyond three months: six months in Dindo et al. (2015), Ferreira et al. (2018), and Dindo et al. (2020b), and six, 14 and 20 months in Pedersen et al. (2019). Seven manuscripts (54%) explicitly indicated where the intervention was delivered. All interventions were delivered in-person. All interventions but two were delivered in medical settings.

## Patient Characteristics and Conditions

Across 13 studies, three (23%) targeted migraine, two (15%) diabetes, two (15%) primary gastrointestinal conditions, one (8%) multiple sclerosis, one (8%) multiple functional somatic syndromes, one (8%) general chronic pain and mild traumatic brain injury, and one (8%) recruited patients characterized as "at risk" for vascular disease. Two studies (15%) targeted patients undergoing surgery who were identified *a priori* as being at risk for post-surgical pain and/or chronic opioid use.

Eight studies (62%) required participants to meet a minimum threshold of comorbid psychiatric distress for inclusion: three required participants to meet DSM-IV criteria for a current major depressive episode, three required participants to meet established cut-offs for clinically significant depression or anxiety symptomatology, and one required participants to meet DSM-IV criteria for either current major depressive disorder, generalized anxiety disorder, or DSM-5 criteria for posttraumatic stress disorder (PTSD). One study required significant condition-related distress (Welch, 2014). In samples from studies not requiring co-morbid psychiatric distress for inclusion, rates of clinically significant depression and anxiety ranged from 17–53% and 16–30%, respectively. One study did not report mental health comorbidities (Gregg et al., 2007).

## Narrative Review: Outcomes Assessed and Intervention Efficacy

For the purpose of this review, treatment outcomes were categorized into four domains: (a) functioning and related domains; (b) mental health; (c) physical health; and (d) ACT processes. Given that pilot/feasibility trials do not have adequate power to detect statistically significant effects, results from statistical significance testing are reported for non-pilot trials only. Within-group effect sizes are reported to assess preliminary efficacy in pilot/feasibility trials.

**Functioning and related domains.—**Thirteen manuscripts (93%) reported 27 outcomes in this domain, including daily functioning, functioning in specific life domains, disability, QoL, healthcare utilization, pain interference, medication use and cessation, and disease self-management, all of which were measured using self-report instruments. Specific constructs and measures are listed in Table 2. Of the 27 outcomes assessed across all studies,

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approximately half were condition-specific and half were general to health and functioning. In the three non-pilot trial manuscripts that assessed this domain, four of seven outcomes (57%) showed significantly greater improvement in the ACT group than the control group, and one (14%) showed a trending significant ($p = .06$) outcome. In the 10 pilot/feasibility trial manuscripts that assessed this domain, 20 of 20 outcomes (100%) showed medium-to-large effect sizes. Effect sizes varied within and across studies (see Table 2). In total, 24 of 27 outcomes in this domain (88%) showed results favoring ACT.

**Mental health.—**Eleven manuscripts (79%) reported 24 mental health outcome effects total, including depression ($n = 10$), anxiety ($n = 7$), stress ($n = 2$), PTSD symptoms ($n = 1$), GI-specific anxiety ($n = 1$), and general or composite mental health status ($n = 3$). In two non-pilot trial manuscripts, one of six outcomes (17%) showed significantly greater improvement in the ACT group (depression symptoms in Dindo et al., 2020b). In nine pilot/feasibility trial manuscripts, 15 of 18 outcomes (83%) showed medium-to-large effect sizes. Those that did not were depression and anxiety in breast cancer patients (Hadlandsmyth et al., 2019) and general mental health status in adults with multiple sclerosis (Sheppard et al., 2010). In total, 16 of 24 outcomes in this domain (67%) showed results favoring ACT.

**Physical health.—**Ten manuscripts (71%) reported 13 physical health outcome effects total, including general physical health status ($k = 3$), raw HbA1c and diabetes control as assessed by HbA1c ($k = 1$), headache frequency and severity ($k = 1$), inflammatory bowel disease symptoms ($k = 1$), irritable bowel syndrome symptom severity ($k = 1$), pain cessation ($k = 1$), and pain severity ($k = 2$). In three non-pilot trial manuscripts, one of five outcomes (20%) was statistically significant in favor of the ACT group (greater increase in proportion of glycemic control in Gregg et al., 2007). In seven pilot/feasibility trial manuscripts, three of eight outcomes (38%) showed medium-to-large effect sizes: headache frequency and severity (Dindo et al., 2014), inflammatory bowel disease activity (Hou et al., 2017), and irritable bowel syndrome symptoms severity (Ferreira et al., 2018). In total, four of 13 outcomes in this domain (31%) reported results favoring ACT.

**ACT processes.—**Eight manuscripts (57%) reported 11 ACT process outcomes, including experiential avoidance, acceptance, psychological flexibility, and engagement in values-consistent behavior. Two studies administered the general Acceptance and Action Questionnaire-II (Bond et al., 2011), and six administered condition-specific AAQ variants. Two manuscripts examined three ACT-adjacent process constructs: thought suppression, “mindful attention awareness,” and pain catastrophizing (Hadlandsmyth et al., 2019; Sheppard et al., 2010). In one non-pilot trial, the ACT group reported significantly greater increases in diabetes acceptance at three-month follow-up than a diabetes management education condition (Gregg et al., 2007). In eight pilot/feasibility trial manuscripts, seven of 10 outcomes (70%) showed medium-to-large effect sizes. Small effects were detected for pain acceptance and values-consistent behavior among Veterans who underwent orthopedic surgery, despite greater rates of pain cessation and opioid cessation (Dindo et al., 2018), and for pain acceptance in female breast cancer patients who underwent breast surgery (Hadlandsmyth et al., 2019). Medium-to-large effect sizes were shown for thought suppression among patients with multiple sclerosis and patients with diabetes (Sheppard et

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al., 2010; Welch, 2014). Small effects were shown for mindful attention awareness among patients with multiple sclerosis (Sheppard et al., 2010) and pain catastrophizing among breast cancer patients (Hadlandsmyth et al., 2019). In total, eight of 11 outcomes in this domain (73%) showed results favoring ACT.

Three manuscripts conducted some form of mediational analyses using ACT process measures. Gregg et al. (2007) found that changes in diabetes acceptance and self-reported self-management behavior mediated impact of treatment on changes in HbA1c in patients with diabetes mellitus type 2. Using a proxy mediation method, Dindo et al. (2015) found that psychological flexibility (measured by the Experiencing Questionnaire) partially mediated reductions in depressive symptoms among individuals at risk for vascular disease with clinically significant anxiety or depression. Finally, Ferreria et al. (2018) used hierarchical multiple regression to examine the unique contribution of changes in irritable bowel syndrome acceptance (pre-treatment to post-treatment) in accounting for variance in change in outcome measures (pre-treatment to follow-up). They found that changes in irritable bowel syndrome acceptance significantly predicted changes in all outcomes, even when accounting for symptom severity.

## Meta-analysis: Outcomes Assessed and Intervention Efficacy

Figure 2 shows effect sizes and meta-analytic results for five of eight controlled studies (63%) reporting a functioning-related outcome. Four studies assessed general functioning using the World Health Organization Disability Assessment Schedule II (WHODAS; Rehm et al., 1999). The fifth study (Dindo et al., 2015) assessed general well-being using the World Health Organization Quality of Life-BREF (WHO-QoL-BREF; Skevington, Lotfy, & O'Connell, 2004), from which scores on the psychological well-being subscale were used. Effects were estimated using data from three-month follow-up for four studies and six-month follow-up (the shortest available) for one study (Pedersen et al., 2019). Random effects meta-analysis found that ACT did not significantly outperform comparator groups on these outcomes (mean Hedges' $g = -0.51$, 95% CI [−1.19, 0.16], $p = .14$) despite a medium pooled effect size. Study effects displayed considerable heterogeneity ($I^2 = 86\%$; Q [$df$=4]: 28.84, $p < .001$; $\tau^2 = 0.50$). The prediction interval shown in Figure 2 crosses 0, which does not suggest that future effects are expected to favor ACT. Visual examination of the funnel plot (Supplement Figure 1) and Eggers' test of funnel plot asymmetry did not indicate the presence of publication bias ($p = .20$).

The nine within-group effects for functioning and related domains across all trial types are depicted graphically in Figure 3. Of nine effects, five were from the WHODAS (Dindo et al., 2012; Huddleston et al., 2018; Pedersen et al., 2019; Dindo et al., 2020a; Dindo et al., 2020b), one was from WHO-QoL-BREF (Dindo et al., 2015), one was from the Quality of Life Inventory (Sheppard et al., 2010), and two were from measures of condition-specific health-related QoL (the Short IBD Questionnaire in Hou et al., 2017; IBS Impact on Quality of Life Scale in Ferreira et al., 2018). Effect sizes were medium to large, with two studies clearly favoring ACT.

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## Discussion

To our knowledge, this is the first systematic review with meta-analysis of single-session ACT interventions in CHC populations. Our goal was to take a systematic approach to describing the state of the literature and to use both narrative and quantitative methods to estimate the efficacy of single-session ACT for CHC. Our search yielded 14 manuscripts (from 13 unique studies), nine of which were not included in prior reviews. Ten of the 13 included studies were pilot/feasibility trials and quality of all studies was variable, underscoring the nascent nature of this literature. Overall, efficacy results were promising, but varied by condition and population as well as specific outcome type. Narrative review of ACT efficacy found significant results in RCTs or medium-to-large effect sizes in pilot/feasibility trials for the majority of outcomes (69%) across the four outcome domains of interest. We found that all four studies that required participants to meet DSM diagnostic criteria for a psychiatric condition reported significant or large effect size improvements in mental health symptoms and significant or medium-to-large effect size improvements in functioning and related domains. Contrary to conclusions in Graham et al. (2016), which noted that intervention duration should be longer because CHC can be long-standing, severe, and accompanied by significant psychiatric issues, there is promise for single-session ACT in CHC, including when psychiatric distress is prominent.

For ACT process outcomes, acceptance, psychological flexibility, and values-consistent behavior were most commonly assessed, and often measured with condition-specific measures, in line with recommendations (Ong, Lee, Levin, & Twohig, 2019). Although the current review focused on direct influences of treatment on ACT process outcomes, the three studies that conducted some form of mediational analysis found evidence for partial or proxy mediation. Assessing purported mechanisms is essential for optimizing behavioral interventions for CHC populations, especially in RCTs where multiple active intervention types may be compared.

Results from meta-analysis found a medium-sized effect of single-session ACT on functioning and well-being, though the pooled estimate was not statistically significant. These results should be interpreted tentatively, since two of the five meta-analyzed effects came from pilot/feasibility trials, and the five effects displayed considerable heterogeneity. Ultimately, pilot trial results inform treatment acceptability and feasibility rather than efficacy (Bowen et al., 2009). Thus, results from pilot RCTs should be reproduced in fully powered RCTs. Data from additional RCTs will better inform the efficacy of single-session ACT for people with CHC.

### Recommendations for Future Studies

While current review findings suggest promise for this treatment approach, additional high-quality trials are needed to bolster the evidence. Below we offer multiple considerations for enhancing the literature in this area, from general strategies to those specific to the single-session ACT format.

1. Given the variability of study quality and the preponderance of pilot/feasibility studies, additional well-designed RCTs are needed to fully examine the efficacy

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of single-session ACT for CHC. Such studies should include pre-specified hypotheses and outcomes, *a priori* power calculations, adequate sample size, blinded assessors, and therapist adherence and competency ratings. RCTs should also compare single-session ACT to an active control condition (e.g., education, relaxation) and ensure equivalence in length, intensity, therapist proficiency, and any additional treatment components (e.g., booster sessions).

2. Only four of the reviewed studies included follow-up periods beyond three months. Future studies would benefit from multiple follow-up assessments (perhaps up to one year) to better examine the long-term efficacy of single-session ACT. Other ACT research has emphasized the importance of longer-term follow-up periods for capturing unexpected increases in improvement over time as well as maintenance of treatment effects compared to comparator conditions (Clarke, Kingston, Wilson, Bolderston, & Remington, 2012; Gifford et al., 2011; González-Menéndez, Fernández, Rodríguez, & Villagrá, 2014). Increasing the follow-up period will inform the relative efficacy of single-session ACT and whether it can be offered as a standalone intervention or should be offered as part of a more comprehensive treatment package.

3. Several studies used general measures of functioning that assess outcomes less amenable to behavioral interventions such as ACT (e.g., mobility and self-care in the WHODAS), whereas others used condition-specific measures, which are ostensibly more sensitive to outcomes of interest to patients and providers. Selecting validated and appropriate measures of functioning and QoL as the primary outcome for single-session ACT studies of CHC is critical to establishing efficacy. Future studies should use both general and condition-specific measures when possible. Further, all included studies used self-report measures of functioning, most of which rely on retrospective recall. Investigators may consider using novel assessments of functioning that are ecologically valid and less prone to social desirability and recall biases, such as ecological momentary assessment (EMA) and accelerometry or other behavioral measures relevant to the target CHC.

4. There was variability in participation and dropout rates across the reviewed studies, especially high for conditions with psychiatric comorbidity or greater levels of disability (e.g., multiple sclerosis). Given that single-session interventions can facilitate access to treatment, virtual delivery may further enhance reach. Virtual delivery may increase willingness and ability to participate and may be less distressing for some individuals. Virtual delivery may also increase access among underserved populations.

5. Single-session interventions deliver a great amount of information in a short period. Traditional post-treatment assessments were uncommon in included studies and may not be psychometrically appropriate if they are not sensitive enough to capture change after a very short test-retest latency period. Therefore, incorporating a participant comprehension check should be considered. Instituting a comprehension check both immediately following the intervention

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and at follow-up may provide information that can be used to optimize single-session delivery. A recently developed measure that may be suitable for this purpose is the ACT-SQ, which captures how well ACT processes were realized during a treatment session (Probst et al., 2020).

6. Patient characteristics and the clinical complexity of CHCs varied widely and may have contributed to modest outcomes in some of the included studies. Examining clinical variables as moderators of treatment outcome can shed light on whether single-session ACT is adequate for those with greater disease burden. Additionally, the majority of participants in most studies were white. Given the higher prevalence of some CHCs in racial and ethnic minority groups, future studies should assess these variables as potential moderators to inform culturally sensitive approaches.

7. Only nine of the 14 manuscripts included ACT process measures, despite the importance of examining the impact of treatment on the purported processes of change. We strongly encourage all future single-session ACT studies to include multiple measures of ACT processes, preferably both condition-specific and general, and conduct adequately powered mediation analyses where possible. Newer measures that assess multiple ACT processes in one questionnaire may be appropriate. This is an important step to identifying underlying treatment processes that predict treatment outcomes.

8. Only two of the included studies delivered supplementary components (i.e., booster session in Dindo et al., 2018; bibliotherapy and support calls in Ferreira et al., 2018), and both demonstrated robust treatment findings. Future studies should specifically test whether adjunctive intervention components increase efficacy. This may include telephone- or text-based booster sessions, ACT-based smartphone applications or websites, and/or peer-support groups. Future studies also may consider implementing a multiphase optimization strategy (MOST; Collins, 2018) to better understand the briefest and most cost-effective intervention that still achieves efficacy. Relatedly, stepped-care approaches for treatment of non-responders could evaluate outcomes following a single-session intervention and provide additional intervention (e.g., full-length ACT) as indicated.

Additionally, future pilot or feasibility studies should consider the recommendations of Bowen et al. (2009) to assess outcomes in eight domains: acceptability, demand, implementation, practicality, adaptation, integration, expansion, and limited-efficacy testing. Future randomized controlled trials incorporating the above recommendations will answer important questions regarding for whom single-session ACT may be efficacious, the optimal delivery method and intervention design for specific populations, and the potential role of single-session ACT in treatment of CHC populations, for example, as “first-line” treatment in stepped-care approach.

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### Review Limitations

In terms of assessing study quality, there is no ideal tool for evaluating single-session psychotherapy studies, nor are we aware of a single tool that adequately assesses the quality of both pilot/feasibility studies and RCTs. The POMRF was chosen because its items assess a range of nuanced design and reporting features that are important for establishing strong empirical evidence for behavioral intervention efficacy. We modified the POMRF for our purposes, following precedent from a related review (Graham et al., 2016). However, the reliability and validity of the POMRF have not been rigorously evaluated and it has been criticized for not assessing process-based therapy variables that are especially important in ACT (Atkins et al., 2017). We encourage the development of validated quality assessment tools appropriate for brief psychotherapy interventions.

Not all controlled studies reported functioning-related outcomes, which limited the size of the meta-analysis to five effects. These five effects demonstrated considerable heterogeneity, which could not be explored using meta-regression or subgroup analyses due to the small size of the analysis. Four of the five meta-analyzed effects came from studies conducted by the same primary author, which may introduce bias. As evidence continues to accumulate, future meta-analyses should explore possible sources of heterogeneity based on both clinical and methodological factors. For example, Hadlandsmyth et al. (2019) was an outlier among included studies in terms of delivery method (individualized rather than group-based) and duration (two hours versus the modal five hours), and showed small effects across all outcomes. Meta-regression of multiple studies with varying design qualities will allow for more definitive conclusions about efficacy and optimal delivery.

It is also important to note that general (rather than condition-specific) measures of functioning and related domains were meta-analyzed to reduce statistical heterogeneity. However, condition-specific measures may be more sensitive to change. Future studies and larger meta-analyses should include both condition-specific and general measures of functioning that are sensitive to the intervention.

We calculated between- and within-group effect sizes for studies where these were not reported in the manuscript. However, these within-group effect sizes did not account for the dependence between pre- and post-test scores, which may lead to inflated estimates (Cuijpers et al., 2017; Cheung, 2019). Future studies should report appropriate effect sizes in addition to statistical significance testing. Finally, the present review did not formally assess risk of bias across studies. Of the 13 studies reviewed, only one was pre-registered, which precludes assessment of selective reporting. The funnel plot of meta-analyzed effects did not suggest publication bias, and the inclusion of grey literature should reduce publication bias in the present review. However, it will be important to systematically assess whether pre-specified analyses are reported in future reviews.

Despite these limitations, the current review has several strengths. It provides an up-to-date and broadly scoped synthesis of single-session ACT for CHC studies and is the first to meta-analyze treatment outcomes. We reported grey literature results, various outcome types, and provided detailed description of study design and intervention characteristics.

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### Conclusion

Brief interventions hold promise for increasing access to behavioral interventions among CHC patients, who tend to have a greater burden of healthcare needs than other psychotherapy consumers. Our systematic review identified 13 studies collectively assessing the feasibility, acceptability, and efficacy of single-session ACT as delivered to individuals with various CHCs. Studies generally reported results favoring ACT, especially in functioning and related domains, which is the primary target of ACT. However, there were few RCTs, and sample sizes were relatively small. Given the relatively limited dose of treatment delivered in a single session, and the likelihood that patients with CHCs have complex clinical presentations, it is critical to further evaluate the efficacy of brief interventions to better inform treatment design and delivery, especially in traditional and integrated healthcare settings.

## Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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**Highlights:**

- ACT is used to improve functioning in chronic health condition populations
- Single session ACT may be more feasible and acceptable than multi-session ACT
- Single-session ACT appears feasible and beneficial across several chronic health conditions
- Meta-analysis found a medium-sized, non-significant effect on general functioning
- Larger, randomized controlled trials are warranted to test efficacy

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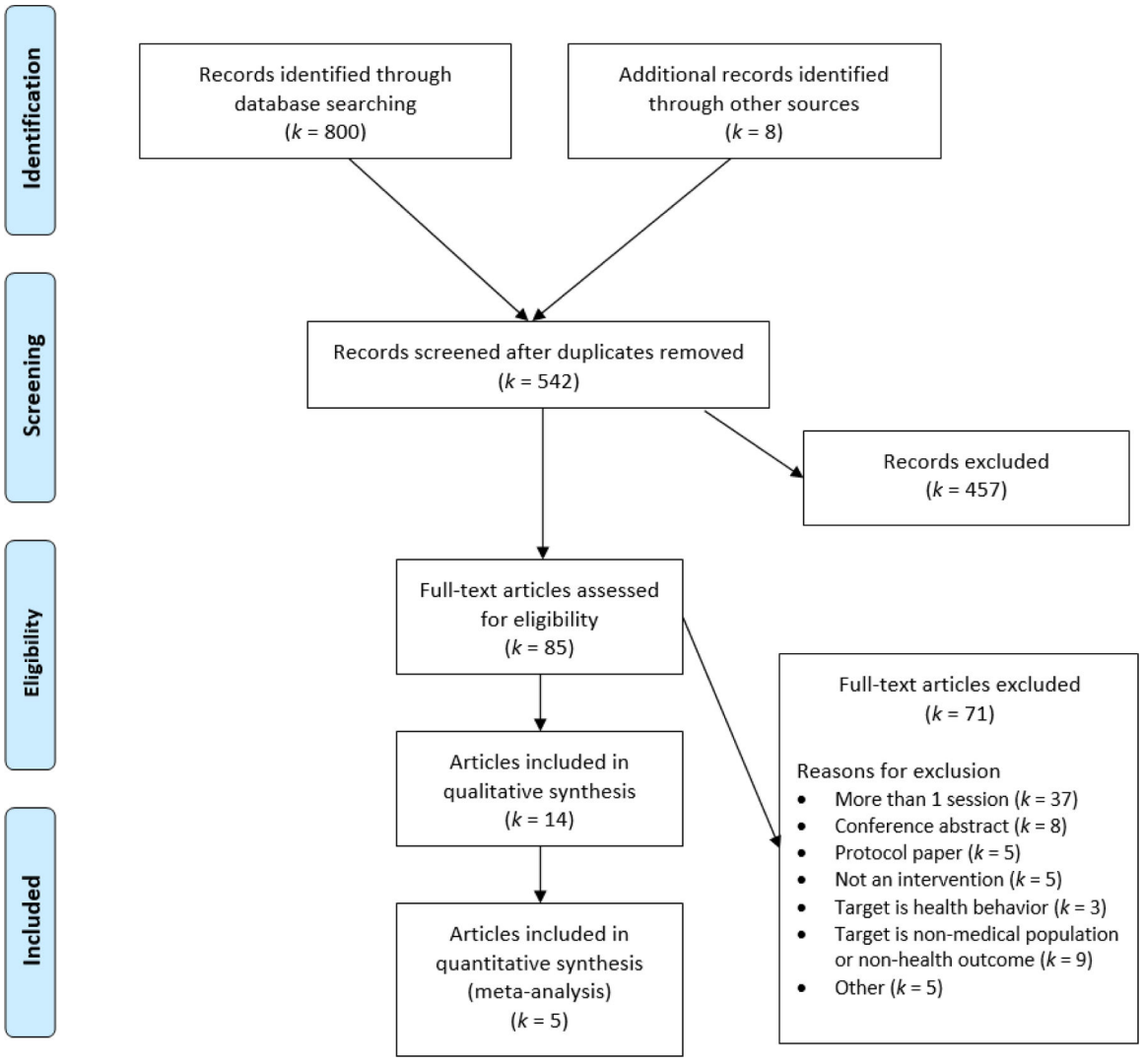


**Figure 1.**
Study screening and selection flowchart (adapted from Moher et al., 2009).

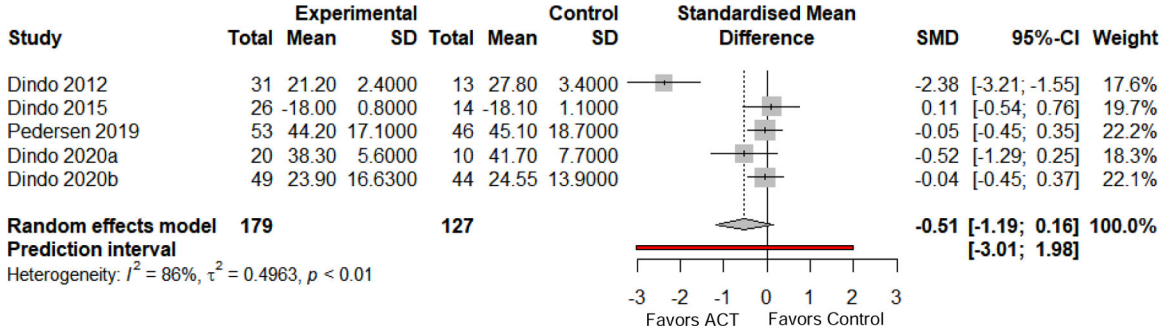


**Figure 2.**

Between-group meta-analysis results and forest plot for **functioning-related outcomes** at **follow-up**. Functioning-related outcomes included measures of functioning and well-being. Where studies reported more than one relevant outcome, the general (rather than condition-specific) measure was selected to reduce heterogeneity. Error bars are 95% confidence intervals; dashed line is pooled effect size; red line is prediction interval. SD = standard deviation. SMD = standardized mean difference, calculated as Hedges' *g*.



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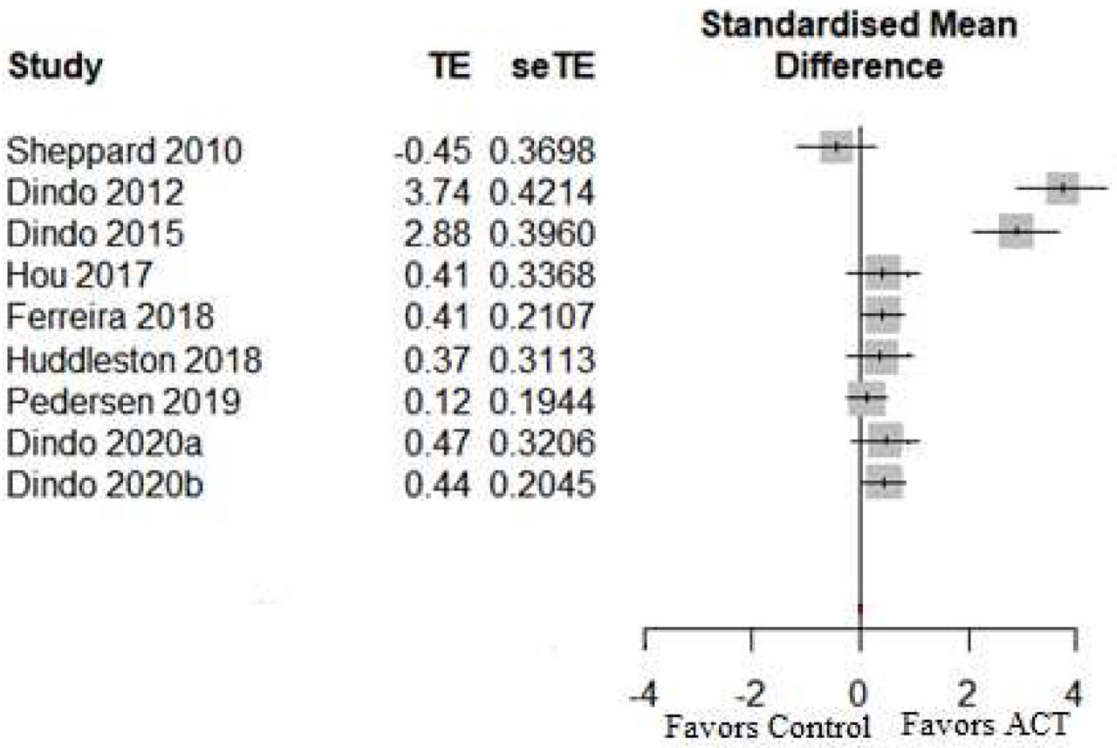


**Figure 3.**

Within-group effect sizes and forest plot for **functioning-related outcomes** at **follow-up**. Functioning-related outcomes included measures of disability, symptom interference, well-being, and quality of life. Where studies reported more than one relevant outcome, the general (rather than condition-specific) measure was selected to reduce heterogeneity. Error bars are 95% confidence intervals. TE = Hedges' *g*; seTE = standard error of Hedges' *g*.

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**Table 1.**

Study quality assessment using the Psychotherapy Outcome Study Methodology Rating Scale (POMRF)

| Study | Gregg (2007) | Sheppard (2010) | Dindo (2012) | Dindo (2014)† | Welch (2014) | Dindo (2015) | Hou (2017) | Dindo (2018) | Ferreira (2018) | Huddleston (2018) | Hadland smyth (2019) | Pedersen (2019) | Dindo (2020a) | Dindo (2020b) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Pilot/Feasibility** | N | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | N | Y | N |
| **Controlled** | Y | N | Y | Y | N | Y | N | Y | N | N | Y | Y | Y | Y |
| **1: Clarity of sample description** | 2 | 1 | 1 | 1 | 2 | 1 | 1 | 1 | 1 | 1 | 1 | 2 | 2 | 2 |
| **2: Severity/ chronicity of disorder** | n/a | n/a | n/a | n/a | n/a | n/a | n/a | n/a | n/a | n/a | n/a | n/a | n/a | n/a |
| **3: Representative-ness of sample** | 2 | 1 | 2 | 2 | 1 | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 |
| **4: Reliability of diagnosis** | n/a | n/a | n/a | n/a | n/a | n/a | n/a | n/a | n/a | n/a | n/a | n/a | n/a | n/a |
| **5: Specificity of outcome measures** | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 |
| **6: Reliability and validity of outcome measures** | 2 | 2 | 2 | 0 | 2 | 2 | 2 | 1 | 2 | 2 | 2 | 2 | 2 | 2 |
| **7: Use of blind evaluators** | 1 | n/a | 0 | 0 | n/a | 0 | n/a | 1 | n/a | n/a | n/a | 0 | 0 | 2 |
| **8: Assessor training** | 1 | 0 | 2 | 2 | 0 | 0 | 0 | 0 | 0 | 0 | n/a | 0 | 0 | 0 |
| **9: Assignment to treatment** | 1 | 0 | 0 | 0 | 0 | 1 | 0 | 1 | 0 | 0 | 1 | 1 | 1 | 2 |
| **10: Design** | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 1 | 1 | 2 |
| **11: Power analysis** | 2 | 0 | n/a | n/a | n/a | n/a | 0 | n/a | n/a | n/a | n/a | 2 | n/a | 0 |
| **12: Assessment points** | 0 | 0 | 1 | 1 | 0 | 1 | 0 | 1 | 1 | 1 | 0 | 2 | 0 | 1 |
| **13: Manualized, replicable, specific treatment programs** | 2 | 0 | 0 | 0 | 2 | 0 | 0 | 0 | 2 | 0 | 1 | 0 | 2 | 2 |
| **14: Number of therapists** | 1 | 1 | 1 | 1 | 0 | 0 | 0 | 1 | 1 | 0 | 1 | 1 | 1 | 1 |
| **15: Therapist training/experience** | 1 | 2 | 2 | 2 | 0 | 0 | 0 | 2 | 2 | 0 | 2 | 2 | 2 | 2 |

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| Study | Gregg (2007) | Sheppard (2010) | Dindo (2012) | Dindo (2014)† | Welch (2014) | Dindo (2015) | Hou (2017) | Dindo (2018) | Ferreira (2018) | Huddleston (2018) | Hadland smyth (2019) | Pedersen (2019) | Dindo (2020a) | Dindo (2020b) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **16: Checks for treatment adherence** | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 1 | 0 |
| **17: Checks for therapist competence** | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 2 | 0 |
| **18: Control of concomitant treatments** | 0 | 0 | 1 | 1 | 0 | 1 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 |
| **19: Handling of attrition** | 2 | 1 | 1 | 1 | 0 | 1 | 0 | 2 | 1 | 0 | 0 | 2 | 1 | 1 |
| **20: Statistical analyses and presentation of results** | 2 | 2 | 2 | 2 | 2 | 1 | 2 | 1 | 2 | 2 | 2 | 1 | 2 | 2 |
| **21: Clinical significance** | n/a | n/a | n/a | n/a | n/a | n/a | n/a | n/a | n/a | n/a | n/a | n/a | n/a | n/a |
| **22: Equality of therapy hours** | 2 | n/a | n/a | n/a | n/a | n/a | n/a | n/a | n/a | n/a | n/a | 0 | n/a | 2 |

*Note*. Pilot/feasibility determination made based on source author's use of the terms to describe study.

†Same parent study as Dindo (2012)

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**Table 2.**

Manuscripts reporting studies of single-session ACT interventions for medical populations ($K$ =14)

| | First author (year) | Population/ Condition | Participant characteristics | Study design, setting, intervention duration, assessment schedule | Intervention and comparator conditions | Outcomes and measures | Key Findings |
|---|---|---|---|---|---|---|---|
| **1** | Gregg (2007) | Adults with diabetes mellitus type 2 | $N$ = 81; $M_{age}$ = 50.9 years; female = 46.9%; white = 23.5%; employment = 10% working full-time, 9% working part-time, 25% unemployed looking for work, 6% unemployed not looking for work, 8% retired, 28% disabled/ unable to work; education = 57% with some education greater than High School; income not reported; $M_{BMI}$ = 32.6 | Randomized controlled trial; 7-hour workshop; Setting: community health center; Baseline, 3-month FU | (1) ACT + Diabetes management education ($n$ = 43)<br>(2) Diabetes management education ($n$ = 38) | (a) HbA1c<br>(b) Diabetic Control (HbA1c < 7.0%)<br>(c) Diabetes self-management (3 self-report items based on exercise, diet, glucose monitoring)<br>(d) Diabetes acceptance (Acceptance and Action Diabetes Questionnaire/ AADQ) | (a) Nonsignificant trend for greater improvement in HbA1c in Group (1) > Group (2) at FU; Cohen's $d$ = 0.35<br>(b) Significant increase in rate of diabetic control in Group (1) at FU, $p$ < .01, but not Group (2); Cohen's $d$ = 0.61<br>(c) Group (1) increase > Group (2) increase at FU, both $p$ < .05; Cohen's $d$ = 0.68<br>(d) Group (1) increase significant ($p$ < .01) and > Group (2) increase at FU; Cohen's $d$ = 0.78 |
| **2** | Sheppard (2010) | Adults with multiple sclerosis (MS) | $N$ = 15; $M_{age}$ ($SD$) = 53.13 years (7.68); female = 80%; white = 66.7 %; employment = 46% unemployed or receiving disability payments; education = 60% with Bachelors degree or higher; income not reported | Uncontrolled feasibility trial; 5-hour workshop; Setting: local hotel conference venue; Baseline, 3-month FU | ACT + MS education | (a) Depression (Beck Depression Inventory-II/ BDI-II)<br>(b) Impact of Fatigue (Modified Fatigue Impact Scale/MFIS)<br>(c) Pain Effects on mood/ behavior (Pain Effects Scale/PES)<br>(d) Physical Health (SF-36)<br>(e) Mental Health (SF-36)<br>(f) Quality of Life (Quality of Life Inventory/QOLI)<br>(g) Thought Suppression (White Bear Suppression Inventory/WBSI)<br>(h) Mindful Attention Awareness Scale (MAAS) | (a) Baseline to FU mean difference = −8.31; Hedge's $g$ = −0.73<br>(b) Baseline to FU mean difference = −11.21; Hedge's $g$ = −0.50<br>(c) Baseline to FU mean difference = 4.98; Hedge's $g$ = −0.98<br>(d) Baseline to FU mean difference for physical health = −0.86; Hedge's $g$ = −0.10<br>(e) Baseline to FU mean difference for mental health = 4.23; Hedge's $g$ = 0.36<br>(f) Baseline to FU mean difference = 1.11; Hedge's $g$ = 0.45<br>(g) Baseline to FU mean difference = 5.35; Hedge's $g$ = −0.33<br>(h) Baseline to FU mean difference = 0.22; Hedge's $g$ = −0.22 |
| **3** | Dindo (2012) | Adults with migraine and depression (current depressive episode) | $N$ = 45; $M_{age}$ ($SD$) = 32.8 years (13.2); female = 94%; white = 92%; employment = 89% working or in school; education = 89% > 12 years; income not reported | Non-randomized controlled pilot trial; 5-hour workshop; Setting: university hospital and clinics; Baseline, 2-, 6-, 12-week FU | (1) ACT + migraine psychoeducation ($n$ = 31)<br>(2) Waitlist/TAU ($n$ = 14) | (a) Depression Sx (Hamilton Rating Scale for Depression/HRSD)<br>(b) General functioning (World Health Organization Disability Assessment Schedule II/WHODAS) $^{M}$<br>(c) Migraine-related disability (Headache Disability Inventory/HDI) | (a) Group (1) decrease > Group (2) decrease at 12-week FU; Cohen's $d$ = 1.18<br>(b) Group (1) increase > Group (2) increase at 12-week FU; Cohen's $d$ = 0.98<br>(c) Group (1) decrease > Group (2) decrease at 12-week FU; Cohen's $d$ = 1.03 |
| **4** | Dindo (2014) $^{\dagger}$ | Adults with migraine and | $N$ = 45; $M_{age}$ ($SD$) = 31.4 years (12.3); female = 94%; | Non-randomized controlled pilot trial; | (1) ACT + migraine psychoeducation ($n$ | (a) Headache frequency/ severity | (a) Group (1) decreased in frequency (OR=0.57) and severity (OR=0.41) at 12- |

| | First author (year) | Population/ Condition | Participant characteristics | Study design, setting, intervention duration, assessment schedule | Intervention and comparator conditions | Outcomes and measures | Key Findings |
|---|---|---|---|---|---|---|---|
| | | depression (current depressive episode) | white = 87%; employment = 90% working or in school; education = 92% > 12 years; income not reported | 5-hour workshop; Setting: university hospital and clinics; Baseline, 2-, 6-, 12-week FU | = 38) (2) Waitlist/TAU ($n$ = 22) | (b) Acute headache medication use<br>(c) Leisure and work disability<br>(d) Visit to healthcare professional<br>Note: all outcomes measured using daily headache diary | week FU, no change for Group (2) (OR=0.84, 1.0)<br>(b) Group (1) decreased in acute medication use (OR=0.64) at 12-week FU, no change for Group (2) (OR=0.97)<br>(c) Group (1) decreased in leisure disability (OR=0.56) and work disability (OR=1.0) at 12-week FU, no change for Group (2) (OR=0.78, 1.8)<br>(d) At baseline, 20% of Group (1) and 22% of Group (2) attended medical visit in last month; at 12-week FU, 3% of Group (1) and 33% of Group (2) |
| **5** | Welch (2014) | Adults with diabetes mellitus type 2 with significant distress (either diabetes management regimen distress or emotional burden) | $N$ = 31; $M_{age}$ ($SD$)= 43 years (9.1); female = 70%; white = 45%; employment status not reported; $M_{years}$ education ($SD$) = 13.9 (1.2); income not reported | Uncontrolled pilot trial; 8-hour workshop; Setting: psychology graduate school; Baseline, post-treatment, 2-week FU | ACT | (a) Self-care behavior (Summary of Diabetes Self-Care Activities scale revised version/SDSCA)<br>(b) Diabetes-related distress (Diabetes Distress Scale 17/ DDS17) total<br>(c) Diabetes acceptance (AADQ)<br>(d) Thought suppression (WBSI)<br>(e) Depression (DASS-21)<br>(f) Anxiety (DASS-21)<br>(g) Stress (DASS-21) | (a) Baseline to FU mean difference on subscales:<br>(1) General diet = 0.82; Cohen's $d$ = 0.52<br>(2) Diabetes-specific diet = 0.62; Cohen's $d$ = 0.61<br>(3) Exercise = −2.12; Cohen's $d$ = 1.62<br>(4) Blood-glucose testing = 0.58; Cohen's $d$ = 0.29<br>(5) Foot care = −1.29; Cohen's $d$ = 0.76<br>(6) Smoking status = −0.9; Cohen's $d$ = 0.22<br>(b) Baseline to FU mean difference on total score = −1.51; Cohen's $d$ = 1.74<br>(c) Baseline to FU mean difference = 18.85; Cohen's $d$ = 1.95<br>(d) Baseline to FU mean difference = 19.15; Cohen's $d$ = 1.58<br>(e) Baseline to FU mean difference = −3.1; Cohen's $d$ = 0.57<br>(f) Baseline to FU mean difference = −4.4; Cohen's $d$ = 0.42<br>(g) Baseline to FU mean difference = −5.7; Cohen's $d$ = 0.72 |
| **6** | Dindo (2015) | Adults at risk for vascular disease * with clinically significant anxiety or depression Sx | $N$ = 44; $M_{age}$ = 45 years; female = 67%; white = 74%; employment not reported; education = 70% completed college; income not reported | Randomized controlled pilot trial; 6-hour workshop; Setting: university hospital and clinics; Baseline, 12- and 24-week FU | (1) ACT + psychoeducation ($n$ = 30)<br>(2) TAU ($n$ = 14) | (a) General wellbeing (World Health Organization Quality of Life-BREF/ WHOQOL-BREF) $M$<br>(b) Depression (HRSD)<br>(c) Anxiety (Hamilton Rating Scale for Anxiety/ HSRA)<br>(d) Psychological Flexibility/decentering (Experiencing Questionnaire/EQ) | (a) In Group (1), all four domains (physical, social, psychological, environment) improved at 24-week FU; in Group (2), only psychological domain was improved<br>(b) Group (1) decrease > Group (2) decrease at 12- and 24-week FU; unspecified effect size = 1.4 at 24-week FU<br>(c) Group (1) decrease > Group (2) decrease at 12- and 24-week FU; unspecified effect size = 1.5 at 24-week FU<br>(d) Increase in Group (1) at unspecified FU; Group (2) results not reported |

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| | First author (year) | Population/ Condition | Participant characteristics | Study design, setting, intervention duration, assessment schedule | Intervention and comparator conditions | Outcomes and measures | Key Findings |
|---|---|---|---|---|---|---|---|
| **7** | Hou (2017) | Adults with inflammatory bowel disease (IBD) and clinically significant anxiety or depression symptoms | $N$ = 20; $M_{age}$ = 51 years; female = 30%; % white not reported; employment not reported; education not reported; income not reported; $n$=9 with Crohn's disease, $n$=10 with ulcerative colitis, $n$=1 IBD unclassified | Uncontrolled feasibility study; 5-hour workshop; Setting: Veterans Affairs medical center and university medical school; Baseline, 3-month FU | ACT + IBD education | (a) Health-related Quality of Life (the Short IBD Questionnaire/SIBDQ)<br>(b) Depression (DASS-21)<br>(c) Anxiety (DASS-21)<br>(d) Stress (DASS-21)<br>(e) IBD activity (Harvey Bradshaw Index for pts. with Crohn's/HBI)<br>(f) IBD activity (partial Mayo score for pts with Ulcerative Colitis/pMayo) | (a) Baseline to FU mean difference = 0.5; Hedge's $g$ = 0.41<br>(b) Baseline to FU mean difference = −2.1; Hedge's $g$ = −0.39<br>(c) Baseline to FU mean difference = −2.6; Hedge's $g$ = −0.65<br>(d) Baseline to FU mean difference = −1.9; Hedge's $g$ = −0.39<br>(e) Baseline to FU mean difference = −0.5; Hedge's $g$ = −0.15<br>(f) Baseline to FU mean difference = −1.2; Hedge's $g$ = −0.47 |
| **8** | Dindo (2018) | Veterans receiving orthopedic surgery at risk for chronic pain or prolonged opioid use ** | $N$ = 88; $M_{age}$ ($SD$) = 63 years (10); female = 7%; white = 82.5%; employment not reported; education = 68% with some education greater than High School; income not reported | Randomized controlled pilot trial; 5-hour workshop; Setting: Veterans Affairs medical center; Baseline and 3-month FU for CPAQ and CPVI; DLPM submitted weekly for 14 weeks post-treatment | (1) ACT workshop + 1 individualized "booster" session by phone 2–4 wks following workshop ($n$ = 44)<br>(2) TAU ($n$ = 44) | (a) Pain cessation (Daily Log of Pain and Pain Medication/ DLPM)<br>(b) Opioid use/ cessation (DLPM)<br>(c) Pain acceptance (Chronic Pain Acceptance Questionnaire total/CPAQ)<br>(d) Values-based behavior (Chronic Pain Values Inventory/ CPVI) | (a) Median days to pain cessation was 66 for Group (1) and 74 for Group (2); HR = 1.42 [95% CI: 0.68, 2.95]<br>(b) 29% of Group (1) taking opioids at 7 weeks v. 52% of Group (2); HR = 1.44 [95% CI: 0.74, 2.78]<br>(c) Mean difference between Group (1) and (2) at FU = 2.07 [95% CI: −7.46, 11.61]; HR = 1.42 [95% CI: −7.11, 9.57]<br>(d) Mean difference between Group (1) and (2) on 'mean success' at FU = −0.50 [95% CI: −1.01, 0.01]; HR = 0.13 [95% CI: −0.33, 0.59]; mean difference between Group (1) and (2) on 'discrepancy score' at FU = 0.21 [95% CI: −0.23, 0.65]; HR = −0.42 [95% CI = −0.85, 0.01] |
| **9** | Ferreira (2018) | Adults with refractory Irritable Bowel Syndrome (IBS) | $N$ = 79; $M_{age}$ ($SD$) = 48 years (13); female = 93%; race/ethnicity not reported; employment not reported; education = 68% with post-secondary education; income not reported. | Uncontrolled pilot trial; 6-hour workshop followed by 2 months of bibliotherapy and 2 FU support calls; Setting: gastroenterology outpatient clinic; Enrollment, pre-treatment, post-treatment (2 months following pre-treatment), 6-month FU | ACT + IBS education workshop, bibliotherapy, 2 individualized support calls | (a) IBS Acceptance (IBS Acceptance and Action Questionnaire; IBSAAQ)<br>(b) Symptom severity (IBS Symptom Severity Scale; IBSSSS)<br>(c) Quality of life (IBS Impact on Quality of Life Scale; IBS36)<br>(d) IBS avoidant behaviors (Behavioural Responses Questionnaire; IBS-BRQ)<br>(e) Gastrointestinal specific anxiety (Visceral Sensitivity Index; VSI) | (a) Pre- to post-treatment mean difference = 7.24; Cohen's $d$ = 0.32; Pre-treatment to FU mean difference = 9.82; Cohen's $d$ = 0.50<br>(b) Pre- to post-treatment mean difference = −41.48; Cohen's $d$ = 0.41; Pre-treatment to FU mean difference = −49.78; Cohen's $d$ = 0.47<br>(c) Pre- to post-treatment mean difference = −17.41; Cohen's $d$ = 0.41; Pre-treatment to FU mean difference = −23; Cohen's $d$ = 0.55<br>(d) Pre- to post-treatment mean difference = −8.57; Cohen's $d$ = 0.32; Pre-treatment to FU mean difference = −10.18; Cohen's $d$ = 0.39<br>(e) Pre- to post-treatment mean difference = −6.3; Cohen's $d$ = 0.76; Pre-treatment to FU mean difference = −8.73; Cohen's $d$ = 1.10 |
| **10** | Huddleston (2018) | Veterans with migraines and co-occurring | $N$ = 32; Age = 36% under 45, 36% 45–55 years, 24% 56–65 years, 1% over 65 | Uncontrolled pilot trial; 5-hour workshop; Setting: | ACT + migraine education | (a) Depression Sx (HRSD)<br>(b) Anxiety Sx (HRSA)<br>(c) General functioning | (a) Baseline to FU mean difference = −7.95; Cohen's $d$ = −1.93<br>(b) Baseline to FU mean difference = |

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| | First author (year) | Population/ Condition | Participant characteristics | Study design, setting, intervention duration, assessment schedule | Intervention and comparator conditions | Outcomes and measures | Key Findings |
|---|---|---|---|---|---|---|---|
| | | depression (current depressive episode) | years; female = 36%; white = 24%; employment = 25% employed full- or part-time, 6% retired, 34% unemployed, 22% disabled, 13% student; education = 56% with some education greater than High School; income not reported | Veterans Affairs medical center; Baseline, 3-month FU | | (WHO Disability Assessment Schedule II/ WHODAS)<br>(d) Headache-related disability (HDI)<br>(e) Pain acceptance (CPAQ)<br>(f) Values-based behavior (CPVI)<br>(g) Psychological flexibility (Acceptance and Action Questionnaire/AAQ-II) | −6.57; Cohen's $d$ = −1.84<br>(c) Baseline to FU mean difference = −5.38; Cohen's $d$ = −0.38<br>(d) Baseline to FU mean difference = −7.44; Cohen's $d$ = −0.39<br>(e) Baseline to FU mean difference = 8.48; Cohen's $d$ = 0.48<br>(f) Baseline to FU mean difference = 0.5; Cohen's $d$ = 0.48<br>(g) Baseline to FU mean difference = −5.53; Cohen's $d$ = −0.71 |
| **11** | Hadlandsmyth (2019) | Adult women undergoing surgery for breast cancer or ductal carcinoma in situ at risk for persistent postsurgical pain *** | $N$ = 62; $M_{age}$ ($SD$) = 53 years (12); female = 100%; white = 87%; employment not reported; education not reported; income = 17% < $40,000, 24% $40,000–79,999, 59% $80,000+ | Randomized controlled pilot trial; 2-hour individual session 2 weeks post-surgery; Setting: comprehensive cancer center; Baseline, 3-month FU | (1) ACT therapy + TAU ($n$ = 24)<br>(2) TAU (medical care) ($n$ = 30) | (a) Pain intensity (0–10 scale)<br>(b) Pain catastrophizing (Pain Catastrophizing Scale/ PCS)<br>(c) Depression (Patient Health Questionnaire-8)<br>(d) Anxiety (Generalized Anxiety Disorder-7)<br>(e) Pain acceptance (CPAQ) | (a) At FU, 8.3% in Group (1) reported moderate-to-severe pain v. 13.3% in Group (2); phi = 0.08<br>(b) At FU, 4.2% in Group (1) reported elevated pain catastrophizing v. 3.3% in Group (2); phi = 0.02<br>(c) At FU, 12.5% in Group (1) reported elevated depression v. 13.3% in Group (2); phi = 0.01<br>(d) At FU, 4.2% in Group (1) reported elevated anxiety v. 13.3% in Group (2); phi = 0.16<br>(e) Mean difference at FU = 1.66, favoring Group (1); Cohen's $d$ = 0.10 |
| **12** | Pedersen (2019) | Adults with multiple functional somatic syndromes (FSS) | $N$ = 121 (for conditions of interest); $M_{age}$ ($SD$) = 39 years (9); female = 83%; race/ ethnicity not reported; employment = 26% employed or student, 31% unemployed, 31% disability pension or flexible work; education = 31% greater than basic school (Denmark); income not reported; Functional Somatic Syndromes: 29% Irritable Bowel Syndrome, 79% Chronic Fatigue Syndrome, 74% Fibromyalgia, 74% tension headaches, 55% non-cardiac chest pain; average number of FSS = 3.9 | Randomized, controlled, 3-arm trial; Setting: university general hospital; Baseline, 6-, 14, and 20-month FU | (1) ACT (6-hour workshop) + Enhanced Care (1–1.5 hour psychoeducation consultation with a physician 1–2 weeks after randomization) ($n$ = 61)<br>(2) Enhanced Care only ($n$ = 60)<br>(3) Extended ACT (nine 3-hr group sessions) + Enhanced Care ($n$ = 59)<br>Note: this review reports results comparing Groups (1) and (2) only | (a) Patient-rated overall health (5-pt clinical global improvement scale/CGI)<br>(b) Physical Health (SF-36)<br>(c) Mental Health (SF-36)<br>Note: total of 18 secondary outcomes assessed, including: depression Sx, anxiety and somatic Sx (Hopkins Symptom Checklist/SCL-92; BDS checklist); illness worry (Whiteley-7); disability (WHODAS 2.0) $^{M}$ | (a) No difference between Groups (1) and (2) at 14-month FU<br>(b) No difference between Groups (1) and (2), $p$ = .98 at 14-month FU<br>(c) No difference between Groups (1) and (2), $p$ = .59 at 14-month FU<br>Note: no significant differences between Groups (1) and (2) in change over time on the 18 secondary outcomes |
| **13** | Dindo (2020a) | Veterans with chronic pain, mild Traumatic Brain Injury (mTBI), and | $N$ = 39; $M_{age}$ ($SD$) = 36.6 years (6.2); female = 0%; white = 42%; employment = 51% employed full- or part-time; $M_{years}$ education | Randomized controlled pilot trial; 5-hour workshop; Setting: Veteran Affairs medical | (1) ACT + psychoeducation ($n$ = 20)<br>(2) TAU ($n$ = 12) | (a) PTSD (Posttraumatic Stress Disorder Checklist; PCL-C)<br>(b) Depression, Anxiety, and Stress (DASS-21 total) | (a) Group (1) decrease > Group (2) decrease at FU; Cohen's $d$ = 0.33<br>(b) Group (1) decrease > Group (2) increase at FU; Cohen's $d$ = 0.68<br>(c) Group (1) improved, Group (2) |

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| | First author (year) | Population/ Condition | Participant characteristics | Study design, setting, intervention duration, assessment schedule | Intervention and comparator conditions | Outcomes and measures | Key Findings |
|---|---|---|---|---|---|---|---|
| | | current diagnosis of MDD, GAD, or PTSD | (*SD*) = 14.2 (1.7); Past month diagnoses: 68% PTSD, 54% MDD, 16% GAD; most severe TBI = 26% Stage 1 mTBI, 55% Stage 2 mTBI, 19% Stage 3 mTBI | center; Baseline, 3-month FU | | (c) Reintegration (Military to Civilian Questionnaire/ M2C-Q)<br>(d) Disability (WHODAS 2.0) *M*<br>(e) Pain Severity (Brief Pain Inventory/BPI)<br>(f) Pain Interference (BPI)<br>(g) Psychological flexibility (AAQ-II) | worsened at FU; Cohen's *d* = 0.47<br>(d) Group (1) decrease > Group (2) decrease at FU; Cohen's *d* = 0.44<br>(e) No difference in Group (1) and Group (2) decrease at FU; Cohen's *d* = 0.10<br>(f) Group (2) decrease > Group (1) decrease at FU; Cohen's *d* = 0.78<br>(g) Group (1) increase > Group (2) increase at FU; Cohen's *d* = 0.56 |
| **14** | Dindo (2020b) | Adults with migraines and co-occurring depression (current depressive episode) | *N* = 136; $M_{age}$ (*SD*) = 35.8 years (13.9); female = 83%; white = 76%; employment = 81% employed or in school; education = 57% with more than 12 years of education; age of onset of migraines *M* (*SD*) = 19 (10.6); 31% taking antidepressants; number of migraine/headache days during month prior to baseline *M* (SD) = 7.4 (3.4); 87% taking abortive anti-migraine medication; 35% taking preventative anti-migraine medication | Randomized controlled trial; 5 to 6-hour workshop; Setting: hospital; Baseline, 3-, and 6-month FU | (1) ACT + migraine education (*n* = 56)<br>(2) Support + migraine education (*n* = 47) | (a) Depression Sx (Hamilton Rating Scale of Depression/HRSD)<br>(b) Current depressive episode (depression module of SCID-IV)<br>(c) Anxiety (Structured Interview Guide for the Hamilton Anxiety Rating Scale/SIGH-A)<br>(d) Headache-related disability (HDI)<br>(e) General Functioning (WHODAS 2.0) *M*<br>(f) Social relationship functioning (World Health Organization Quality of Life/WHO-QOL)<br>(g) Environment (WHO-QOL)<br>(h) Psychological well-being (WHO-QOL)<br>(i) Physical health (WHO-QOL) | (a) Group (1) proportion of treatment responders > Group (2) at 3-month FU, *p* < .05, OR = 3.10<br>(b) Nonsignificant result for group (1) proportion of participants meeting criteria < Group (2) at 3-month FU, *p* = .33, OR = 0.54<br>(c) Nonsignificant result for group (1) proportion of treatment responders > Group (2) at 3-month FU, *p* = .11, OR = 2.45<br>(d) Group (1) proportion of treatment responders (i.e., ≥ 29 decline in total score) mean decrease > Group (2) at 3-month FU, *p* < .05, OR = 4.47<br>(e) Nonsignificant difference in mean improvement at 3-month FU, *p* = .23, Cohen's *d* = 0.33<br>(f) Group (1) increase > Group (2) increase at 3-month FU, *p* = .01, Cohen's *d* = 0.62<br>(g) Group (1) increase > Group (2) increase at 3-month FU, *p* = .05, Cohen's *d* = 0.47<br>(h) Nonsignificant trend for Group (1) increase > Group (2) increase at 3-month FU, *p* = .06, Cohen's *d* = 0.46<br>(i) Nonsignificant difference in mean increase at 3-month FU, *p* = .40, Cohen's *d* = 0.27 |

*Note*. *N* reflects the number of participants randomized (for studies with > 1 condition) or assigned to treatment (for single-arm studies); FU = follow-up; Sx = symptoms; *M* = mean; *SD* = standard deviation; TAU = treatment as usual; OR = odds ratio; HR = hazard ratio; SCID-IV = Structured Clinical Interview for DSM-IV; PTSD = posttraumatic stress disorder; MDD = major depressive disorder; GAD = generalized anxiety disorder. Unable to calculate effect sizes for Pedersen (2019). Effect sizes reported for studies with comparison conditions are between-group effects; effect sizes reported those studies without comparison conditions are within-group effects.

† Subsample from Dindo (2012) study, focused on headache outcomes

* At-risk defined as having hypertension, diabetes mellitus or impaired fasting glucose, dyslipidemia, or obesity

** At-risk defined as having high levels of preoperative pain and clinically significant anxiety or depression

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*** At-risk defined as under the age of 50, having a preexisting chronic pain condition, elevated anxiety, elevated depression, or elevated pain catastrophizing, assessed pre-surgery

*M* Included in meta-analysis