



# Acceptance and commitment therapy (ACT) for caregivers of children with chronic conditions: A mixed methods systematic review (MMSR) of efficacy, process, and acceptance.

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## ABSTRACT

Caregivers of children with chronic conditions face enormous challenges and often poor mental health. ACT may facilitate psychological adjustment for this population. This MMSR therefore aimed to examine the efficacy and acceptability of ACT for caregivers of children (diagnosed aged <18) with long-term conditions. PsychInfo, Ovid MEDLINE, and EMBASE were searched. Studies were included that mentioned ACT as the intervention under study in the manuscript and referenced an existing ACT protocol or stated that the applied intervention was guided by core processes of ACT. Both individual and group ACT interventions were eligible for inclusion. Studies were excluded if they applied ACT to other populations, adopted an inappropriate research methodology (e.g., case study), or didn't publish in English in a peer reviewed journal. A total of 19 eligible studies were returned from searches based on these criteria. However, despite both individual and group ACT interventions being eligible for inclusion, none of the 19 studies delivered ACT individually. Meta-analysis revealed significant effects of group ACT interventions on parental mood at post-intervention (SMD = -0.43, P = 0.001), follow-up (SMD = -0.65, P = 0), and both time points combined (SMD = -0.52, P = 0). Group ACT interventions also had significant effects on parenting confidence at both time-points combined (SMD = 0.34, P = 0.018), and on cognitive fusion at follow-up (SMD = -6.12, P = 0.016). Further, significant effects of the intervention on psychological flexibility were revealed at post-intervention (SMD = -2.92, P = 0.007), follow-up (SMD = 5.19, P = 0), and both time points combined (SMD = -3.89, P = 0). Narrative synthesis then suggested positive impacts of group ACT interventions on mood, general wellbeing, and all ACT processes. Finally, qualitative findings indicated that group ACT interventions facilitated a sense of all being in the same boat which allowed parents to open-up. Mindfulness exercises and peer interaction were identified as particularly helpful aspects of the intervention. ACT was therefore shown to be effective and acceptable in improving the health of caregivers. Future research evaluating ACT interventions delivered in non-group-based is now required.

## 1. Introduction

Chronic conditions such as diabetes, cancer, and brain injury are defined as health issues of long duration and generally slow progression (WHO. *Noncommunicable Diseases*, 2016). A child's diagnosis of a chronic condition has been shown to increase emotional and behavioural problems (Bennett et al., 2021). Caregivers of children with chronic conditions are also impacted by their child's diagnosis. For

example, Vasilopoulou and Nisbet (2016) revealed that caring for a child with autism reduces Quality of life (QoL), which they define as a complex and multidimensional concept that allows for a detailed evaluation of adaptation, both positive and negative, across several domains including physical health, mental health, and social functioning. Further, caring for a child with a chronic condition increases psychological distress as caregivers realise the difficulty and longevity of managing their child's condition (Estes et al., 2013; Mcstay et al., 2013).

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This distress often leads to psychological health issues, with 61% of caregivers of children with diabetes meeting criteria for depression, and 59% meeting criteria for anxiety (Streisand et al., 2008). These psychological health issues have then been shown to affect attachment formation and the cognitive, emotional, social, and behavioural development of children (Manning & Gregoire., 2009). Further, children exposed to elevated maternal depressive symptoms and anger display more behavioural problems and worse prosocial functioning than children in high quality childcare (Goelman et al., 2014). Supporting caregivers of children diagnosed with chronic conditions is therefore gaining recognition as an important aspect of healthcare (Guite et al., 2018), particularly for parents of children with conditions which require intensive management. Psychological intervention represents one way to support wellbeing in this group, and one novel psychological approach which has a growing presence in clinical health settings is Acceptance and Commitment Therapy (ACT) (Graham et al., 2016; Thewes et al., 2014).

### 1.1. Acceptance and commitment therapy (ACT)

ACT is a psychological intervention tracing its roots to functional contextualism (Hayes, 2004), relational frame theory (Hayes et al., 2001), and radical behaviourism. Informed by these theories and pragmatic philosophy, ACT aims to engender a quality within behaviour termed psychological flexibility (PF): “the capacity to persist or to change behaviour in a way that 1.) includes conscious and open contact with thoughts and feelings (openness), 2.) appreciates what the situation affords (awareness), and 3.) serves one’s goals and values (engagement)” (McCracken & Morley, 2014, p. 225). As is apparent in this definition, PF is a compound process that can be considered to comprise several sub-processes – often conceptualised as a tri-flex of three sub processes (openness, awareness, engagement) or as a hex-a-flex of six (experiential acceptance, defusion; present-moment-focus, self-as-context; contact with values, committed action). A range of therapy techniques are therefore used within ACT to enhance PF. Commonly used techniques include reflection on values and goal setting, metaphor, perspective-taking, and mindfulness exercises. Techniques may be selected to target PF sub-process. For example, verbal distancing might be used to increase openness/defusion, and centring exercises could be used to develop the capacity of greater awareness/present-moment-focus. Given ACT’s pragmatic underpinnings, “workability” is also encouraged by directing participants to consider the effectiveness of their actions, given their own overarching goals and values, and encouraging them to select behaviours that are life-enhancing. Overlapping with this concept is a key skill in noticing, sometimes termed context sensitivity (Graham et al., 2021). This involves increased awareness of thoughts and feelings and the contexts that influence them, alongside greater awareness of the range of options available in each situation and the effectiveness of choices. Thus, ACT takes an experiential approach, where participants are invited to try exercises or shift perspectives and notice changes in experience (Villatte et al., 2015). To date, systematic reviews (SR’s) suggest that ACT is effective for reducing distress and improving well-being across mental and physical health conditions (Graham et al., 2016; Veehof et al., 2011; A-Tjak et al., 2015; Öst, 2008; Öst, 2014; Swain et al., 2013). It has therefore been argued that ACT is particularly suitable in the context of chronic health conditions (Graham et al., 2016).

### 1.2. Why ACT is useful for caregivers of children with chronic conditions?

The approach here is to target PF to enhance functioning of both the caregiver and child, through making caregivers more aware of their interactions with distressing thoughts and feelings so that their behaviours align more closely with their core values. When caring for a child with a chronic condition, negative beliefs about the illness or the future

may simply reflect the challenging context in which the caregiver and child find themselves. Teaching caregivers to become aware of their interactions with these negative beliefs therefore allows them to make more effective choices (Coyne et al., 2011). More specifically, ACT encourages parents to notice where challenging thoughts and feelings occur as a reflection of values – and show willingness to have such experiences as a part of activity that is valued or important (Graham et al., 2015). For example, caregivers may have thoughts about ‘being a failure’ or ‘being a bad parent’ when talking with their child about ineffective use of their medication. When these thoughts are present, caregivers might approach them in ways that lead to entanglement, such as worry or rumination, that have a negative impact on the interaction they have with their child. ACT offers skills for reducing the competing influence of these thoughts (e.g., verbal distancing, perspective-taking) and feelings (e.g., the addition of appetitive functions via metaphor) over engagement in life-enhancing activities. Thus, in this example parents should apply these skills as they have challenging conversations with their child regarding their use of medication, and notice if this enhances their experience of, or effectiveness within, this interaction. In support of the effectiveness of such an approach in improving PF and various other health outcomes, we are aware of several trials demonstrating positive impacts on caregivers of children with a wide range of chronic conditions (Burke et al., 2014; Rayner et al., 2016; Sairanen et al., 2019).

### 1.3. Systematic reviews

Existing SR’s suggest that ACT may be a useful intervention for caregivers in paediatric settings. For example, Jin et al. (2021) found significant effects of ACT for psychological and behavioural changes among parents of children with chronic health conditions. More specifically, results indicated that ACT significantly improved parental PF and reduced psychological distress compared with usual care and waitlist control groups. However, Jin et al.’s (2021) review did not come without limitations, as only eight studies were included. Further, high clinical and statistical heterogeneity across the included studies meant narrative synthesis was performed instead of a meta-analysis. Byrne et al.’s (2020) review also found significant effects of ACT on stress, depression, and anxiety among parents of children who were presenting with physical and emotional difficulties whilst living with a chronic condition. Similar improvements were noted on several ACT mechanisms of change outcomes for parents, including mindfulness, acceptance, and cognitive fusing. However, broad study eligibility criteria led to a high level of heterogeneity among studies which again prevented the use of meta-analysis. Byrne et al.’s (2020) review included studies that applied ACT to both caregivers and children. This makes it difficult to identify whose treatment is impacting on caregiver health, as Murphy et al. (2018) found that children’s health is strongly associated with the health of their parents. It is therefore difficult to identify whether changes to caregiver health occurred as a direct result of their own treatment, or because of changes in the child’s health after receiving ACT. A more recent review examined the effects of ACT on family caregivers of adults and young people with chronic conditions through a meta-analysis with a random effects model (Han et al., 2021). 24 articles were included in the review. The meta-analysis found moderate effects of ACT on depressive symptoms and QoL, small effects on anxiety, and small to moderate effects on stress. However, Han et al. (2021) included studies targeting family caregivers of both adults and young people, meaning the results of the meta-analysis do not capture the effects of ACT on caregivers of children diagnosed aged 18 or younger (<18) in isolation. This is important to highlight as there are notable differences in caring for adults and young people, as the National Alliance for Caregiving (NAC) state that caring for children is a more intensive caregiving experience than caring for adults. This is because it results in a greater burden of care, with caregivers of children spending an average of 11 h more per week providing care than

caregivers of adults aged 18 or older. Further, Han et al. (2021) did not perform a meta-analysis on ACT processes. The ACT model is specifically designed to target ACT processes, meaning assessments of changes to cognitive fusion, mindfulness, and PF in particular are crucial in evaluating the success of the intervention. Han et al.'s (2021) review also contained some methodological shortcomings. First, only one author was involved in the search process which may have introduced bias. Second, the meta-analyses only used post-test data since not all the studies were designed to measure if treatment effects were maintained over time, meaning follow-up treatment effects were not taken into consideration despite some of the reviewed studies reporting follow-up means. Further, none of the three aforementioned SR's contained a qualitative component, meaning findings regarding caregiver experiences of ACT have yet to be collated. It is therefore important to review the existing research regarding participant experiences of ACT, provided there is enough of it to draw clear and relevant themes from. This would enhance our understanding of how acceptable ACT is among caregivers of children with chronic conditions, and how to tailor the intervention for this population.

#### 1.4. Aims

This review aims to collate published studies which have applied ACT to caregivers of children with chronic conditions, in order to accurately characterise the field and examine the efficacy and acceptability of the intervention. The current research will therefore exclude studies that apply ACT to children to disentangle which factors are causing changes to caregiver health outcomes. Building on the existing evidence base, this review will adopt a mixed-methods approach, meaning a qualitative component will be included to explore caregiver experiences of ACT. The review will also conduct a meta-analysis on the effects of ACT on ACT processes including mindfulness, cognitive fusion, and PF, to assess whether ACT works via the proposed mechanisms of action, and to establish which aspects of PF are most influential on caregiver health outcomes. Moreover, this review will address methodological nuances of existing research (Byrne et al., 2020; Han et al., 2021; Jin et al., 2021) by concentrating on caregiver-focused ACT interventions and on contexts where children are diagnosed aged <18 years.

## 2. Method

### 2.1. Design

The review followed Joanna Briggs Institute (JBI) guidelines and was pre-registered on PROSPERO (ID - CRD42020178766) as a MMSR taking a convergent segregated approach to data synthesis.

### 2.2. Inclusion/exclusion criteria

The review included quantitative, qualitative, and mixed methods studies. The quantitative component of the review considered studies that evaluated the effectiveness of ACT on the broad (psychological, social, physical) health outcomes of caregivers of children (diagnosed aged <18) with chronic conditions. Studies were included that mentioned Acceptance and Commitment Therapy/Training as the intervention under study in the manuscript and referenced an existing ACT treatment protocol or stated that the applied intervention was guided by the core processes/principles of ACT. Both individual and group ACT interventions were eligible for inclusion. Studies which included child outcomes remained eligible provided they also included caregiver outcomes and did not apply ACT to children. The qualitative component of the review considered studies that explored the caregiver's experiences of ACT. Mixed method studies were only considered if data from the quantitative or qualitative components could be clearly extracted. Studies were excluded if they 1) were duplicates 2) applied

ACT to other populations; 3) applied ACT to children; 4) did not measure any relevant outcomes (mood, general wellbeing, parenting confidence, cognitive fusion, mindfulness, or PF); 5) described a hypothetical intervention; 6) were SR's; 7) were literature reviews/journal chapters; 8) were study protocols; 9) were case studies; 10) only presented preliminary findings; 11) were not published in a peer-reviewed journal; or 12) were not published in English.

### 2.3. Search procedures

Prior to the search for eligible studies a comprehensive search for existing SR's on the topic was conducted to ensure rationale for conducting the current research. Once completed, the formal search for eligible studies began. Searching for eligible studies is described by JBI as a three-stage process. The first stage involved initial limited searches of PsychInfo, Ovid MEDLINE and EMBASE from their earliest available listing to April 2020. Appendix A outlines the terms that were initially entered into all three databases for both the quantitative and qualitative searches. The initial quantitative search included terms contained in the population, intervention, and quantitative outcomes columns. The qualitative search included terms contained in the population, intervention, and qualitative phenomena of interest columns. The original searches were very narrow as only specific terms such as "Acceptance and Commitment Therapy" were entered into the databases. The second stage of searching for eligible studies included an analysis of the text words and index terms contained within the bibliographic databases of eligible studies identified from the initial search, which were then used to build a more comprehensive search strategy. Appendix B shows the terms entered into the formal search conducted on PsychInfo. These terms were altered for the other two databases depending on the subject headings they offered. The third and final phase of searching then involved a thorough hand search of the reference lists of previous SR's and of all eligible studies identified during formal screening to identify any studies which had been missed.

### 2.4. Formal screening

Following the search, all identified citations were collated and uploaded into EndNote X9 and duplicates were removed. EndNote X9 is a citation management tool used to store, organise, and cite references. The titles/abstracts of the remaining studies were then imported onto the Rayyan software for formal screening. Rayyan is a screening tool used by researchers working on SR's to speed up the process of screening and selecting studies for inclusion. There were four independent reviewers selecting studies for inclusion, all of whom are listed as authors on the paper (CDG, health psychology research PhD student; EB, research health psychologist; SW, health psychology research PhD student; CG, research clinical psychologist). Each title/abstract was screened by any two of the four reviewers. Reasons for the exclusion of each study were recorded throughout and reviewers were blinded to each other's decisions. The full text versions of the potentially relevant titles/abstracts were then retrieved and assessed in detail against the inclusion/exclusion criteria by two independent reviewers (CDG, health psychology research PhD student; SW, health psychology research PhD student) to ensure that the included studies didn't meet any of the previously outlined exclusion criteria. When there was a disagreement during the screening of either the titles/abstracts or full-text manuscripts, the two reviewers discussed and reassessed the article until an agreement was reached. On occasions where this was not possible, the two original reviewers discussed the article with a third assessor to reach a final decision.

### 2.5. Quality assessment

Study quality was assessed using JBI's standardised critical appraisal checklists. Two reviewers performed independent critical appraisal

**Table 1**  
P-curve disclosure table.

Paper	Quoted text from original paper indicating prediction of interest	Study design	Key result	Quoted text from original paper with statistical results	Results
Duncan, Coatsworth, and Greenberg (2009)	Acceptance was expected to differ significantly over time by workshop group, with ACT group participants showing greater increases in acceptance over time than Support group participants.	RCT. (ACT vs Control)	Differences of means between groups post-intervention	These analyses revealed that acceptance scores were not significantly different between the ACT workshop group and the Support workshop group at postworkshop, $t(21) = .055$ , $p = 0.955$ , $\eta^2 = .006$ .	$t(21) = .055$ , $p = 0.955$ , $\eta^2 = .006$ . $t(21) = .339$ , $p = 0.738$ , $\eta^2 = .005$ .
Cody (2007)	We hypothesised that stress, cognitive fusion and experiential avoidance processes decreased in the ACT-PT group more than in the control group.	RCT. (ACT vs Control)	Differences of means between groups post-intervention	Tests of between-subjects effects showed a significant effect of treatment on MAAS ( $F(1, 37) = 6.21$ , $p < 0.02$ , $\eta^2 = .14$ ) but no effect of treatment on CFQ ( $F(1, 37) = 0.11$ , $p = 0.74$ , $\eta^2 = .14$ ).	$F(1, 37) = 6.21$ , $p < 0.02$ , $\eta^2 = .14$ $F(1, 37) = 0.11$ , $p = 0.74$ , $\eta^2 = .14$ .
Bögels, Lehtonen, and Restifo (2010)	We hypothesised that parents of children with ABI who participated in the ACT + SSTP intervention would also demonstrate improved parenting confidence.	RCT. (ACT vs Control)	Differences in experimental group means pre-post intervention.	The ACT + SSTP group showed a significant, large increase in confidence in managing child behaviour from pre-to post-intervention (Mdiff = 15.37, SE = 3.16, $t(54.82) = 4.86$ , $p < 0.001$ , 95%CI [9.04, 21.71], $d = 0.95$ ).	$t(54.82) = 4.86$ , $p < 0.001$ , 95%CI [9.04, 21.71], $d = 0.95$ .
Free (2007)	Finding significant changes in ACT process measures would provide some evidentiary support that the improvement observed may be associated with the psychological processes targeted by the intervention as proposed by the ACT model.	Quasi-experimental study with no control group (Pre vs Post)	Differences in means across time-points.	There was a significant change in AAQ-2 across time, $F(1.75, 48.99) = 15.03$ , $p < 0.001$ , $\eta^2 = 0.35$ There was a significant change in CFQ across time, $F(1.18, 33.04) = 11.42$ , $p = 0.001$ , $\eta^2 = 0.29$ .	$F(1.75, 48.99) = 15.03$ , $p < 0.001$ , $\eta^2 = 0.35$ $F(1.18, 33.04) = 11.42$ , $p = 0.001$ , $\eta^2 = 0.29$ .
Landi, Pakenham, Crocetti, Grandi, and Tossani (2021)	Not reported	Quasi-experimental study with no control group (pre vs post)	Differences in means across time-points.	ANOVA results identified that time had a significant effect on depression ( $F(2, 54) = 9.76$ , $p < 0.001$ , $\eta^2 = 0.27$ ), stress ( $F(2, 54) = 18.17$ , $p < 0.001$ , $\eta^2 = 0.40$ ), and social isolation ( $F(2, 54) = 4.36$ , $p = 0.02$ , $\eta^2 = 0.14$ ).	$F(2, 54) = 9.76$ , $p < 0.001$ , $\eta^2 = 0.27$ $F(2, 54) = 18.17$ , $p < 0.001$ , $\eta^2 = 0.40$ , $F(2, 54) = 4.36$ , $p = 0.02$ , $\eta^2 = 0.14$

(CDG, health psychology research PhD student; SW, health psychology research PhD student). Quantitative papers selected for retrieval were assessed using JBI's 13 item RCT checklist (Appendix C) (Tufanaru et al., 2017) or nine item Quasi-experimental studies checklist (Appendix D) (Tufanaru et al., 2017), depending on study design. These checklists included questions on methodology such as whether follow-up was conducted and how data was analysed. The two qualitative studies were then assessed using JBI's 10-item qualitative research checklist (Appendix E) (Lockwood et al., 2015), which included qualitative methodological indicators such as whether there was a statement locating the researcher culturally or theoretically and whether participants, and their voices, were adequately represented. Authors of papers were contacted to request any missing or additional data for clarification, where required. Quality scores were based on what percentage of items on each checklist were met. A score above 80% (> 80%) was considered high quality, between 60% and 80% was considered moderate quality, and less than 60% (<60%) was considered low quality. However, all studies, regardless of their methodological quality, underwent data extraction and synthesis (where possible). Inter-rater reliability was computed to gauge agreement amongst reviewers before discussion and consensus. Across the 19 studies, only a moderate level of inter-rater reliability between reviewers was observed ( $k = 0.546$ ,  $p < .001$ ), as according to Krippendorff (2004) it is customary to require a  $k$  value of  $\geq 0.800$ . These findings are inconsistent with other reviews using the same critical appraisal checklists, which reported substantial agreement between reviewers (Arab-Zozani et al., 2020; Lim et al., 2022). The lower inter-rater reliability score in this review was produced because of some disagreements during critical appraisal regarding whether outcome measures used were reliable. Nonetheless, any such disagreements were again resolved through discussion or

consultation with a third reviewer.

## 2.6. Risk of bias

In order to test for publication bias among the included studies, we considered the use of funnel plots to see whether reported effect sizes correlated with sample sizes (Duval & Tweedie, 2000; Egger et al., 1997). P-curve analysis (Simonsohn et al., 2014) was also performed on the findings of five of the included studies published between 2010 and 2020, to test for publication bias and the possible issue of p-hacking within the literature.

## 2.7. Data extraction

Data was extracted from the quantitative and qualitative studies by two independent reviewers (CDG, health psychology research PhD student; SW, health psychology research PhD student) using the standardised data extraction tool in JBI SUMARI. Study country, setting, participant characteristics, group descriptions, outcomes measured, and a description of main results were extracted from the quantitative study documents. Specific details about the population, context, culture, geographical location, study methods, and the phenomena of interest relevant to the review objective were extracted from the qualitative study documents. Study investigators were contacted for unreported data.

## 2.8. Data synthesis

A convergent segregated approach to synthesis and integration was adopted during this review in accordance with the JBI methodology for

MMSR's. This involved separate quantitative and qualitative synthesis followed by integration of the resultant quantitative and qualitative evidence. Quantitative studies were, where possible, pooled with statistical meta-analysis using JBI SUMARI. Relevant health outcomes were assigned to different outcome groups including mood, general well-being, and parenting confidence. The mood outcome group included measures of depression, distress, burnout, internal shame, and mood collectively. The parenting confidence outcome group included measures of parenting self-efficacy and confidence, and the general well-being outcome group included measures of general health and QoL. Three ACT process measures (cognitive fusion, mindfulness, and PF) were also separated into different outcome groups prior to meta-analysis. Reported means, standard deviations, and sample sizes of intervention and control groups were then entered into JBI SUMARI for meta-analysis and pooled for each of the outcome groups. A random-effects model with the inverse variance method was adopted because of differences between the included studies. Standardised mean differences (SMD's) were used as the effect size statistics for outcome groups which included outcomes measured using different measurement tools. SMD's above 0.2, 0.5, and 0.8 were considered small, medium, and large, respectively. Weighted mean differences (WMD's) were used as the effect size statistics for outcome groups which included outcomes that were all assessed using the same measurement tool. Significance was determined through analysis of the forest plots, as  $p$  values of less than or equal to 0.05 ( $\leq 0.05$ ) were considered statistically significant. The forest plots also display mean scores, standard deviations, and an  $I^2$  statistic, as well as the total number of participants assigned to each group. The line of no effect labels were traditional (experimental vs control), meaning the group with lower mean scores were favoured. The  $I^2$  statistic was used to measure heterogeneity, with  $I^2$  values below 50% indicating low heterogeneity, above 50% indicating moderate heterogeneity, and above 75% indicating high heterogeneity (Higgins et al., 2003). Where meta-analysis was not possible, due to the absence of a control group or high heterogeneity between studies during statistical pooling, narrative synthesis was carried out. Outcome groups remained the same as they were during meta-analysis during narrative synthesis. Narrative synthesis involved the findings of the relevant study outcomes, along with study biases, strengths, and limitations, being manually summarised in words and tables. Qualitative research findings were, where possible, pooled using JBI SUMARI through the meta-aggregation approach. This involved the aggregation of the results sections of each qualitative paper through assembling the findings and categorizing them based on similarity in meaning to generate a set of statements that represented that aggregation. These categories were then subjected to a synthesis to produce a comprehensive set of findings that could inform evidence-based practice. The findings of each single method synthesis included in this review were then configured according to the JBI methodology for MMSR. This involved quantitative and qualitative evidence being juxtaposed together and organized/linked into a line of argument to produce an overall configured analysis.

### 3. Results

#### 3.1. Study selection

Search procedures returned 8216 studies from PsychInfo ( $n = 2792$ ), OVID Medline ( $n = 2768$ ), and EMBASE ( $n = 2656$ ) combined, 1339 of which were identified as duplicates and removed. The titles/abstracts of the remaining 6877 studies were then imported onto the Rayyan software. 6803 of the citations did not include the application of an ACT-based intervention and were therefore not related to the research question. Of the remaining 74 studies, 14 were duplicates, 11 delivered ACT to a different population, seven applied ACT to children, one measured an irrelevant outcome (cognitive regulation), four were SR's, three were literature reviews/journal chapters, three were study protocols, two were case studies, and one only presented preliminary

findings. 28 full-text studies were therefore retrieved following formal screening. Nine of the full-text studies met exclusion criteria, with seven not being published in a peer reviewed journal and two not being published in English, leaving 19 studies to be included in the review (Fig. 1). It should also be noted that when there was a disagreement between reviewers during the formal screening process, articles ended up being included 57% of the time, following a discussion with a third assessor.

#### 3.2. Description of the studies

Of the 19 studies included in the review, 17 adopted quantitative research methods including 10 pre-post studies and seven RCT's. The remaining two studies adopted qualitative research methods. The 17 quantitative studies all included both a pre-intervention and post-intervention assessment. Thirteen carried out follow-up assessments. All 19 ACT interventions were delivered in a group format with treatment lengths varying from two sessions across one week to 10 sessions across 10 weeks. Eight of the studies included a control group. Six of these involved inactive control group comparisons, while two included active control groups as comparators. One study compared three groups, including a group which received ACT combined with another intervention (Stepping Stones Triple P; SSTP), a WLC group, and a group which received the other intervention on its own. Table 2 then shows that of the 18 studies that reported caregiver gender, 94% applied ACT to a sample consisting predominantly of mothers. Further, 47% of studies were conducted exclusively with caregivers of children with autism. Finally, of the 8 studies that reported caregiver ethnicity, 75% were conducted with a sample consisting predominantly of Caucasians.

#### 3.3. Pre-post study quality

The mean quality score for pre-post studies was 7.72 ( $SD = 0.46$ ) out of nine on JBI's checklist for quasi-experimental studies, which denoted high quality ( $>80\%$ ) during the current review. Seven of the 10 pre-post studies were therefore considered high quality, while three were considered moderate quality (60%–80%) (Corti et al., 2018; Pennefather et al., 2018; Poddar et al., 2015). Several consistent strengths were apparent across the studies, as all 10 began by clearly outlining what 'cause' and 'effect' they were examining, leaving no confusion regarding the aims of each study. All 10 studies also made use of appropriate statistical analysis, and nine of the 10 used outcome measures that were psychometrically adequate, with the only exception using self-report measures which may have been difficult for caregivers to understand. However, the included pre-post studies could still have been improved through the inclusion of control groups, as only one of the 10 studies presented control group comparisons. Nonetheless, all 10 quasi-experimental studies included in the review adhered to standard procedure through taking multiple measurements of the outcome at both pre and post-intervention, contributing to their high overall quality score.

#### 3.4. RCT studies quality

The mean quality score of studies using an RCT design was 9.29 ( $SD = 2.71$ ) out of 13 on JBI's checklist for RCT's, which denoted moderate quality (60%–80%) during the current review. Three out of the seven RCT's were considered moderate quality. Of the remaining four RCT's, two were of high quality ( $>80\%$ ) and two were low quality ( $<60\%$ ). True randomisation was used for assignment of participants to treatment groups in six of the seven RCT studies, with the one exception being a study which mentioned randomisation but was unclear on how randomisation was performed. All seven RCT's also made use of appropriate statistical analysis and were found to measure outcomes in a reliable way. However, the included RCT's could have been improved through providing more clarity in certain areas. For example, three out of the

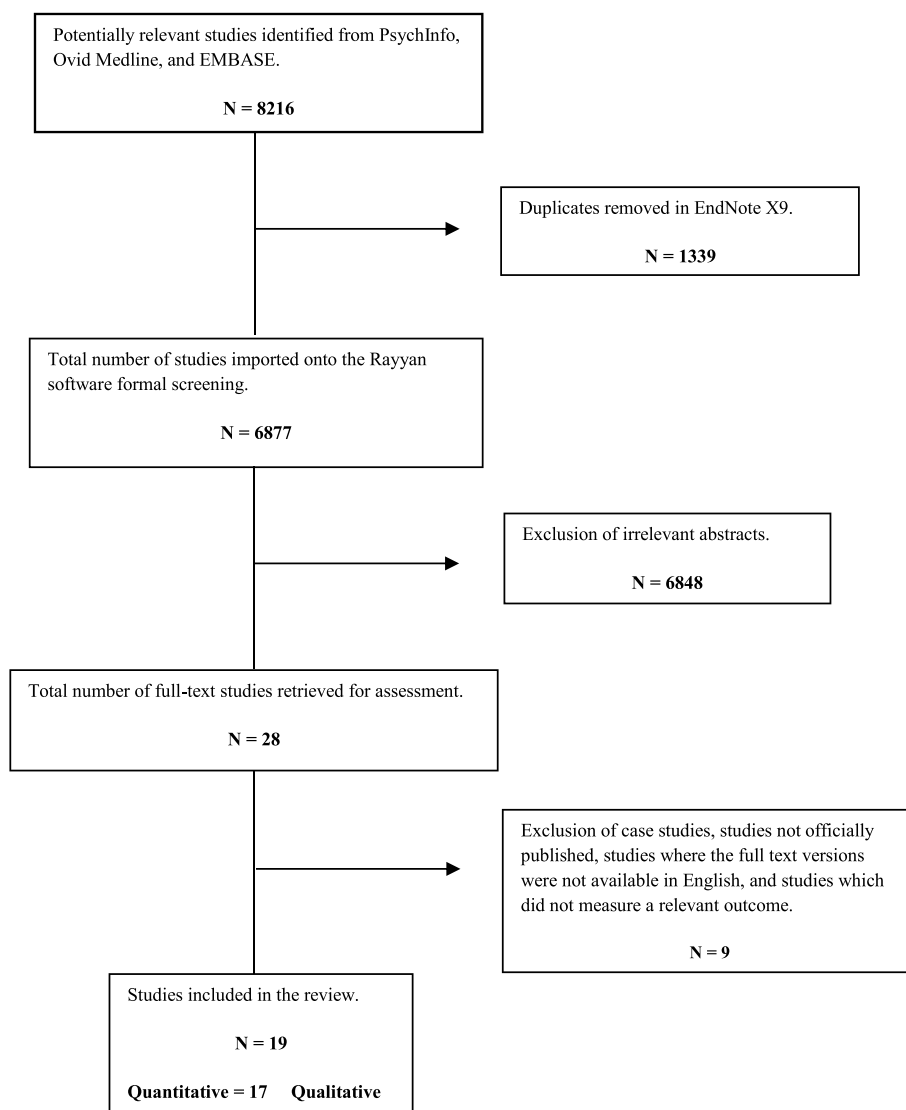


Fig. 1. Prisma study flowchart.

seven RCT's were unclear on whether treatment groups were concealed. Moreover, only two of the seven made it clear that participants had been blinded to treatment assignment. It should also be noted that in one of the studies, all participants in the TAU group withdrew prior to completing the treatment, meaning comparisons could not be drawn between the experimental and control groups. However, despite containing some inconsistencies, all 7 RCT's were found to have an appropriate trial design, with any deviations from the RCT design being accounted for in the conduct of the trial.

### 3.5. Qualitative studies quality

Given that only two qualitative studies met eligibility criteria, the mean is not a useful indicator of quality. Of these two qualitative studies included, one scored within the threshold to denote a moderate quality study (60%–80%), while the other was considered a low-quality study ( $\leq 60\%$ ). This low-quality score came from the authors not clarifying their chosen research methodology, which meant items one to five on the checklist could not be met due to a lack of clarity regarding the congruity between the research methodology and the philosophical perspective, research question, methods, analysis of data, and interpretation of results. The authors also failed to provide evidence of ethical approval by an appropriate body. However, despite these areas

for improvement, both studies adequately represented participants through direct quotations and drew conclusions which flowed from analysis of the data.

### 3.6. Risk of bias within studies

Given that fewer than 10 studies were included in the meta-analysis, funnel plots were unable to be used as a way of testing publication bias, as the power of the tests was too low to distinguish chance from real asymmetry (Higgins et al., 2019). However, p-curve analysis was still conducted on five of the included studies (Table 2) and revealed that both the half and full p-curve tests were right-skewed with  $p < 0.01$  (Fig. 2). A set of studies with sufficient power and low amounts of bias/p-hacking tend to produce a right-skewed p-curve, as this indicates that the studies contained more low (E.g.,  $p = 0.01$ ) than high ( $p = 0.04$ ) significant p values (Simonsohn et al., 2014). Fig. 2 therefore suggests that neither publication bias nor p-hacking impacted the findings of the review.

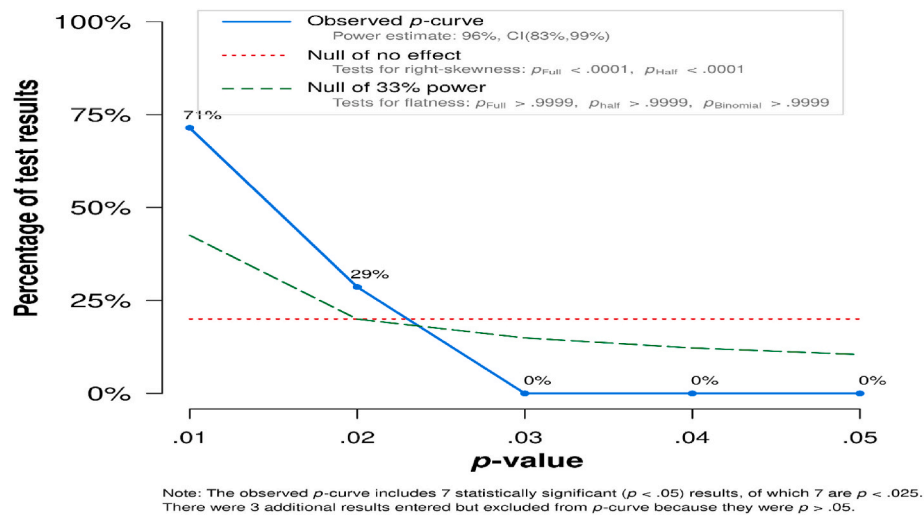
### 3.7. Meta-analysis

#### 3.7.1. Mood

Six means collected by four different mood outcome measures

**Table 2**  
Demographic characteristics of participants in included studies.

Study	Child's condition	Total sample size	Mothers	Fathers	Ethnicity (Caucasian)	Ethnicity (Other)	Child age range	Child age mean
Chong et al. (2019)	Asthma	168	148	20	Not reported	Not reported	3–12	6.81
Hahs et al. (2019)	Autism	18	13	5	12	6	5–13	8.44
Drouillard (2019)	Autism	23	18	5	9	15	Not reported	10.65
Whittingham et al. (2016)	Cerebral Palsy	67	65	2	Not reported	Not reported	2–12	5.3
Sairanen et al. (2019)	Any chronic condition	74	60	14	Not reported	Not reported	0–18	10.3
Corti et al. (2018)	Autism	42	22	20	Not reported	Not reported	2–4	2.92
Bögels, Lehtonen, and Restifo (2010)	Any brain injury	59	53	6	52	7	2–12	7
Blackledge and Hayes (2006)	Autism	20	15	5	12	8	Not reported	Not reported
Fung et al. (2018)	Autism	29	29	0	Not reported	Not reported	Not reported	10.40
Lunsky et al. (2018)	Autism	29	29	0	Not reported	Not reported	Not reported	10.40
Pennefather et al. (2018)	Autism	23	21	2	Not reported	Not reported	4–8	6
Burke et al. (2014)	Any chronic condition	11	Not reported	Not reported	Not reported	Not reported	Not reported	5.7
Poddar et al. (2015)	Any neurodevelopmental condition	10	10	0	Not reported	Not reported	Not reported	9.87
Rayner et al. (2016)	Any chronic condition	11	11	0	Not reported	Not reported	0–18	3.6
Wallace et al. (2016)	Any chronic pain	8	8	0	8	0	13–18	Not reported
Weiss et al. (2019)	Any chronic pain	212	188	24	192	20	Not reported	15.33
Kowalkowski (2012)	Autism	17	17	0	13	4	4–11	0
Reid et al. (2016)	Autism	5	5	0	1	4	9–14	Not reported
Thompson-Janes et al. (2016)	Any learning disability	11	5	6	Not reported	Not reported	5–15	Not reported



**Fig. 2.** Line graph produced by p-curve analysis of five studies conducted between 2010 and 2020 testing the effects of ACT on general health outcomes.

(Depression, Anxiety, and Stress Scale's, DASS; Internal Shame Scale, ISS; Beck Depression Inventory–II, BDI- II; Shirom-Melamed Burnout Questionnaire, SMBQ) across four studies from Table 3 were included during meta-analysis of mood outcomes. Effect sizes were standardised as the included studies measured similar outcomes but used different measurement instruments. Three random effects meta-analyses then tested the effects of ACT on mood at post-intervention, follow-up, and both time points combined (post-intervention and follow-up combined). Heterogeneity between measures was moderate at post intervention ( $I^2 = 54%$ ) and both time points combined ( $I^2 = 51%$ ), and low at follow-up ( $I^2 = 48%$ ). Meta-analysis revealed a small SMD at post-intervention (SMD =  $-0.49$ , 95% CI =  $[-0.82, -0.16]$ ), and moderate SMD's at follow-up (SMD =  $-0.64$ , 95% CI =  $[-0.97, -0.31]$ ) and both time-

points combined (SMD =  $-0.55$ , 95% CI =  $[-0.78, -0.31]$ ). Further inspection of forest plots (Figs. 3–5) revealed that SMD's were significant at post-intervention ( $Z = -2.92$ ,  $P = 0.004$ ), follow-up ( $Z = -3.82$ ,  $P = 0$ ), and both time-points combined ( $Z = -4.62$ ,  $P = 0$ ). It was also observed that all three forest plots favoured the experimental group, meaning the experimental groups mood scores were significantly lower than the control groups at these time points.

### 3.7.2. General wellbeing

No meta-analysis was conducted on general wellbeing as only one study from Table 3 included a measure of general wellbeing.

**Table 3**  
Overview of studies included in meta-analysis.

Study	Design	Format of intervention	Was the intervention tailored?	Treatment Length	Study quality score	Control	Analysis	Outcomes (measures)	Outcome Group	Group means (SD's) at post-intervention	Groups means (SD's) at follow-up
Chong et al. (2019)	RCT	Group intervention	ACT protocol was modified based on ACT training manuals used in previous studies, which led to positive effects for caregivers of children with chronic conditions,	4 × 2 hour sessions	13/13	Asthma education talk plus 3 telephone follow-ups	Generalised estimating equations examined differences between groups at post-intervention and follow-up, as well as differences in improvements between groups across time points.	Acceptance and Action Questionnaire-II (AAQ-II) Depression, Anxiety, and Stress Scale (DASS-21) Parent Asthma Management Self-Efficacy Scale (PAMSES) Pediatric Asthma Caregiver's Quality of Life Questionnaire (PACQOL)	Psychological Flexibility Mood Parenting Confidence General wellbeing	Experimental group – 16.13 (6.6) Control group – 19.42 (9.44) Experimental group – 14.35 (13.56) Control Group – 17.11 (19.25) Experimental group – 3.99 (0.64) Control Group – 3.92 (0.64) Experimental group – 5.66 (1.01) Control group – 5.18 (1.28)	Experimental group – 14.67 (6.6) Control group – 20.4 (8.16) Experimental group – 13.97 (11.36) Control Group – 20.02 (17.78) Experimental group – 3.94 (0.64) Control Group – 3.72 (0.64) Experimental group – 5.67 (0.92) Control group – 5.32 (1.10)
Hahs et al. (2019)	RCT	Group intervention	No	2 × 2 hour sessions	7/13	Inactive	One-tailed unpaired <i>t</i> -test of change scores examined differences in improvements between groups from pre to post-intervention.	Internalized Shame Scale (ISS) Cognitive Fusion Questionnaire (CFQ) Beck Depression Inventory-II (BDI-II) Mindfulness Attention Awareness Scale (MAAS) Frieberg Mindfulness Inventory (FMI)	Mood Cognitive fusion Mood Mindfulness Mindfulness	Experimental group - 41.33 (7.69) Control group – 58.67 (10.54) Experimental group – 31.56 (6.44) Control group – 36.22 (15.63) Experimental group – 5.22 (4.41) Control group – 7.44 (4.39) Experimental group – 73.56 (8.29) Control group – 56.78 (10.94) Experimental group – 43.89 (8.29) Control group – 39.56 (7.13)	No Follow-up No Follow-up No Follow-up No Follow-up
Drouillard (2019)	RCT	Group intervention	ACT metaphors and activities were modified to refer directly to the experience of parenting a child with ASD.	1 × 5.5 h session	9/13	Treatment-related information with supplemented general parent support	Split-plot repeated measures (ANOVAs) to identify differences between groups at all time points combined and differences in improvements between	Acceptance and Action Questionnaire-II (AAQ-II) Cognitive Fusion Questionnaire (CFQ)	Psychological flexibility Cognitive fusion	Experimental group – 17.57 (11.73) Control group – 19.2 (10.14) Experimental group – 18.64	Experimental Group – 13.58 (4.44) Control Group – 18.45 (7.95) Experimental group – 15.42

(continued on next page)



Table 3 (continued)

Study	Design	Format of intervention	Was the intervention tailored?	Treatment Length	Study quality score	Control	Analysis	Outcomes (measures)	Outcome Group	Group means (SD's) at post-intervention	Groups means (SD's) at follow-up
Whittingham et al. (2016)	RCT	Group intervention	No	8 × 2 hour sessions	13/13	WLC group SSTP group.	groups across time points (Time × Group interaction). Analyses of covariance (ANCOVAs) examined differences between groups at post-intervention and follow-up.	Depression, Anxiety, and Stress Scale (DASS-21) Cerebral Palsy Daily Parenting Tasks Checklist (CP-DPTC)	Mood Parenting confidence	(11.36) Control group – 18.93 (9.88) Experimental group – 9.89 (12.56) Control group – 24.58 (25.21) Experimental group – 90.97 (22.59) Control Group – 86.12 (15.57)	(8.33) Control group – 19.64 (8.7) No Follow-up No Follow-up
Sairanen et al. (2019)	RCT	Group intervention	Acceptance-exercises focusing on one's relationship with self were included, since recent studies have supported this as useful for parents of children with chronic conditions.	Not reported	9/13	Inactive	Hierarchical linear modelling (HLM) (Wald test) examined differences in improvements between groups across time points (Time × Group interaction).	Shirom-Melamed Burnout Questionnaire (SMBQ) Depression, Anxiety, and Stress Scale (DASS-21) Acceptance and Action Questionnaire-II (AAQ-II) Cognitive Fusion Questionnaire (CFQ) Five Facet Mindfulness Questionnaire (FFMQ)	Mood Mood Psychological flexibility Cognitive fusion Mindfulness	Experimental group – 4.01 (1.3) Control group – 4.74 (1.06) Experimental group – 30.66 (24.7) Control group – 34.44 (20.01) Experimental group – 19.46 (8.8) Control group – 21.43 (11.42) Experimental group – 42.31 (15.33) Control group – 41.82 (16.54) Experimental group – 132.28 (21.83) Control group – 124.98 (31.55)	Experimental group – 3.39 (1.56) Control Group – 4.78 (1.32) Experimental group – 23.9 (27.11) Control Group – 47.73 (38.74) Experimental group – 17.96 (8.26) Control group – 21.23 (11.22) Experimental group – 39.73 (14.42) Control group – 47.74 (16.33) Experimental group – 137.55 (23.06) Control group – 116.23 (21.06)
Corti et al. (2018)	Pre-post with control group	Group intervention	No	12 × 1.5 h sessions	7/9	Inactive	Two-way repeated measures ANOVA examined differences in improvements between groups across all time points. MANCOVA analysis examined differences between groups post-intervention.	Cognitive Fusion Questionnaire (CFQ) Mindfulness Attention Awareness Scale (MAAS)	Cognitive Fusion Mindfulness	Experimental group – 21.3 (6.55) Control group – 19.5 (4.5) Experimental group – 4.33 (0.17) Control group – 4.74 (0.54)	No Follow-up No Follow-up
Bögels et al. (2010)	RCT	Group intervention	No	8 × 2 hour sessions	9/13	Inactive	Mixed model repeated measures (MMRM) linear regression analyses.	Parenting Tasks Checklist (PTC)	Parenting confidence	Experimental group – 86.27 (14.58) Control group – 73.37 (13.91)	Not analysed

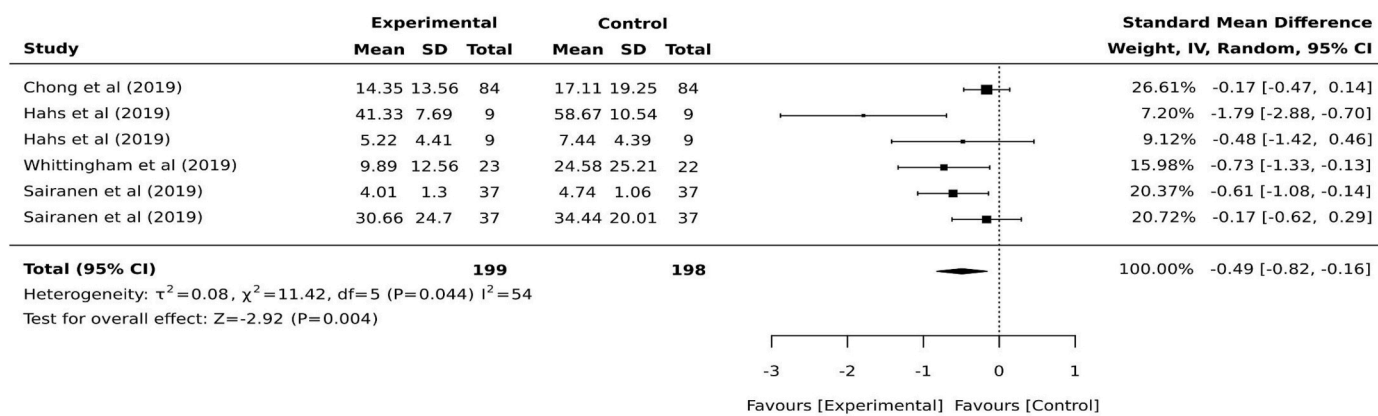


Fig. 3. Forest plot produced by random effects meta-analysis of mood at post-intervention.

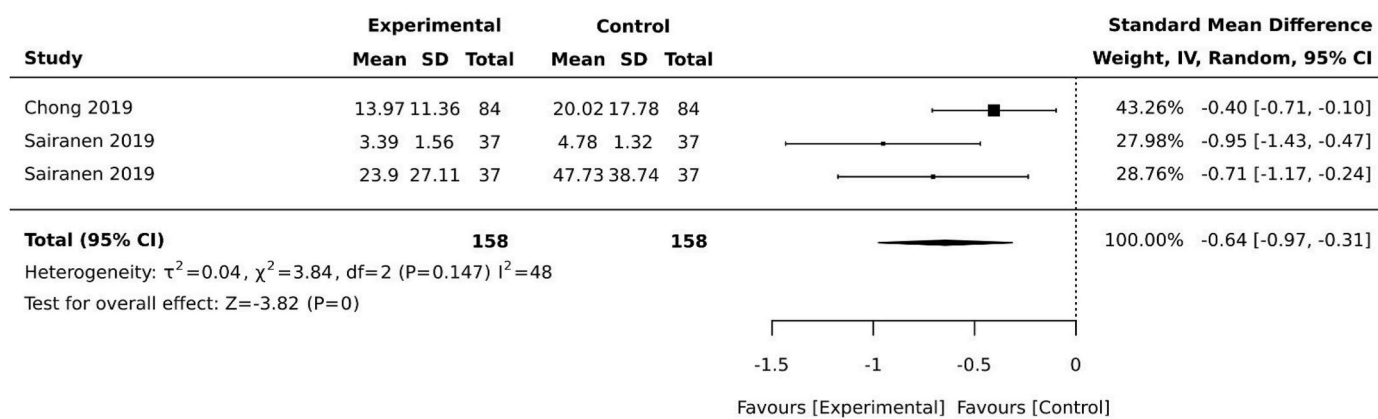


Fig. 4. Forest plot produced by random effects meta-analysis of mood at follow-up.

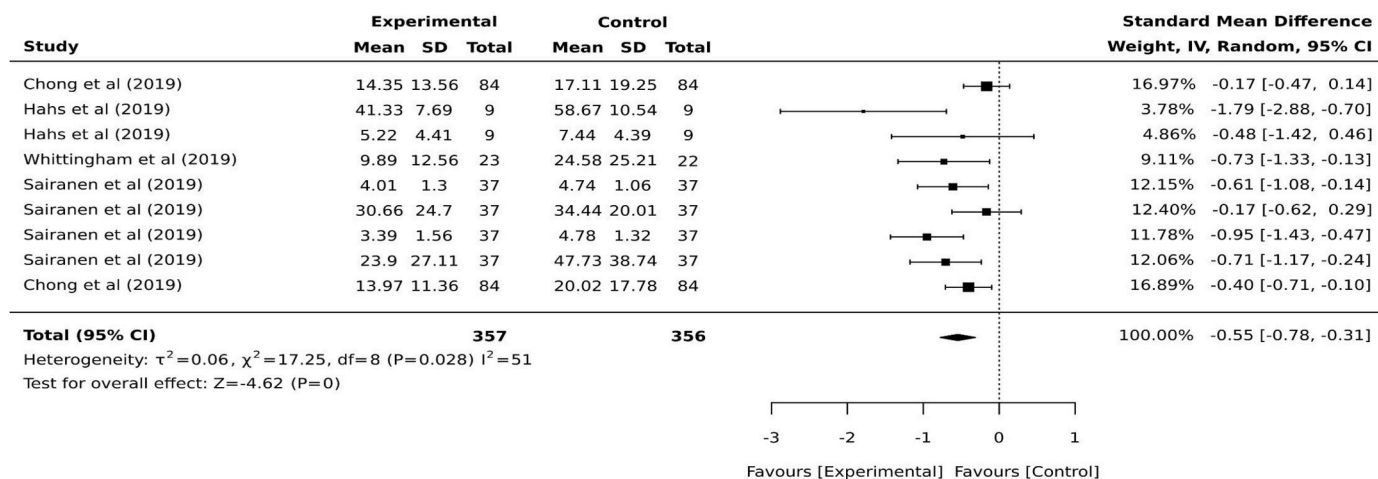


Fig. 5. Forest plot produced by random effects meta-analysis of mood at both time-points combined.

### 3.7.3. Parenting confidence

Three means collected by three different parenting confidence measurement instruments (Parent Asthma Management Self-Efficacy Scale, PAMSES; Cerebral Palsy Daily Parenting Tasks Checklist, CP-DPTC; Parenting Tasks Checklist, PTC) from Table 3 were included during meta-analysis of parenting confidence outcomes, meaning effect sizes were again standardised. Two random effects meta-analyses tested the effects of ACT on parenting confidence at post-intervention and both time points combined. No meta-analysis was conducted on parenting

confidence at follow-up due to only one of the parenting confidence outcomes from Table 3 being assessed at this time point. Heterogeneity of mean scores was moderate at post-intervention ( $I^2 = 65%$ ) and small at both time-points combined ( $I^2 = 48%$ ). Meta-analysis then revealed small SMD's at post-intervention (SMD =  $-0.38$ , 95% CI =  $[-0.09, 0.84]$ ) and both time points combined (SMD =  $0.34$ , 95% CI =  $[0.06, 0.62]$ ). Inspection of the forest plots (Figs. 6 and 7) revealed these effects were significant at both time-points combined ( $Z = 2.37$ ,  $P = 0.018$ ), but not at post-intervention ( $Z = 1.58$ ,  $P = 0.114$ ). Further, the forest plot at

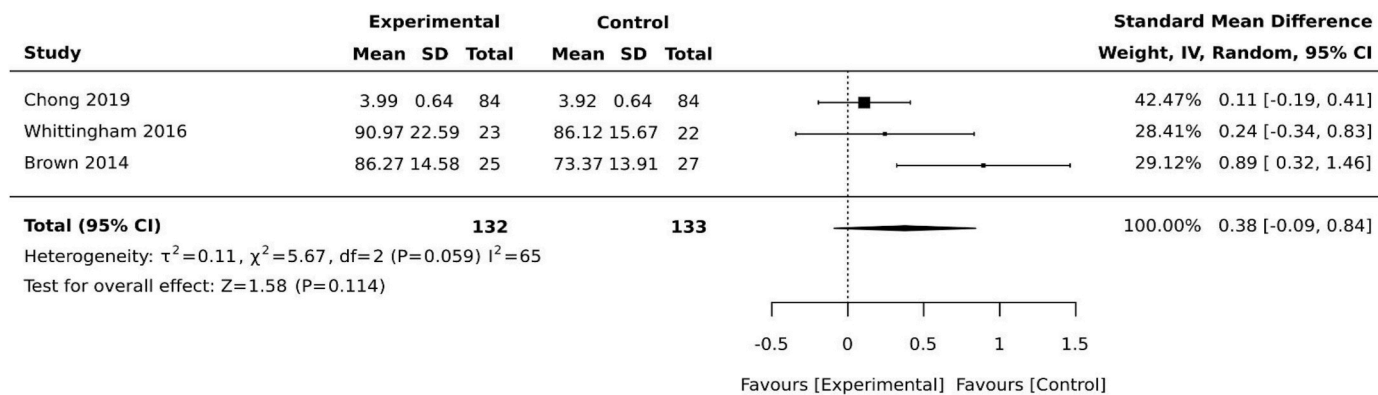


Fig. 6. Forest plot produced by random effects meta-analysis of parenting confidence at post-intervention.

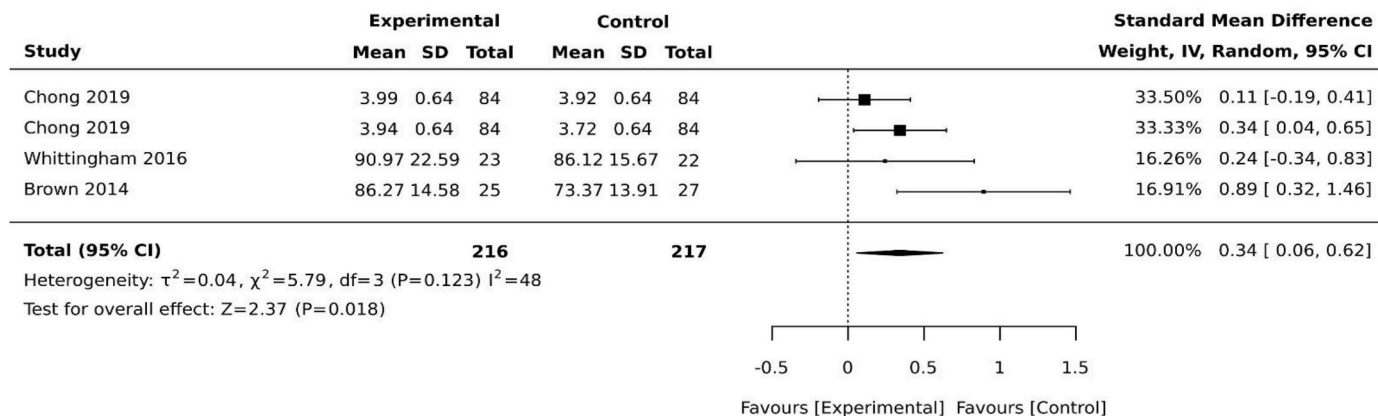


Fig. 7. Forest plot produced by random effects meta-analysis of parenting confidence at both time points combined.

both time-points combined favoured the control group, indicating that parenting confidence was significantly higher among caregivers in ACT groups than caregivers in control groups at this time-point.

### 3.7.4. Cognitive fusion

Four means collected using the same measurement instrument (Cognitive Fusion Questionnaire, CFQ) were included during meta-analysis of cognitive fusion, meaning effect sizes were expressed as WMD's. Another three random effects meta-analyses then tested the effects of ACT on cognitive fusion at both time points combined, post-intervention, and follow-up. Heterogeneity between studies was low at both time points combined ( $I^2 = 43%$ ) and non-existent at post-

intervention ( $I^2 = 0%$ ) and follow-up ( $I^2 = 0%$ ). Meta-analysis revealed a small WMD at both time points combined (WMD =  $-1.91$ , 95% CI =  $[-5.38, 1.56]$ ). However, inspection of the forest plot (Fig. 10) revealed that this WMD was not statistically significant ( $Z = -1.08$   $P = 0.281$ ). Pooling also revealed a small WMD at post-intervention (WMD =  $0.98$ , 95% CI =  $[-1.82, 3.77]$ ). However, inspection of the forest plot (Fig. 8) again revealed that effects were not statistically significant ( $Z = 0.69$ ,  $P = 0.493$ ). The final meta-analysis then revealed a large WMD at follow-up (WMD =  $-6.12$ , 95% CI =  $[-11.09, -1.15]$ ). Inspection of the forest plot (Fig. 9) revealed that the effects were significant at this time point ( $Z = -2.4$ ,  $P = 0.016$ ), and that forest plots favoured the experimental group, meaning the experimental groups CFQ scores were

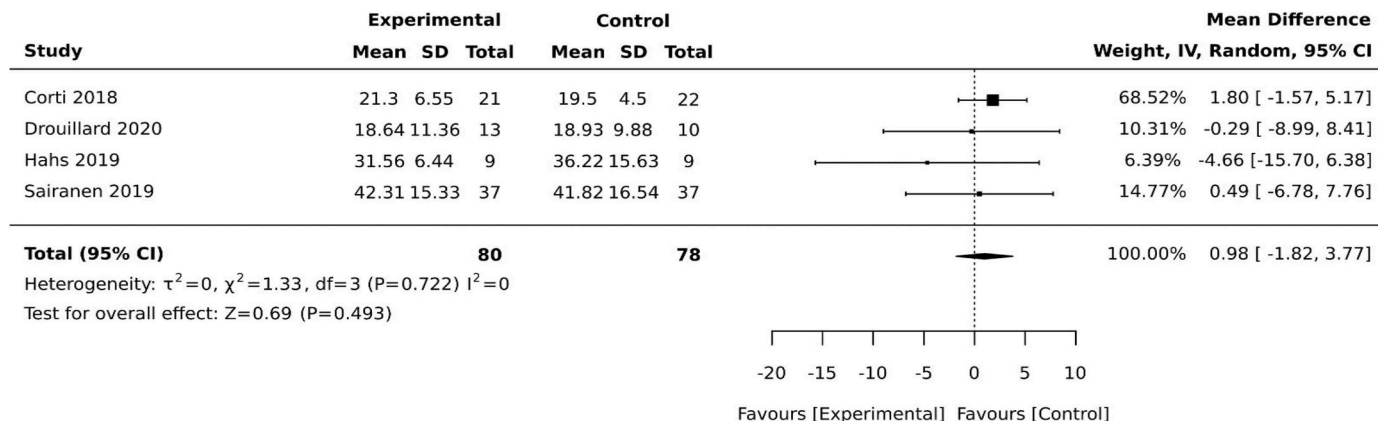


Fig. 8. Forest plot produced by random effects meta-analysis of cognitive fusion at post-intervention.

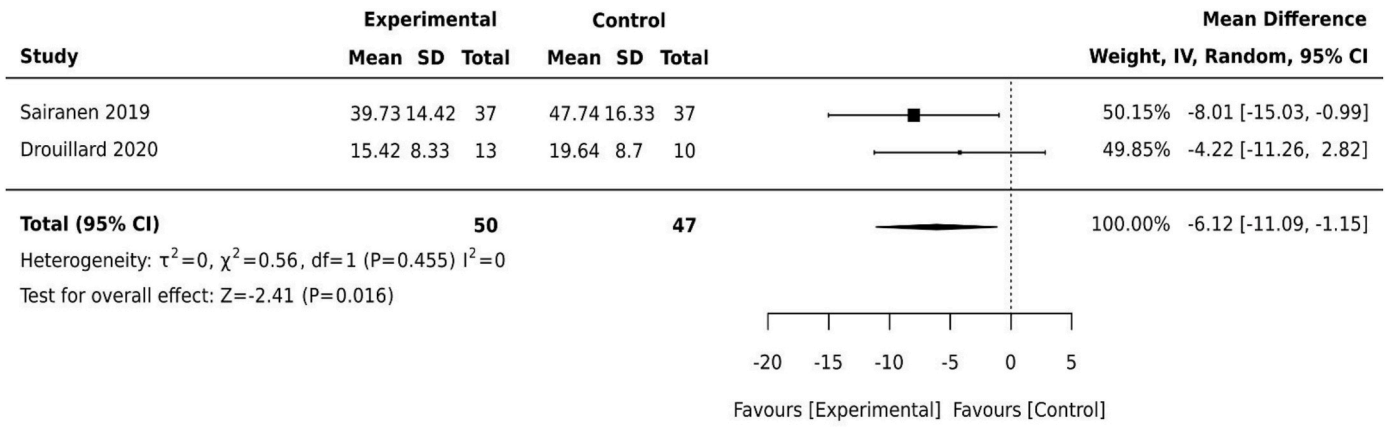


Fig. 9. Forest plot produced by random effects meta-analysis of cognitive fusion at follow-up.

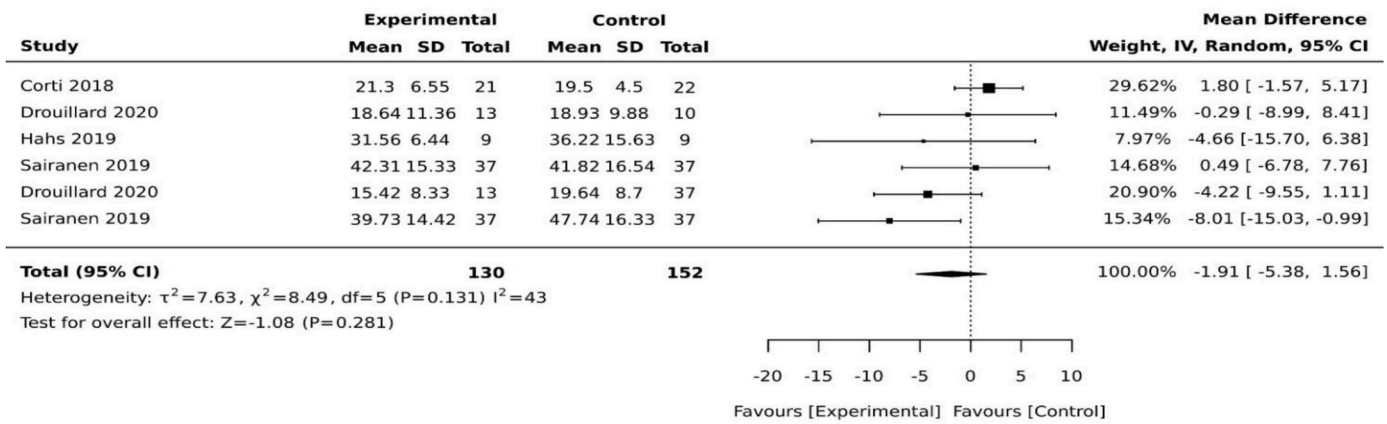


Fig. 10. Forest plot produced by random effects meta-analysis of cognitive fusion at both time points combined.

significantly lower than the control groups at follow-up.

### 3.7.5. Mindfulness

Four means collected by three different mindfulness outcome measures (Mindfulness Attention Awareness Scale's, MAAS; Frieburg Mindfulness Inventory, FMI; Five Facet Mindfulness Questionnaire, FFMQ) across three studies from Table 3 were included during meta-analysis of mindfulness outcomes. Effect sizes were standardised as the included studies used different measurement instruments to assess mindfulness. Heterogeneity of studies measuring mindfulness was high at post-intervention ( $I^2 = 88$ ) and at both time points combined ( $I^2 =$

88), which could mean that the studies included in these meta-analyses have nothing in common. Further, only one of the mindfulness outcomes from Table 3 was assessed at follow-up. It was therefore inappropriate to synthesise mindfulness outcome means at any time point.

### 3.7.6. Psychological flexibility

Three means collected using the same measurement instrument (Acceptance and Action Questionnaire-II, AAQ- II) were included during meta-analysis of PF, meaning effect sizes were expressed as WMD's. A further three random effects meta-analyses were performed to test the effects of ACT on PF at all three time points. Heterogeneity was low at

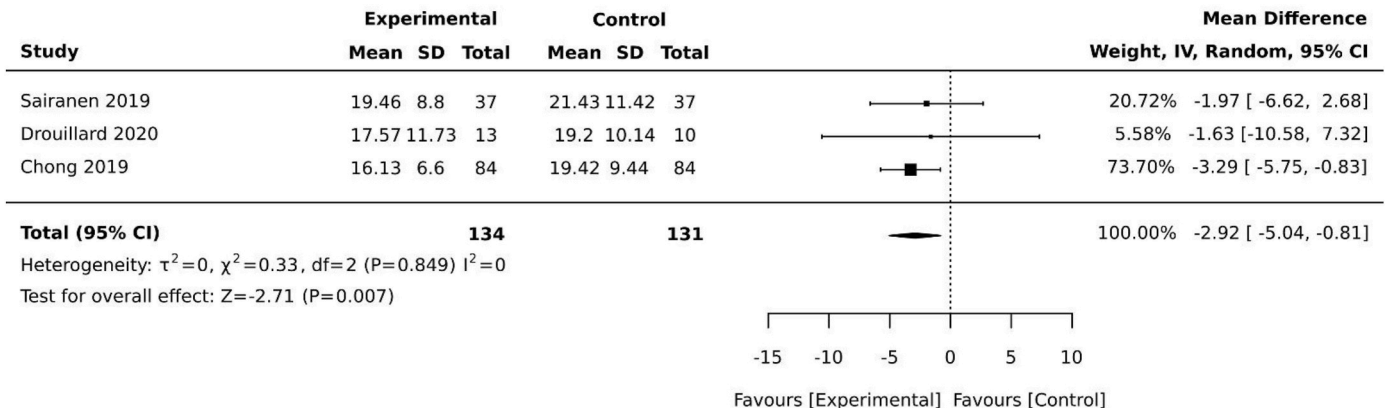


Fig. 11. Forest plot produced by random effects meta-analysis of psychological flexibility at post-intervention.

both time points combined ( $I^2 = 20$ ), and non-existent at post-intervention ( $I^2 = 0$ ), and follow-up ( $I^2 = 0$ ). Meta-analysis revealed moderate to large WMD's at post-intervention (WMD =  $-2.92$ , 95% CI =  $[-5.04, -0.81]$ ), follow-up (WMD =  $5.19$ , 95% CI =  $[-7.08, -3.31]$ ), and both time points combined (WMD =  $-3.89$ , 95% CI =  $[-5.59, -2.19]$ ). Analysis of the forest plots (Figs. 11–13) then found that effect sizes were statistically significant at post-intervention ( $Z = -2.71$ ,  $P = 0.007$ ), follow-up ( $Z = -0.54$ ,  $P = 0$ ), and both time points combined ( $Z = -4.49$ ,  $P = 0$ ). It was also observed that all three forest plots favoured the experimental group, meaning the experimental groups PF scores were significantly lower than the control groups at these time points.

### 3.8. Narrative synthesis

#### 3.8.1. Mood

Table 4 outlines a full breakdown of each of nine studies that did not contain a control group, as well as the studies from Table 3 which included outcomes that could not be synthesised through meta-analysis. The mood outcome group included measures of depression, anxiety, and distress. Two studies in Table 4 produced a combined score for all three of these outcomes using the DASS (Lunsky et al., 2018; Rayner et al., 2016). Rayner et al. (2016) found no significant change in DASS scores across all three time points. However, a more recent study found significant improvements in DASS scores from pre to post-intervention, pre-intervention to follow-up, post-intervention to follow-up, and across all time points (Lunsky et al., 2018). It is therefore difficult to conclude whether ACT has any significant impacts on mood when only looking at studies that measure all three mood outcomes collectively, as Lunsky et al. (2018) and Rayner et al. (2016) revealed conflicting findings despite using the same outcome measure and both receiving high quality scores during critical appraisal. To resolve this conflict within the existing literature, it is important to also synthesise findings of included studies that measured moods individually. For example, two studies in Table 4 included measures of depression (Blackledge & Hayes, 2006; Weiss et al., 2019). The first revealed improvements in depression from pre to post-intervention and pre-intervention to follow-up (Blackledge & Hayes, 2006). The more recent study of the two then supported Blackledge and Hayes (2006) findings and reported significant reductions in depression from pre to post-intervention, which were maintained from post-intervention to follow-up (Weiss et al., 2019). None of the studies in Table 4 measured anxiety on its own. However, Five of the 14 studies evaluated the effects of ACT on psychological distress (Blackledge & Hayes, 2006; Burke et al., 2014; Kowalkowski, 2012; Pennefather et al., 2018; Rayner et al., 2016). Rayner et al. (2016) revealed that changes to distress scores across all three time points were not significant. Further, the most recent study of the five only revealed a medium effect of ACT on psychological distress from pre-post intervention (Pennefather et al., 2018). However, Kowalkowski (2012) revealed conflicting findings to Pennefather et al. (2018), as significant effects

were identified from pre to post-intervention. Burke et al. (2014) then revealed significant reductions in distress symptoms from pre to post-intervention, as well as from post-intervention to follow-up and across all three time points. Finally, Blackledge and Hayes (2006) supported the findings of Burke et al. (2014) and Kowalkowski (2012), as significant effects of ACT on psychological distress were again identified, this time from pre to post-intervention and pre-intervention to follow-up. Three previously discussed studies also measured parental distress (Kowalkowski, 2012; Lunsky et al., 2018; Pennefather et al., 2018). Lunsky et al. (2018) revealed significant effects of ACT from pre to post intervention, pre-intervention to follow-up, and across all three time points. The other two studies reported non-significant or small effects of ACT on parental distress from pre to post-intervention (Kowalkowski, 2012; Pennefather et al., 2018). In summary, two outcomes measuring moods collectively and 10 measuring individual moods were identified across seven studies in Table 4. Of these 12 outcomes, 10 directly compared mean scores taken at pre and post-intervention, with seven finding either significant or large changes in mood scores between the two time points. Seven outcomes then directly compared the mean scores taken at pre-intervention and follow-up, with six reporting significant changes between the time points. Only three outcomes compared scores from post-intervention to follow-up, with two reporting non-significant effects, and the other reporting only small effects. Finally, six of the 12 mood outcomes were measured and compared across all three time points, with four revealing significant improvements because of the ACT intervention. Therefore, despite having non-significant or small effects on caregiver mood from post-intervention to follow-up, ACT did have significant impacts on a majority of mood outcomes measurements taken from pre to post-intervention, pre-intervention to follow-up, and across all three time points. Further, most of the seven studies that included a measurement of mood received high quality scores, indicating that these findings are reliable.

#### 3.8.2. General wellbeing

The general wellbeing outcome group included measures of general health and QoL. Two studies in Table 4 measured general health (Blackledge & Hayes, 2006; Poddar et al., 2015). Poddar et al. (2015) found significant effects of ACT on general health from pre to post-intervention. Blackledge and Hayes (2006) provided support for Poddar et al.'s (2015) research, as results revealed significant effects of ACT from pre to post-intervention, as well as from pre-intervention to follow-up (Blackledge & Hayes, 2006). There were also only two studies in Table 4 that measured QoL (Chong et al., 2019; Poddar et al., 2015). Poddar et al. (2015) again indicated significant effects of ACT from pre to post-intervention. Chong et al. (2019) then supported Poddar et al.'s (2015) findings, revealing significant differences on QoL measures between groups at post-intervention and follow-up, favouring parents in the ACT group at both time points. In summary, these findings suggest

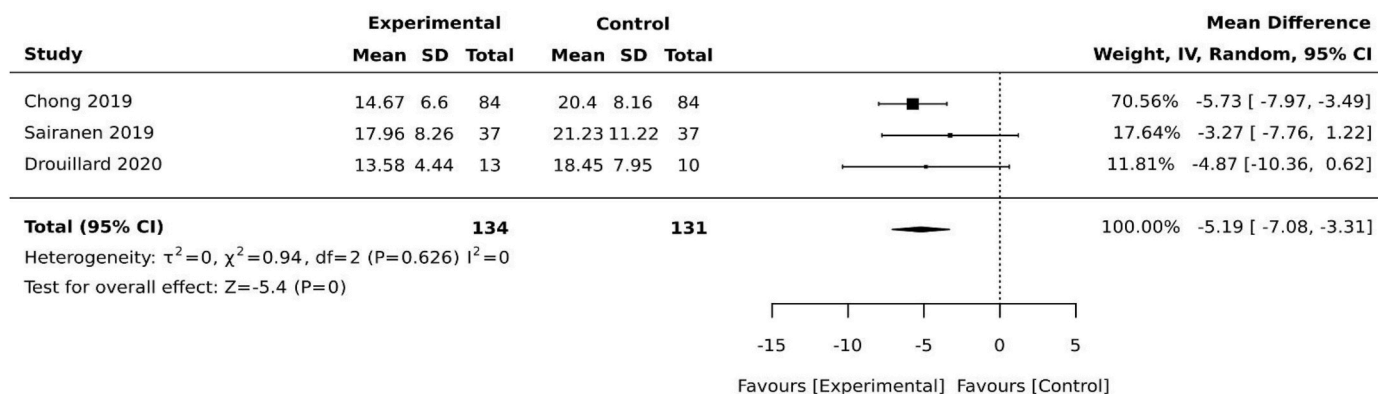


Fig. 12. Forest plot produced by random effects meta-analysis of psychological flexibility at follow-up.

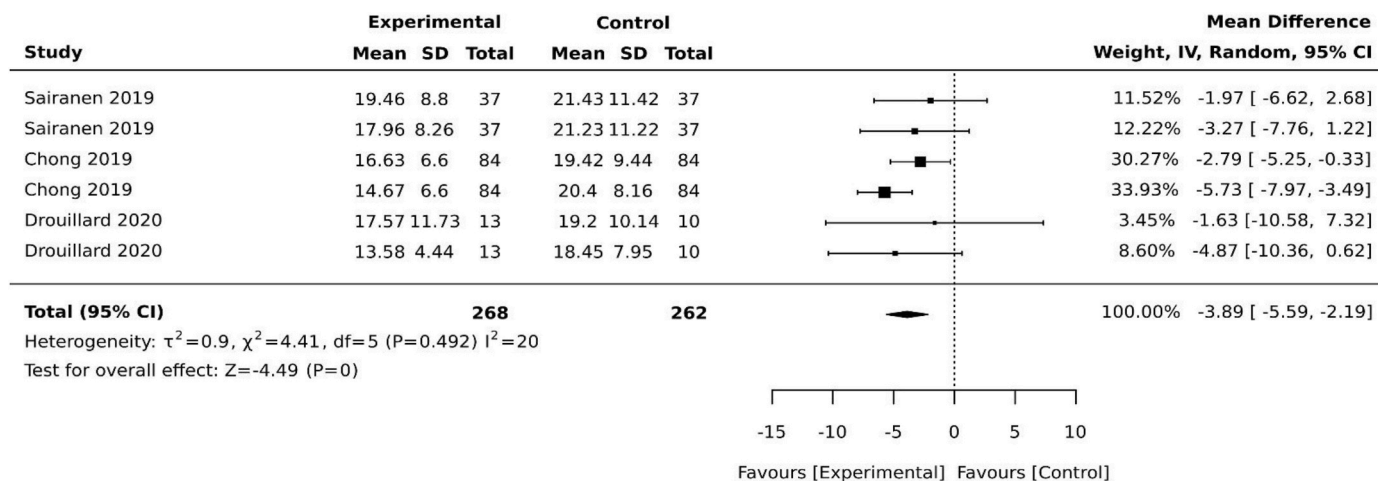


Fig. 13. Forest plot produced by random effects meta-analysis of psychological flexibility at both time points combined.

significant effects of ACT on general wellbeing from pre to post-intervention, and pre-intervention to follow-up. Despite Poddar et al.'s (2015) moderate quality score, Blackledge and Hayes (2006) received as high a quality score as any pre-post study included in this review, while Chong et al.'s (2019) RCT received a perfect quality score, highlighting the strong reliability of these findings.

3.8.3. Parenting confidence

No studies included in Table 4 measured this construct.

3.8.4. Cognitive fusion

Only one of the studies in Table 4 measured cognitive fusion (Fung et al., 2018). Caregivers reported significant improvements in cognitive fusion scores from pre to post-intervention, pre-intervention to follow-up, and post-intervention to follow-up. Further, Fung et al.'s (2018) research received a high-quality score during critical appraisal, indicating that these findings are reliable.

3.8.5. Mindfulness

Four outcomes across three studies from Table 4 measured mindfulness (Burke et al., 2014; Hahs et al., 2019; Sairanen et al., 2019). A previously discussed pre-post study found that ACT had medium effects on MAAS scores from pre to post-intervention and post-intervention to follow-up, as well as statistically significant effects across all time points (Burke et al., 2014). An RCT then revealed significant differences in improvements between groups from pre to post-intervention on the MAAS, but not on the FMI (Hahs et al., 2019). Finally, Sairanen et al. (2019) revealed significant differences in improvements in favour of the ACT group from pre to post-intervention and across all three time points, but not from post-intervention to follow-up (Sairanen et al., 2019). In conclusion, significant effects of ACT on mindfulness were identified across all three time points on all mindfulness outcome measurements taken at this time point. However, it remains difficult to make further conclusions regarding the effects of ACT at other time-points due to conflicting findings from pre-post intervention and post-intervention to follow-up. Two of these three studies received high quality scores, indicating that these findings are reliable.

3.8.6. Psychological flexibility

Eight previously discussed studies from Table 4 included PF outcomes (Blackledge & Hayes, 2006; Burke et al., 2014; Fung et al., 2018; Kowalkowski, 2012; Poddar et al., 2015; Rayner et al., 2016; Wallace et al., 2016; Weiss et al., 2019). Six compared PF scores at pre-intervention to scores at post-intervention, with four finding significant effects. Three outcomes then compared pre-intervention scores to follow-up scores, with all three finding significant effects of ACT on PF

between these time points. Three of the outcomes also compared post-intervention and follow-up scores, with one finding significant effects, one medium effects, and one non-significant effects of ACT. Finally, four studies compared scores across all time points, with all four finding significant effects of ACT on PF. Therefore, the effects of ACT on PF are significant from pre to post-intervention, pre-intervention to follow-up, and across all time points. However, it remains unclear whether there are significant effects of ACT on PF from post-intervention to follow-up, due to conflicting findings across the existing research in this area. Of the eight studies that included a PF measurement, six received high quality scores, indicating strong reliability of these findings.

3.9. Meta-aggregation

From the two qualitative studies included in the review, eight findings were extracted based on eight direct quotes from caregivers. Once extracted, these eight findings were aggregated into four common themes which appeared when analysing the two qualitative studies side by side. Themes were generated by the authors of this review through the meta-aggregation process. These themes included: (1) All being in the same boat; (2) Opening up; (3) Finding mindfulness enjoyable; and (4) Mixing with others as a highlight of the intervention. These four themes were then merged to produce two synthesised findings: (1) Group ACT interventions facilitated a sense of all being in the same boat which allowed parents to open-up (Table 5). (2) Group mindfulness and discussions with peers support emotional coping as they relax parents and provide them with hope. The first synthesised finding derived from the two themes which focused on how the caregivers felt throughout the ACT intervention. Participants first described an awareness of being around others with relatable experiences throughout the intervention process, and then went on to describe how this non-judgemental atmosphere allowed them to open-up and speak about topics they would usually avoid. The second synthesised finding was derived from the other two themes which were similar with regards to the aspects of the intervention caregivers found the most useful in improving their emotional coping. Both studies included positive feedback on using mindfulness techniques, with participants in one of the studies describing them as relaxing (Reid et al., 2016), and participants in the other study stating that they liked the meditation part of the intervention as it enabled them to keep calm when their child was having a tantrum (Thompson-Janes et al., 2016). Parents in both studies then went on to describe how they also found the social aspect of the intervention useful, with parents from one study describing being inspired by others (Reid et al., 2016), while parents in the other qualitative study described receiving a good mix of feedback from other parents

**Table 4**  
Overview of studies included in narrative synthesis.

Study	Design	Format of intervention	Was the intervention tailored?	Treatment Length	Study quality score	Control	Analysis	Outcomes (measures)	Outcome group	Effects of ACT pre to post-intervention	Effects of ACT pre-intervention to follow-up	Effects of ACT post-intervention to follow-up	Effects of ACT across all time points
Blackledge & Hayes. (2006)	Pre-post	Group intervention	No	2 × 7 hour sessions	8/9	N/A	Wilcoxon signed rank tests examining the effects of ACT pre to post-intervention and pre intervention to follow-up.	Beck Depression Inventory–II (BDI-II) Global Severity Index of Brief Symptom Inventory (GSI) General Health Questionnaire-12 (GHQ-12) Acceptance and Action Questionnaire-9-item version (AAQ)	Mood Mood General wellbeing Psychological flexibility	Significant (p value not reported) Significant (p - 0.021) value not reported) Significant (p - 0.048) Significant (p value not reported) Non-significant (p value not reported)	Significant (p - 0.006) Significant (p - 0.021) Significant (p - 0.048) Significant (p - 0.043)	Not analysed Not analysed Not analysed	Not analysed Not analysed Not analysed Not analysed
Fung et al. (2018)	Pre-post	Group intervention	The ACT group format and content was based on previous work adapting ACT for clinical and non-clinical populations, including individuals with chronic pain, depression, anxiety, HIV, and mental health stigma.	Not reported	8/9	N/A	Within-subjects repeated measures (ANOVA) examining effects of ACT across the three time points.	Acceptance and Action Questionnaire Version II (AAQ-II) Cognitive Fusion Questionnaire (CFQ)	Psychological flexibility Cognitive fusion	Significant (p - 0.001) Significant (p - 0.01)	Significant (p - <0.001) Significant (p - 0.04)	Non-significant (p value not reported) Significant (p - 0.04)	Significant (p - <0.001) Significant (p - 0.001)
Lunsky et al. (2018)	Pre-post	Group intervention	The ACT group format and content was based on previous work adapting ACT for clinical and non-clinical populations, including individuals with chronic pain, depression, anxiety, HIV, and mental health stigma (Lunsky et al., 2018 & Fung et al., 2018 used exactly the same ACT protocol)	Not reported	8/9	N/A	Within-subjects repeated measures (ANOVA) examining effects of ACT across the three time points.	Depression Anxiety Stress Scale-21 (DASS-21) Parenting Stress Index 4th edition (PSI-4) Health subscale	Mood Mood	2/2 Significant Stress (p - 0.001) Depression (p - <0.029) Significant (p - 0.004)	2/2 Significant Stress (p - <0.001) Depression (p - 0.001) Significant (p - 0.005)	2/2 non-significant Stress (p - 0.669) Depression (p - <0.550) Non-significant (p - 1.00)	2/2 Significant Stress (p - <0.001) Depression (p - <0.001) Significant (p - 0.001)
Pennefather et al (2018)	Pre-post	Group intervention	No	3 × 1.5 h sessions	7/9	N/A	Paired Sample t-tests examining	Parental Stress Scale (PSS)	Mood Mood	Small effect (d - 0.45)	No Follow-up No Follow-up	No Follow-up No Follow-up	

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Table 4 (continued)

Study	Design	Format of intervention	Was the intervention tailored?	Treatment Length	Study quality score	Control	Analysis	Outcomes (measures)	Outcome group	Effects of ACT pre to post-intervention	Effects of ACT pre-intervention to follow-up	Effects of ACT post-intervention to follow-up	Effects of ACT across all time points
							effects of ACT pre to post-intervention.	Daily Coping Inventory (DCI)		Medium effect (d = 0.53)			No Follow-up No Follow-up Significant (p < 0.001)
Burke et al. (2014)	Pre-post	Group intervention	No	4 × 1.5 h sessions	8/9	N/A	Repeated measures ANOVA examining effects of act across all time points, pre to post-intervention, and post-intervention – follow-up.	PTSD Checklist-Civilian Version Parental Psychological Flexibility Questionnaire (PPFQ) Mindfulness (Mindfulness Attention Awareness Scale) (MAAS)	Mood Psychological Flexibility Mindfulness	Large effect (d = 0.92) Medium effect (d = 0.66) Medium effect (d = 0.54)	Not analysed Not analysed Not analysed	Small effect (d = 0.47) Small effect (d = 0.48) Medium effect (d = 0.78)	Significant (p < 0.001) Significant (p < 0.001) Significant (p = 0.009)
Poddar et al. (2015)	Pre-post	Group intervention	No	Not reported	7/9	N/A	Non-parametric Wilcoxon signed rank coefficient examining the effects of ACT group pre to post-intervention.	General Health Questionnaire (GHQ) Acceptance and Action Questionnaire Version II (AAQ-II) World Health Organisation Quality of Life Assessment – BREF (WHOQOL-BREF)	General wellbeing Psychological flexibility General wellbeing	Significant (p = 0.005) Significant (p = 0.005) Significant (p = 0.08)	No Follow-up No Follow-up No Follow-up	No Follow-up No Follow-up No Follow-up	No Follow-up No Follow-up No Follow-up
Rayner et al. (2016)	Pre-post	Group intervention	Take A Breath™ (TAB) is an early intervention group program tailored to provide parents with skills to manage the psychological challenges presented by their child's illness, and designed to prevent more serious long-term mental health difficulties	5 × 2 hour sessions	8/9	N/A	Multilevel mixed-effects linear regression examining the effects of ACT across all time points.	Depression Anxiety Stress Scale (DASS) Parental Psychological Flexibility Questionnaire (PPFQ) Posttraumatic Stress Disorder Checklist-Specific (PTSD checklist)	Mood Psychological flexibility Mood	Not analysed Not analysed Not analysed	Not analysed Not analysed Not analysed	Not analysed Not analysed Not analysed	3/3 non-significant Depression (p = 0.463) Anxiety (p = 0.562) Stress (p = 0.486) Total (no p value reported) Significant (p = 0.030) Non-significant (p = 0.0)
Wallace et al. (2016)	Pre-post	Group intervention	Drawing on a variety of published and unpublished resources, sessions were developed to utilize the ACT	4 × 1.5 h sessions	8/9	N/A	Hierarchical linear modelling examining the effects of ACT pre to post-intervention and	Parent Psychological Flexibility Questionnaire (PPFQ)	Psychological flexibility	Significant (p = <0.01)	Not analysed	Significant (p = <0.001)	Not analysed

(continued on next page)



Table 4 (continued)

Study	Design	Format of intervention	Was the intervention tailored?	Treatment Length	Study quality score	Control	Analysis	Outcomes (measures)	Outcome group	Effects of ACT pre to post-intervention	Effects of ACT pre-intervention to follow-up	Effects of ACT post-intervention to follow-up	Effects of ACT across all time points
			“Hexaflex” model to target psychological flexibility by helping parents recognize areas in which they may be stuck, and to develop strategies to pursue values-based action.				post-intervention – follow-up.						
Weiss et al. (2019)	Pre-post	Group intervention	The leaders of parent groups followed a general schedule of topics to be covered and did not use a treatment manual. This decision was made to allow flexibility to tailor treatment to address unique concerns most salient to parents at that time.	9 × 2 hour sessions	8/9	N/A	Multilevel modelling (MLM) analyses examining effects of ACT pre to post intervention, pre-intervention to follow-up, and across all three time points.	Center for Epidemiological Studies-Depression for Adults (CES-D). The Parent Psychological Flexibility Questionnaire (PPFQ).	Mood Psychological flexibility	Significant (p - <0.001) Significant (p - <0.001)	Significant (p - 0.017) Significant (p - <0.001)	Not analysed Not analysed	Significant (p - 0.004) Significant (p - <0.001)
Kowalkowski (2012)	RCT.	Group intervention	No	8 × 1.5 h sessions	5/13	Inactive	Repeated measures ANOVA examining the effects of ACT pre to post-intervention and pre-intervention to follow-up.	Brief Symptom Inventory-18 (BSI-18) Parental Stress Index-Short Form (PSI-SF) Acceptance and Action Questionnaire-II (AAQ-II) Five Facet Mindfulness Questionnaire (FFMQ).	Mood Mood Psychological flexibility Mindfulness	Significant (p = 0.05) Non-significant (p - 1.00) Not analysed Not analysed	Non-significant (p - 0.109) Significant (p - 0.017) Not analysed Insignificant	Not analysed Not analysed Not analysed Not analysed	Not analysed Not analysed Not analysed Not analysed
Hahs et al. (2019)	RCT.	Group intervention	No	2 × 2 hour sessions	7/13	Inactive	One-tailed unpaired t-test of change scores examined differences in improvements between groups from pre to post-intervention.	Frieberg Mindfulness Inventory (FMI) Mindfulness Attention Awareness Scale (MAAS)	Mindfulness Mindfulness	Insignificant (p - 0.25) Significant (p - <0.01)	No follow-up No follow-up	No follow-up No follow-up	No follow-up No follow-up
Sairanen et al (2019)	RCT.	Group intervention	Acceptance-exercises focusing	Not reported	9/13	Inactive	Hierarchical linear modelling	Five Facet Mindfulness	Mindfulness	Significant (p - 0.015)	not analysed		Significant (p - 0.002)

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Table 4 (continued)

Study	Design	Format of intervention	Was the intervention tailored?	Treatment Length	Study quality score	Control	Analysis	Outcomes (measures)	Outcome group	Effects of ACT pre to post-intervention	Effects of ACT pre-intervention to follow-up	Effects of ACT post-intervention to follow-up	Effects of ACT across all time points
			on one's relationship with self were included, since recent studies have supported this as useful for parents of children with chronic conditions.				(HLM) (Wald test) examined differences in improvements between groups across time points (Time × Group interaction).	Questionnaire (FFMQ)				Non-significant (p = 0.180)	
Chong et al. (2019)	RCT.	Group intervention	ACT protocol was modified based on ACT training manuals used in previous studies, which led to positive effects for caregivers of children with chronic conditions,	Not reported	13/13	Asthma education talk plus 3 telephone follow-ups	Generalised estimating equations examined differences between groups at post-intervention and follow-up, as well as differences in improvements between groups across time points.	Pediatric Asthma Caregiver's Quality of Life Questionnaire (PACQOL)	General wellbeing	Not analysed	Not analysed	Not analysed	Significant (p = 0.001)

**Table 5**  
Overview of studies included in meta-aggregation.

Study	Design	Format of intervention	Was the intervention tailored?	Treatment Length	Study quality score	Analysis	Results
Reid et al. (2016)	Phenomenological	Group intervention	No	2 × 4 hour sessions	7/10	Thematic analysis	Participants reported that they found the Parent Well-Being Workshops useful, and they felt that they were better able to cope with stress following attendance at the workshops. In addition, they described new perceptions of their difficulties and felt that they were able to incorporate mindfulness into their daily lives with positive effects on both their own well-being and on others around them.
Thompson-janes et al. (2016)	Phenomenological	Group intervention	No	Not reported	4/10	Thematic analysis	Four main themes were identified: (i) parent’s pre-group narratives, (ii) barriers and solutions, (iii) positive aspects of Confident Parenting and (iv) positive outcomes of Confident Parenting. The themes highlighted how beneficial parents found Confident Parenting as well as raised helpful ideas about how to engage families where there are practical and personal barriers to attending.

(Thompson-Janes et al., 2016). Supporting quotes for both synthesised findings are shown in Table 6.

**Table 6**  
Supporting quotes for synthesised findings.

Themes	Supporting Quotes	Synthesised Findings
Opening-up	<p>“We could actually talk about the fact that some of our kids hit and punch ... we usually can’t talk about [these things] anywhere else”</p> <p>“When we understand how everybody tends to just feel and share things people start to open-up and just opening up and expressing your feelings and your difficulties is a relief in a way”</p>	Group ACT interventions facilitated a sense of all being in the same boat which allowed parents to open-up.
All being in the same boat	<p>“Just absolutely fantastic, knowing that you’re not the only people in the same boat, struggling with a child with behaviour problems”</p> <p>“You meet these people and you realise that ‘right they are also in the same boat”</p>	
Found mindfulness particularly useful and relaxing	<p>“I was a bit cynical about it (mindfulness) but it really did work”</p> <p>“I liked the meditation bit; I reckon it’s helped me to keep more calm when my son’s having a tantrum, I think he’s noticed as well ‘cos he seems to calm down quicker than normal”</p>	Group mindfulness and discussions with peers support emotional coping as they relax parents and provide them with hope.
Mixing with others as a highlight of the intervention	<p>“Hearing stories that they told and the strength and they’ve never given up”</p> <p>“I’ve found it a good way just to discuss a problem ... and generally you’ve got a mix of feedback from the experts or from the families”</p>	

## 4. Discussion

### 4.1. Summary of findings – primary outcomes

This was the first convergent segregated MMSR that included meta-analysis of the effects of ACT interventions on the primary outcomes and ACT processes of caregivers of children with chronic conditions. However, it is worth noting that interventions delivered in group-based formats met the inclusion/exclusion criteria, so results of the meta-analysis do not generalise beyond these types of intervention. Results of the meta-analysis showed significant effects of ACT on caregiver mood outcomes at post-intervention, follow-up, and both time points combined. Despite having slightly different study inclusion/exclusion criteria, this finding is commensurate with those of an earlier meta-analysis by Han et al. (2021). In addition to the results of our meta-analysis, the narrative synthesis of quantitative findings also revealed that most studies found significant or large effects of ACT on mood. These findings are again in line with previous narrative syntheses (Byrne et al., 2020; Jin et al., 2021) which also reported significant improvements on measures of moods including stress and depression following ACT. In line with Han et al. (2021), the current meta-analysis also found an overall effect of ACT interventions on parenting confidence in family caregivers at both time-points combined. This was the first narrative synthesis on the efficacy of ACT on general wellbeing, and it showed that ACT was associated with improved QoL and general health of caregivers. Earlier work has suggested a large proportion of caregivers typically engage in avoidance-based coping, which has been shown to be associated with greater general stress among caregivers of children with epilepsy (Rodenburg et al., 2007). The positive impact of ACT may be due to the focus on engaging with the understandable challenging emotions that come with care-giving – one based in the openness, awareness, and engagement implicit in PF. These findings also support trial data regarding the impacts of ACT in different health contexts (Gloster et al., 2020) as ACT was shown to impact all three primary health outcomes. Therefore, it could be therapeutically helpful if clinicians were to deliver ACT to caregivers struggling with poor general wellbeing and low confidence in their abilities to manage their child’s chronic condition, rather than the intervention being exclusively applied to those displaying mood challenges such as distress, depression, and burnout.

### 4.2. Summary of findings - ACT process measures

PF can be broken down into many different sub-processes and there

exist many questionnaire measures of these processes. Based on the process measures included in the review, we were able to review the effects of ACT on three processes. First, overarching PF, then cognitive fusion, and mindfulness. Meta-analysis revealed large significant effects of ACT on PF at post-intervention, follow-up, and both time points combined. Large significant effects of ACT on cognitive fusion at follow-up were also identified. These findings added to the existing literature by showing that two delineated treatment targets of ACT improve as a result of the intervention, emphasising the role of the hypothesised therapeutic processes of ACT. Further, observational studies show correlations between such ACT processes and various other health outcomes in caregivers of children with various disabilities, with PF negatively associated with psychological symptoms, caregiver burden, and stress (Evans et al., 2012; Lloyd & Hastings, 2008; Weiss et al., 2012), and cognitive fusion positively associated with psychological distress (Bardeen & Fergus, 2016). However, it is important to note that the current research did not test mediational processes and therefore cannot add to or support existing research findings regarding the effects of ACT processes on other health outcomes. Narrative synthesis then revealed significant effects of ACT on mindfulness across all three time-points. Existing research suggests that these kind of increases to mindfulness can cause reductions to caregiver stress and preoccupation (Baer, 2003; Hayes et al., 2006). The same literature also revealed that this link exists because mindfulness encourages caregivers to focus more on the opportunities associated with the present moment, leading to less emotional disturbance. One limitation of our analysis of ACT processes was that we didn't measure all processes posited to contribute to PF because of the narrow range of process measures used in included studies. We were therefore unable to comment on whether interventions worked via processes of values, committed action, experiential avoidance, or self-as-context. There are existing measures for such processes (e.g., CompACT; Francis et al., 2016; Self-as-Context Scale; Zettle et al., 2018; Committed Action Questionnaire; Bailey et al., 2016) and future trials should consider including such measures to examine whether improvements following ACT are carried to these other aspects of PF.

#### 4.3. Summary of findings - Caregiver experiences of ACT

Another novel aspect of this review was the inclusion of a qualitative synthesis to explore perceived acceptability of ACT among caregivers of children living with a chronic condition. It was hoped that this would help to establish which aspects of ACT they find enjoyable/useful, so that future ACT interventions could be tailored around their previous experiences of the intervention. However, just two studies conducted exclusively with parent caregivers were returned from our search and thus included in this review, suggesting that we are missing valuable information about ACT interventions in this context – going beyond questions of efficacy into implementation. Although this can be seen as a limitation of the current review, it does identify a gap within the existing literature as it highlights a need for more qualitative research on the acceptability of ACT among caregivers of children with chronic conditions. Nonetheless, from these two studies we were still able to identify two clear and relevant themes. The first theme was that group ACT interventions facilitated a sense of all being in the same boat, which allowed parents to open-up. Participants described taking strength from the example of other parents who have adjusted through similar circumstances. This finding not only speaks to ACT as an acceptable intervention for parents of children with chronic conditions, but also highlights interesting implications for clinical practice, as it suggests some benefits of utilising group-based interventions and making them more readily available to parents in clinical settings. Previous research in this area investigated the influence of a parent-to-parent peer support scheme on parents whose children have a disability or additional need. Results indicate that parent-to-parent peer support has a significant positive influence on parents' levels of psychological distress and their ability to cope, as it was found to open-up opportunities for continued

growth and personal fulfilment (Bray et al., 2017). These findings again suggest that parents can learn from each other's experiences as they open-up and share personal stories in a safe space. It is also worth noting that delivering ACT to larger groups is perhaps less expensive than delivering the intervention individually, which is becoming increasingly important as health services across the world are currently operating within a challenging climate. Qualitative synthesis also identified mindfulness techniques as a particularly useful aspect of the intervention. This theme could have clinical implications as it suggests that clinicians should tailor ACT interventions to the needs of parents through allocating more time to practicing mindfulness.

#### 4.4. Summary of findings - Risk of Bias

No evidence of publication bias or p-hacking was detected through p-curve analysis. This finding increases the dependability of results as it suggests that selective reporting did not impact the findings of the review.

#### 4.5. Summary of findings - Intervention heterogeneity

There is heterogeneity among the interventions included in this review. For example, Table 2 highlighted some large differences in the age of participant's children, as some studies applied ACT to caregivers of very young children (Corti et al., 2018; Rayner et al., 2016), while others targeted caregivers of older children and adolescents (Fung et al., 2018; Weiss et al., 2019). Treatment lengths ranging from four (Hahs et al., 2019) to 18 (Corti et al., 2018) hours were applied across the 14 studies that reported this information. Further, while some interventions were tailored specifically for caregivers of children with chronic conditions (Duncan, Coatsworth, & Greenberg, 2009; Sairanen et al., 2019), others were not and instead applied ACT interventions that have been previously delivered to other populations (Burke et al., 2014; Pennefather et al., 2018). This diversity makes it challenging to synthesise findings and indicates that it may be inappropriate to derive an estimate of overall effect sizes from this set of studies. However, it is important to note that despite these differences between studies, clinical heterogeneity was lower in this review than in previous SR's in this area (Jin et al., 2021), due to some similarities in participant and intervention characteristics. For example, all included studies delivered ACT in group format, despite interventions not being excluded on the basis of their configuration. Table 2 then shows that of the 18 studies that reported caregiver gender, 94% applied ACT to a sample consisting predominantly of mothers. Further, a large proportion of included studies also targeted caregivers of children with the same condition, as 47% of studies were conducted exclusively with caregivers of children with autism. Finally, of the 8 studies that reported caregiver ethnicity, 75% were conducted with a sample consisting predominantly of Caucasians. However, while these similarities between studies are generally seen as a strength of the current review, they do make it difficult to generalise findings across different caregiver populations and point to the importance of reaching out to more diverse participant groups (e.g., fathers, caregivers of non-Caucasian ethnicity).

#### 4.6. State of the evidence

ACT interventions have been used to help caregivers support children diagnosed with a variety of chronic conditions. This application to a wide range of conditions demonstrates the flexibility of the ACT model and reflects the extent to which practitioners working with long-term conditions have embraced ACT (Thewes et al., 2014). However, despite the transdiagnostic applicability of ACT, a notably high number of studies in the current review focused solely on caregivers of children with autism spectrum disorder (ASD), as 42% of studies were conducted with this population. This high percentage of studies focusing on ASD may reflect the research interests of key researchers in the area, a high

clinical need of caregivers of children with ASD, or perhaps greater clinician awareness of the condition. ACT is also transdiagnostic, so we expect effects to be similar across conditions and target a similar range of behavioural and emotional processes that emerge from living in a challenging context. However, the fact remains that the research has been predominantly focused on a select few conditions. Thus, little is known about the application of ACT for caregivers of children with other common chronic conditions such as diabetes, obesity, and cystic fibrosis, leaving a gap within the literature. ACT has also been shown to elicit change in a wide range of health outcomes (Byrne et al., 2020; Jin et al., 2021). However, in the current review, 63% of the research focused on addressing moods such as depression and psychological distress, while only 16% assessed the effects of ACT on general wellbeing outcomes. Moreover, despite a large percentage of the research focusing on mood, none of the studies included a direct measure of anxiety (e.g., Beck Anxiety Inventory). It should also be noted that while most of the included studies applied ACT as the sole intervention, some involved other interventions being combined with ACT. For example, two studies (Brown et al., 2015; Whittingham et al., 2016) involved two ACT sessions and nine parent skills training sessions. This makes it difficult to disentangle which components of the intervention are causing improvements to health outcomes and needs to be addressed in future research. It should also be noted that psychological interventions like ACT can be more effective when specifically tailored to the needs of the population to which they will be applied, as Ryan and Lauber (2002) used Cooper's method of literature integration (Cooper, 1989) to evaluate the efficacy of tailored interventions (TIs) compared to standard interventions (SIs), and found that participants preferred TIs to SIs, perceived that the TIs were more personal, and read and remembered more of the information in TIs. However, only 42% of the reviewed ACT interventions in the current research have been specifically tailored for caregivers. Therefore, future research should ensure ACT interventions are tailored to caregivers to confirm whether this increases the impact of the treatment. However, only 11% of studies in the current review addressed caregiver experiences of ACT. This made establishing the acceptability/understanding the implementation of ACT very difficult as the findings from only two studies produced a tentative body of evidence. There is therefore a need for more qualitative research highlighting which parts of ACT interventions caregivers find acceptable and useful. This is an important issue as psychological interventions require that participants engage, that they like the intervention, and that they practice techniques at home to get the most from their treatment. Therefore, if we know what participants find enjoyable/useful, then we can use this to inform future treatment protocols and tailor the intervention for caregivers of children with chronic conditions. With regards to the methods adopted by the included studies, it is noticeable that just 11% of studies compared ACT to other active treatments. In comparison, 32% of included studies compared ACT to inactive controls. While this allows for an absolute comparison between an ACT intervention and no intervention, it does not allow researchers to conclude that ACT specifically caused improvements to caregiver health outcomes, as the confounding effects, such as expectancy were not controlled. Indeed, it is also possible that this social interaction was what caused improvements to caregiver health outcomes, and that a group intervention alone, without ACT, may have resulted in similar findings. There were also no studies that carried out follow-up assessments of longer than six months, and 37% of studies carried out no follow-up assessment at all. This makes it difficult to tell if ACT interventions are a long-term solution, or if the effects of ACT would reduce over a longer period. Some of these methodological issues of ACT trials have been cited in other reviews (Hofmann and Asmundson, 2008; Öst, 2014), which further highlights the need for more high-quality ACT trials to be carried out for caregiver populations (Graham et al., 2016).

#### 4.7. Recommendations for future research

The bullet points below are recommendations on how to address some of the gaps within the literature through changes to future research methodologies. Fortunately, the theoretical groundwork has been laid out for these issues, making implementing these suggestions easier in future research.

- Future research should examine the impacts of ACT on caregivers of children with a range of chronic conditions to enable us to ascertain whether ACT effects are consistent across caregivers of children with different chronic conditions which have been neglected in the existing research such as diabetes and cystic fibrosis.
- Future research should examine the influence of ACT on a greater range of health outcomes to enable us to ascertain whether ACT effects are consistent across different outcomes which have been neglected in the existing research such as general wellbeing and anxiety.
- Future trials should attempt to examine the effects of ACT without the confounding effects of added interventions, and/or try to understand how ACT specific aspects interact with other treatment components to change outcomes. To this end, fractional factorial trial designs that enable the isolation of combined effects when applying multiple intervention techniques and retain those that appear to be beneficial for further testing could be used in future studies.
- Future research should ensure that any ACT interventions delivered to caregivers are tailored to their needs to enable us to ascertain whether this has an impact on how effective the intervention is on the health outcomes of this population.
- Future research should use qualitative methods to share learning on what engages/disengages participants and intervention implementation.
- To allow greater understanding of the treatment processes in ACT a broader range of measures of PF sub-processes (e.g., values, self-as-context etc.) should be included in future trials.
- Future research should include active control groups which are clearly described and matched in length to enable us to control for demand characteristics and other confounding factors.
- Future research should include longer term follow-up assessments to enable us to see how long treatment effects persist and their pattern over time.
- Future research should include more diverse samples of participants by recruiting fathers and reaching out to a range of ethnicities.

#### 4.8. Limitations of the current review

Several limitations of the present review should be considered when interpreting the findings. First, as is common in SR's (Graham et al., 2016), we did not exclude studies based on treatment fidelity to the ACT model. Instead, studies were automatically included if they mentioned Acceptance and Commitment Therapy/Training as the intervention under study in the manuscript and referenced an existing ACT treatment protocol or stated that the applied intervention was guided by the core processes/principles of ACT. This decision was made because many of the studies investigating the efficacy of ACT on caregivers of children with chronic conditions didn't specify exactly which ACT processes had been applied during intervention, leading the research team to look for other criteria upon which to determine whether a full ACT treatment protocol had been used. Nonetheless, this remains a limitation of the current research because we did not check and therefore cannot know if ACT was delivered consistently and reliably in all the trials. There are ACT fidelity measurement tools available to help avoid this methodological shortcoming in future research (O'Neill et al., 2019).

Despite contributing to the study quality scores, the validity of the measurement instruments used in each study (i.e., CFQ, AAQ-II, PSI-4-

SF) also wasn't considered during formal screening. As a result, studies were not excluded if the measurement instruments they used were of low validity. For example, the validity of the AAQ-II is increasingly being challenged (Rocheffort et al., 2018; Tyndall et al., 2019), but multiple studies using the AAQ-II to measure PF were included in the review. Thus, it is unclear whether the synthesised findings presented in the current review are reliable.

As shown in Table 2, the current review included studies conducted with caregivers of children of a broad range of ages covering multiple developmental periods (e.g., birth–5 years, 6–11 years, and 12–17 years). Just as adult and child caregiving are meaningfully different, so is caring for children of different ages. For example, infants and toddlers have various age-related characteristics that make the management of their diabetes distinct from that of older children (Cody., 2007). This broad age range and the absence of sub-group analysis in the current research therefore make it impossible to tell if ACT has more/less of an impact on caregivers based on the age of their child, reducing the reliability of findings. Moreover, this SR included studies that applied ACT to caregivers of children with any chronic condition. Again, the type of chronic condition the child has could impact on the reliability of results due to distinctions in the management of each condition, despite ACT claiming to be transdiagnostic. It would therefore be useful for future SR's to pre-plan subgroup analysis to examine the effects of ACT on parent functioning by age/developmental periods and/or by chronic conditions, as this was beyond the scope of the current review.

The exclusion of case studies may also be seen as a limitation, as case studies provide clinically useful descriptive accounts and allow further insight into a range of applications, giving clues to clinically useful techniques and participant experiences that can help us improve future treatment protocols.

Moreover, only studies published in peer-reviewed journals were included in this review. This was to enable greater comparability among studies during quality assessment and extraction. We also hoped it would mean a higher level of quality among the included studies. However, we do acknowledge that we may have missed some relevant research and increased the risk of bias because of our decision to exclude grey literature. Researchers may therefore want to include grey literature to help avoid these.

#### 4.8.1. Issues in future

This potential risk of bias presents a further limitation, as funnel plots were unable to be used to test for publication bias as originally intended due to a low number of the included studies being eligible for meta-analysis (Higgins et al., 2019). Further, there was no attempt made by the research team to reach out to other researchers in the field to request their findings on publication bias. This meant that p-curve analysis was the only test of publication bias used throughout this review. A limitation of this method is that it will often fail to detect studies that lack evidential value (Simonsohn et al., 2014). Therefore, if a set of findings combine true effects with non-existent ones, p-curve analysis will usually not detect the latter. Authors of future studies should therefore reach out to other researchers to request their findings on publication bias and/or use other more robust tests alongside p-curve analysis to increase the dependability of results and ensure that any existing publication bias within the literature is detected.

Moreover, in common with an existing SR in the area (Graham et al., 2016), we only used Psychinfo, OVID Medline, and EMBASE during the search process as these are high quality large databases. Although we are confident that these databases captured the relevant literature due to an extensive search process, other high-quality databases such as CINHAL and Web of Science could have also been used.

There were some issues with inter-rater agreement on the JBI quality assessment checklists used during the study, as there were some disagreements between reviewers on certain items included on the checklists. To facilitate inter-rater reliability in future, the reviewers should come to an agreement on aspects of appraisal criteria wording needing

clarification prior to quality assessment.

It was also noticed during critical appraisal that the quality assessment checklists were quite broad and left out relevant questions which may have exposed other limitations within the included literature. This may have led to multiple studies achieving high quality scores despite containing numerous methodological shortcomings. The JBI critical appraisal checklists therefore need to be tested further in a larger scale study to assess their construct validity and inter-rater reliability. In the meantime, future reviews should use more robust evaluative tools such as the Cochrane collaborations tool for assessing risk of bias (Higgins et al., 2011), to ensure that the quality of included studies is accurately represented. It should also be noted that this review only included studies published in English peer-reviewed journals, meaning several studies meeting the search criteria were excluded purely due to language barriers. The decision to exclude non-English studies was a pragmatic one – based on the time-consuming nature of detailed, accurate translation necessitated for the qualitative synthesis. One of the main issues with excluding non-English studies is that it may impact the pattern of findings, as a review of 50 meta-analyses found that including non-English studies influenced effect estimates in more than half of the meta-analyses (Jüni et al., 2002). In five of these cases, estimates became more positive, and in 16, they became less positive, suggesting that English language studies may tend to only report findings when they are statistically significant. Future research could therefore use professional translation services or translation tools (e.g., Google Translate) to translate documents written in other languages into English, as the inclusion of these non-English studies may broaden the generalisability of findings and result in more thorough reviews that are more representative of the current literature.

The presence of clinical heterogeneity is a further limitation of this research, as notable differences in the age of children being cared for and the length/content of the intervention being delivered to caregivers were revealed across included studies. Further, the presence of clinical heterogeneity may have contributed to the high levels of statistical heterogeneity between studies, which prevented the use of meta-analysis on the general wellbeing and mindfulness outcome groups. Given the rapid increase in intervention studies with caregivers of children with chronic conditions, it may soon be possible to conduct a meta-analysis on the effects of ACT on these health outcomes. However, future reviews should also aim to stratify for similar characteristics of the intervention and participants among included studies, to reduce heterogeneity and increase the chances of deriving an estimate of overall effect through meta-analysis.

## 5. Conclusion

This was the first convergent segregated MMSR with meta-analysis of the effects of ACT on the primary outcomes and ACT processes of caregivers of children with chronic conditions. However, it should be noted when looking at the findings that all 19 included studies delivered ACT in groups. Meta-analysis revealed significant effects of group ACT interventions on two out of three primary outcomes (mood & parenting confidence) and two out of three ACT processes (PF & cognitive fusion). Narrative synthesis also then suggested promising impacts of group ACT interventions on two out of three primary outcomes (mood & Parenting confidence), as well as all three ACT processes. Qualitative findings indicate that parents enjoy interacting with peers during group ACT interventions and that group mindfulness may be particularly useful. In conclusion, group ACT interventions represent an efficacious intervention to address the high prevalence of distress and low QOL in caregivers of children with chronic conditions and should be considered in clinical practice. However, a high percentage of studies included in this review were conducted with a sample consisting predominantly of mothers, and Caucasian groups. Further research looking at a wider range of intervention formats and genders and ethnicities is therefore needed in order to be confident that treatment effects generalise across settings, as

implied by the ACT model. It would also be useful to examine the effects of ACT on parent functioning by age/developmental periods (e.g., birth–5 years old, 6–11 years old, and 12–17 years old) and by specific childhood condition, as this was beyond the scope of the current research.

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**Contributors**

Authors A and E designed the study and wrote the review protocol. Author A conducted literature searches and provided summaries of previous research studies. Authors A, B, C, and E carried out formal

screening of study titles/abstracts and selected studies for inclusion. Authors A and D carried out quality assessment of the included studies. Author A carried out statistical analysis and wrote the first draft of the manuscript, and all other authors contributed to and have approved the final manuscript.

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**Declaration of competing interest**

Given their role as an Editorial Board Member, Graham C.D. had no involvement in the peer-review of this article and had no access to information regarding its peer-review. The other authors declare that they had no conflicts of interest with respect to their authorship or the publication of this article.

**Appendices.**

**Appendix A:** PICO Table

Population	Intervention	Quantitative Outcomes	Qualitative Phenomena of Interest
Parent × Chronic*	“Acceptance and commitment**”	Health*	Experience*
OR Caregiver × Long term*	OR “Acceptance & commitment**”	OR Well-being*	OR Perspective *
OR Carer* Long-term*	OR “Acceptance Based**”	OR “Quality of Life**”	OR View *
OR Guardian × Long standing*	OR “Acceptance-Based**”	OR Flexibility*	Or Perception*
OR Foster* Long-standing*	OR ACT	OR Symptoms*	OR Thought *
OR Mother × Developmental		OR Anxiety*	OR Attitude*
OR Father × Serious*		OR Depression*	
OR Family × Terminal*		OR Distress*	
Child × Illness*		OR Stress*	
OR Daughter × Disease*		OR Acceptance*	
OR Son × Condition*		OR Behaviour*	
OR Adolescent × Pain*		OR Relationship*	
OR Infant × Disability*		OR Adjustment*	
OR “Young people**”		OR Avoidance*	

**Appendix B.** Terms entered during formal search conducted on PsychInfo

	Searches	Results
1	exp “Acceptance and Commitment Therapy”/	1758
2	“Acceptance and commitment” or “Acceptance & commitment” or “Acceptance based” or ACT”	75,330
3	1 or 2	75,330
4	exp Parents/	115,660
5	exp × Parenting/	92,448
6	exp × Parent training/	6134
7	exp × Caregivers/	21,269
8	exp × Foster parents/	789
9	exp × Mothers/	24,907
10	exp × Fathers/	6760
11	Exp × Family/	233,426
12	(Carer × or Guardian*)	14,769
13	4 or 5 or 6 or 7 or 8 or 9 or 10 or 11 or 12	293,730
14	exp Chronically Ill Children/	347
15	exp × Daughters/	1954
16	exp × Sons/	902
17	(“Child” or “Adolescent” or “Infant” or “Young people”)	1096689
18	14 or 15 or 16 or 17	1,097,249
19	3 and 13 and 18	2741

**Appendix C.** JBI checklist for randomized controlled trials

	Yes	No	Unclear	NA
1. Was true randomisation used for assignment of participants to treatment groups?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
2. Was allocation to treatment groups concealed?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
3. Were treatment groups similar at the baseline?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
4. Were participants blind to treatment assignment?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
5. Were those delivering treatment blind to treatment assignment?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
6. Were outcomes assessors blind to treatment assignment?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
7. Were treatment groups treated identically other than the intervention of interest?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
8. Was follow-up complete and if not, were differences between groups in terms of their follow-up adequately described and analysed?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
9. Were participants analysed in the groups to which they were randomized?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
10. Were outcomes measured in the same way for treatment groups?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
11. Were outcomes measured in a reliable way?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
12. Was appropriate statistical analysis used?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
13. Was the trial design appropriate, and any deviations from the standard RCT design (individual randomisation, parallel groups) accounted for in the conduct and analysis of the trial?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

Overall appraisal: Include  Exclude  Seek further info.

Appendix D. JBI checklist for quasi-experimental studies

	Yes	No	Unclear	NA
1. Is it clear in the study what is the 'cause' and what is the 'effect' (i.e. there is no confusion about which variable comes first)?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
2. Were the participants included in any comparisons similar?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
3. Were the participants included in any comparisons receiving similar treatment/care, other than the exposure or intervention of interest?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
4. Was there a control group?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
5. Were there multiple measurements of the outcome both pre and post the intervention/exposure?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
6. Was follow-up complete and if not, were differences between groups in terms of their follow-up adequately described and analysed?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
7. Were the outcomes of participants included in any comparisons measured in the same way?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
8. Were outcomes measured in a reliable way?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
9. Was appropriate statistical analysis used?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

Overall appraisal: Include  Exclude  Seek further info

Appendix E. JBI checklist for qualitative research

	Yes	No	Unclear	Not applicable
1. Is there congruity between the stated philosophical perspective and the research methodology?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
2. Is there congruity between the research methodology and the research question or objectives?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
3. Is there congruity between the research methodology and the methods used to collect data?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
4. Is there congruity between the research methodology and the representation and analysis of data?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
5. Is there congruity between the research methodology and the interpretation of results?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
6. Is there a statement locating the researcher culturally or theoretically?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
7. Is the influence of the researcher on the research, and vice-versa, addressed?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
8. Are participants, and their voices, adequately represented?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
9. Is the research ethical according to current criteria or, for recent studies, and is there evidence of ethical approval by an appropriate body?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
10. Do the conclusions drawn in the research report flow from the analysis, or interpretation, of the data?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

Overall appraisal: Include  Exclude  Seek further info

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