Third Wave Interventions for Adolescents with Mental Health Disorders: A Meta-Analysis

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This report is submitted in partial fulfillment of the degree of Master of Psychology (Clinical)

School of Psychology

The University of Adelaide

October 2023

Word Count: 7,926 words

Table of Contents

Declaration

This dissertation contains no material which has been accepted for the award of any other degree or diploma in any University, and, to the best of my knowledge, contains no materials previously published except where due reference is made.

I give permission for the digital version of my dissertation to be made available on the web, via the University's institutional digital repository, the Library Search and also through web search engines, unless permission has been granted by the School to restrict access for a period of time.

Paul Sandery

October 2023

Statement of Contribution

I was responsible for study design, data collection, analysis and interpretation, and manuscript drafts; JB and DD contributed to study design and assisted with data analysis and interpretation and manuscript revisions.

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Acknowledgements

I would like to acknowledge my two supervisors, JB and DD, who have supported me throughout the course of this project. Their expertise, guidance, and encouragement have been invaluable. Thanks to GG for her work as the second reviewer. Her time spent screening a sample of studies was greatly appreciated. Thanks also to VL, research librarian at the University of Adelaide, for her assistance with devising my search strategy.

Author Note

The author has no conflicts of interest to disclose. Extracted data used in this meta-analysis are available upon reasonable request.

Funding

This research did not receive any specific grant from funding agencies in the public, commercial, or not-for-profit sectors.

This article is intended for submission to the Journal of Contextual Behavioural Science, which adheres to the APA (7th edition) reference style. The article meets journal requirements, with review articles needing to be fewer than 10,000 words.

Abstract

Little is known about the efficacy and effectiveness of third wave interventions for adolescents with mental health disorders. This study reviewed the empirical evidence for third wave interventions (mindfulness-based cognitive therapy [MBCT], mindfulness-based stress reduction [MBSR], acceptance and commitment therapy [ACT], dialectical behaviour therapy [DBT], and compassion focused therapy [CFT]) in clinical adolescent populations. Five databases were systematically searched for controlled and uncontrolled studies that had evaluated MBCT, MBSR, ACT, DBT, or CFT with adolescents with mental health disorders. Risk of bias was assessed using the QualSyst tool and effect sizes (Hedges' *g*) were calculated with associated *p*-values, 95% confidence intervals, and prediction intervals. Twelve studies, comprising a pooled sample of 584 adolescents and all evaluating DBT, were identified. Studies were mostly of sound methodological quality. Significant and large pooled effect sizes were found for all six mental health outcomes (depression, anxiety, emotional regulation, suicidal ideation, self-harm, and borderline personality disorder [BPD] symptoms) at post-intervention. However, variability in treatment response across studies was evident. Effect estimates were also less pronounced in between-group designs, particularly at follow-up. No moderation was observed by DBT intensity or duration. While DBT is effective in reducing psychological symptoms in adolescents with mental health disorders, more rigorous, controlled studies are needed to draw more reliable conclusions about the efficacy of DBT in such populations. Greater research in more diverse adolescent samples and with other third wave interventions (e.g., ACT, MBCT) is also warranted.

Keywords: third wave intervention, dialectical behavior therapy, adolescent, mental health, meta-analysis

Highlights

- DBT has been commonly evaluated in clinical adolescent populations
- DBT is effective in improving a range of mental health outcomes
- High quality RCTs are needed to establish DBT's efficacy with these populations
- More research evaluating other third wave interventions (e.g., ACT, MBCT) is needed

Third Wave Interventions for Adolescents with Mental Health Disorders: A Meta-Analysis

Adolescence is a period of significant change. During this time, individuals undergo rapid physical and emotional development, experience rising academic demands, and must learn to navigate an increasingly complex social sphere, all of which can impact mental health (Blakemore, 2019; Sawyer et al., 2012). Poor mental health during adolescence has been linked to a range of negative outcomes across the life course such as school dropout, social withdrawal, unplanned pregnancy, future mental health problems, unemployment, and poorer general health (Copeland et al., 2015; McLeod et al., 2016; Naicker et al., 2013). Due to the growing number of adolescents experiencing psychological problems across the globe (Hall et al., 2019; Twenge et al., 2019; Vizard et al., 2020), it is critical that greater empirical attention is given to interventions targeting mental health problems among adolescents as this may reduce future negative consequences at both the individual and societal level (e.g., economic productivity, healthcare costs; McDaid et al., 2019; World Health Organization, 2022).

Traditional cognitive behaviour therapy (CBT) is considered the ‘gold standard’ treatment for common psychological disorders in adolescents including depression and anxiety (Andrews et al., 2018; National Institute for Health and Care Excellence, 2019). While CBT has demonstrated effectiveness in improving mental health outcomes in youth, such effects tend to be modest, with recent meta-analyses reporting small to medium effect sizes (Keles & Idsoe, 2018; Oud et al., 2019). These findings highlight the potential limitations of CBT when treating adolescents with mental health issues. Furthermore, over the last 20 years, there has been a shift away from traditional CBT in the field of psychotherapy more broadly, given evidence that cognitive components of CBT provide no additive benefit over behavioural components (Jacobson et al., 1996; Longmore & Worrell,

2007). Support for the cognitive mediation hypothesis in CBT (i.e., cognitive changes preceding symptom changes) has also come under question (Longmore & Worrell, 2007). Additionally, research has shown that CBT's effectiveness may be declining over time (Johnsen & Friborg, 2015). As a result, psychology as a discipline has increasingly looked to other therapeutic approaches, such as third wave interventions, to overcome some of the shortfalls of traditional CBT.

Third wave cognitive behavioural interventions focus on modifying the *context* and *function* of individuals' internal experiences (e.g., emotions, thoughts, physical sensations), instead of their *form*, *content*, or *frequency* as in traditional CBT approaches (Hayes, 2004). Put another way, third wave therapies aim to alter how individuals relate or respond to their private world by targeting processes such as acceptance, defusion, mindfulness, and commitment to core values (Hayes & Hofmann, 2021). Since Hayes (2004) coined the phrase 'third wave' nearly 20 years ago, numerous interventions have been developed that follow these principles including mindfulness-based cognitive therapy (MBCT; Segal et al., 2002), mindfulness-based stress reduction (MBSR; Kabat-Zinn, 1990), dialectical behaviour therapy (DBT; Linehan, 1993a), acceptance and commitment therapy (ACT; Hayes et al., 1999), and compassion focused therapy (CFT; Gilbert, 2010).

Despite third wave interventions having common underlying features, particular therapies may employ distinct techniques. For example, through the incorporation of various mindfulness practices (e.g., meditation, yoga, body scans), mindfulness-based interventions (MBIs) aim to increase awareness of and focus on the present moment, thereby helping individuals respond to experiences in a more open, accepting, and non-judgemental manner (Kabat-Zinn, 2003). As a comparison, DBT is based on principles of dialectical philosophy, emphasising the acceptance and validation of individuals' experiences, while simultaneously promoting personal growth and behaviour change (Chapman, 2006). To assist individuals in

finding a balance between acceptance and change, individuals are taught skills in the following four domains: mindfulness, distress tolerance, emotion regulation, and interpersonal effectiveness (Linehan, 1993b). CFT addresses the shame and self-criticism that underpin many psychological difficulties, striving to develop individuals' sense of self-compassion through exercises such as compassionate letter writing, compassionate imagery, and chair work (Gilbert, 2014). Finally, ACT focuses on the cultivation of psychological flexibility, which is the ability to accept the presence of unpleasant thoughts and emotions, be fully engaged in the present moment, and take effective action in line with one's deepest values (Hayes & Hofmann, 2021). The emphasis that ACT places on individuals identifying, clarifying, and moving towards their values via the creation of specific behavioural goals is a key feature that distinguishes it from other third wave interventions.

Researchers have shown increasing interest in third wave therapies in both clinical (e.g., McCauley et al., 2018) and non-clinical settings (e.g., Johnson et al., 2016), as well as across diverse clinical presentations including depression (e.g., Hayes et al., 2011), social anxiety (e.g., Ebrahiminejad et al., 2016), eating disorders (e.g., Jaite et al., 2020), and borderline personality symptomatology (e.g., Hiller & Hughes, 2022). The evidence for third wave interventions in younger populations is accumulating, with several systematic and meta-analytic reviews being published in recent years. For instance, Chi et al.'s (2018) meta-analysis of 18 studies determined that MBSR had moderate effects in reducing depressive symptoms in young people at post-intervention (Hedges' g = -0.45). Evidence also suggested the duration of MBSR treatment was positively related to its effect on depressive symptoms, particularly during follow-up periods. Similarly, another meta-analysis (k = 14; number of studies) found that ACT was superior to treatment-as-usual and waitlist conditions in improving symptoms of depression and anxiety in children and adolescents, although it did not outperform traditional CBT (Fang & Ding, 2020). Another review by Dunning and

colleagues (2019) explored the efficacy of MBIs, finding that in studies using gold standard research designs (i.e., randomised controlled trials [RCTs] with active control groups), MBIs significantly improved youths' mindfulness (Cohen's $d = 0.42$), depression ($d = 0.47$), and anxiety/stress ($d = 0.18$). DBT has also been found to be effective with adolescents, with meta-analytic evidence ($k = 21$) indicating that it is superior to control interventions in decreasing both self-harm and suicidal ideation (Kothgassner et al., 2021).

Although research has demonstrated that third wave interventions can have beneficial impacts on adolescents' mental health broadly, less is known about their effectiveness in clinical populations. Third wave interventions may have less pronounced effects in clinical samples given that psychological symptoms tend to be more severe, intense, and frequent; other physical and mental comorbidities are also likely to be present, which can complicate treatment (Romano et al., 2021; Nibras et al., 2022). Jørgensen and colleagues (2021) examined the efficacy of different psychological therapies (including third wave interventions) for adolescents with full or subthreshold borderline personality disorder (BPD). After reviewing 10 different RCTs, they concluded that no psychological therapy consistently outperformed control interventions on a range of outcomes (e.g., BPD severity, depression, impulsivity). Although there were no reported adverse effects from any of the interventions, attrition rates were high (ranging from 15% to 75%), suggesting that interventions may have not been fully appropriate for adolescents (Jørgensen et al., 2021). The efficacy of third wave interventions has also been explored in the context of eating disorders, although this research has not solely focused on younger populations. Linardon et al.'s (2017) meta-analysis ($k = 27$) found that each of the third wave interventions included (e.g., ACT, DBT, CFT) led to significant improvements in eating disorder symptomology and other associated psychological outcomes (e.g., depression, self-esteem). While third wave interventions were significantly more efficacious than wait-list controls, they were not

significantly different to active comparisons (e.g., CBT). It is important to note, however, this meta-analysis primarily consisted of studies with adult participants; thus, it is unclear whether the same findings can be generalised to adolescent samples.

Recently, Perkins and colleagues (2023) explored the effect of different third wave interventions on psychological outcomes in children and adolescents; however, they only focused on the following interventions: ACT, CFT, MBCT, and metacognitive therapy. Consequently, the effectiveness of other established third wave approaches (e.g., DBT, MBSR) were not examined. In addition, Perkins et al.'s (2023) review aimed to provide a broad overview of third wave intervention research in younger populations across a range of outcomes (e.g., emotional symptoms/internalising problems, interference from emotional or physical difficulties, wellbeing/flourishing), incorporating studies from both clinical and non-clinical settings.

Thus, to the author's knowledge, no systematic review or meta-analysis has previously explored the efficacy and effectiveness of third wave treatments for specific mental health diagnoses (e.g., major depressive disorder, generalised anxiety disorder) in youth. Although third wave approaches are transdiagnostic, there is utility in understanding their efficacy and effectiveness for given diagnostic categories because this may provide a clearer picture of the evidence base in comparison with traditional CBT, where there has been a longstanding focus on diagnostic categories (Hayes & Hofmann, 2021). Such information will allow clinicians to make more informed treatment decisions when treating younger populations, which in turn may lead to better client outcomes.

The current review will examine a range of established third wave interventions (i.e., MBSR, MBCT, ACT, DBT, and CFT), in line with previous reviews of third wave treatments (Linardon et al., 2017; Linardon et al., 2019). Given that younger children and adolescents may have different needs regarding their mental health (García-Carrión et al., 2019; Racine et

al., 2021), this review will solely focus on adolescents. Overall, the aim of this study is to review and quantitatively assess the empirical evidence for third wave interventions for adolescents with mental health disorders.

Methods

This review was conducted in accordance with the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) guidelines (Page et al., 2021; see Appendix B). A review protocol was registered on the Open Science Framework (https://osf.io/69tuh) on June 24th, 2023.

Literature Search

The PubMed, PsycINFO, ProQuest, Embase, and CINAHL databases were searched from database inception to September 9th, 2022 in order to identify studies that focused on third wave interventions in adolescent populations. The search terms and logic grids were developed in consultation with a specialist research librarian (see Table 1). The reference lists of all included studies, as well as related systematic and meta-analytic reviews (Buerger et al., 2021; Fang & Ding, 2020; Perkins et al., 2023), were examined to identify potentially relevant research that may have been missed during the initial database searches. Finally, Scopus citation searching was undertaken for all included studies. Neither process identified any additional, unique records.

Study Eligibility

Study screening was independently undertaken by the author (PS), using Covidence systematic review software (Veritas Health Innovation). To ensure reliability of the screening process, a random subset of 50 articles, generated through a random number generator (CalculatorSoup, n.d.), was screened by a postgraduate psychology student (GG) with previous experience conducting systematic reviews. Inter-rater agreement was excellent (100%).

Given the current study's focus on original, peer-reviewed research, unpublished manuscripts (i.e., theses/dissertations), conference abstracts, review papers, and study

protocols were all ineligible. Studies also needed to be published in English (or available in English translation). The following PICO (Population Intervention Comparison Outcome) criteria were additional requirements:

Population

Studies had to include participants within the age range of 10 to 19 years (inclusive), consistent with the World Health Organization's (2003) definition of adolescence. Studies which included both adolescents and adults (e.g., Darrow et al., 2022), but reported findings for each cohort separately, were deemed eligible. A further criterion was that studies targeted adolescents with a current mental health diagnosis, as defined by the Diagnostic and Statistical Manual of Mental Disorders (DSM, any version; American Psychiatric Association [APA], 2022), or the International Statistical Classification of Diseases and Related Health Problems (ICD, any version; World Health Organization, 2019). Diagnoses based on structured diagnostic interviews informed by these classification systems (e.g., Structured Clinical Interview for DSM [SCID; First et al., 1997], Kiddie Schedule for Affective Disorders and Schizophrenia - Present and Lifetime Version [K-SADS-PL; Kaufman et al., 1997]) were therefore eligible. Conversely, studies that relied solely on self-reported mental health symptoms (e.g., Beck Depression Inventory-II; Beck et al., 1996) to determine eligibility for the given study were excluded due to the biases associated with self-reported data. Indeed, research has shown that people may underreport mental health symptoms for social desirability reasons (Van de Mortel, 2008). Recent evidence also suggests that self-report measures may be prone to an elevation bias, whereby participants provide higher ratings at their first assessment compared to subsequent times (Anvari et al., 2022). Thus, given concerns regarding the validity and reliability of self-report measures, studies were only included if a structured diagnostic assessment was conducted.

Studies involving adolescents with one or more traits/symptoms consistent with a personality disorder were deemed eligible, given differing opinions in the field regarding the appropriateness of providing a diagnosis for young people under the age of 18 (Schmeck, 2022; Uliaszek et al., 2014). Finally, studies involving neurodiverse samples (e.g., autism spectrum disorder, attention-deficit/hyperactivity disorder), or adolescents with a primary or comorbid behavioural disorder (e.g., conduct disorder, oppositional defiant disorder) or chronic physical illness (e.g., diabetes), were also ineligible due to the additional complexities associated with treating such subgroups (Ferrier et al., 2021; Paton & Hiscock, 2019).

Intervention

Studies needed to evaluate a third wave intervention with the aim of improving psychological outcomes in adolescents. Given the breadth of third wave interventions (Hayes & Hofmann, 2021), this review concentrated only on established therapies (i.e., MBCT, MBSR, DBT, ACT, and CFT), in accordance with past reviews by Linardon and colleagues (2017; 2019). Interventions had to target a specific mental health disorder (e.g., depression, social anxiety). Studies which focused on improving wellbeing more broadly, as well as those where elements of third wave interventions were mixed with other therapeutic approaches (e.g., art therapy, yoga, tai-chi) were ineligible, given the focus of this review was exclusively on third wave therapies for mental health. However, studies which made minor modifications to third wave interventions (e.g., adapted DBT program informed by psychodynamic theory; Khalid-Khan et al., 2016) were still included. Studies also needed to implement their interventions in face-to-face settings, either in an individual- or group-based format. Online or app-based mental health interventions or those involving parent-focused training only, without adolescent involvement, were excluded.

Comparison

Studies which included an active comparison (e.g., another treatment) or inactive control condition (e.g., waitlist control) were eligible. Quasi-experimental study designs (i.e., single group, pre-post designs), which provide an important platform for evaluating the 'real-world' effectiveness of a clinical intervention, were also eligible (Handley et al., 2018).

Outcome

Studies needed to administer an established, multi-item measure of mental health symptom severity (e.g., anxiety, depression, suicidal ideation) or general psychological functioning (e.g., emotional regulation capacity) pre- and post-intervention. Studies also needed to provide sufficient quantitative data to allow for the calculation of standardised mean group differences (e.g., group means, standard deviations, results from *t*-tests or one-way ANOVAs, exact *p*-values).

Data Extraction

Key data were extracted from each study by the author and double-checked by a senior researcher (DD). These data included: study characteristics (e.g., study design, country); sample demographics (e.g., sample size, mean age of sample, gender composition of sample); intervention characteristics (e.g., therapy framework, intervention duration and format); and effect size data (e.g., group means and standard deviations for each outcome measure and timepoint). Some data conversion was required, with Asarnow et al. (2021) providing standard errors, which were converted to standard deviations as per the formula provided by Higgins et al. (2022).

Risk of Bias in Studies

Methodological quality and the potential risk of bias in each study was assessed using Kmet et al.'s (2004) 14-item QualSyst tool for quantitative studies. The QualSyst tool assesses aspects that can compromise the validity of a study, including aspects related to the study design, conduct, and analyses (e.g., clarity of research objectives, robustness of sampling procedures and outcome measures, control of potential sample confounds). Each item is scored on a scale from 0 to 2, where 0 = "No", 1 = "Partial", and 2 = "Yes"; there is also an "N/A" response for items that are not applicable to the study design. For each study, an overall quality score was calculated through summing the responses across the 14 items and dividing this score by the maximum possible score (i.e., 28 – [number of "N/A" × 2]). Overall quality scores ranged from 0 (lowest quality) to 1 (highest quality). The percentage of studies meeting each criterion was also calculated.

Statistical Analyses

Effect size data were entered into, and analysed using, Comprehensive Meta-Analysis software (Version 4; Borenstein et al., 2022). Hedges' *g* was chosen as the effect size measure given its ability to correct for bias when sample sizes are small (Borenstein et al., 2009). Effect sizes were interpreted in accordance with recommendations by Cohen (1992), with values of 0.2, 0.5, and 0.8 representing small, medium, and large effects, respectively. Both short-term (i.e., baseline to post-intervention) and longer-term intervention effects (i.e., baseline to follow-up) were examined. Where studies reported more than one follow-up period, the most recent assessment was chosen in order to prevent a unit-of-analysis error when pooling effect estimates (Higgins et al., 2022).

As studies did not provide pre-post correlations (which are required when calculating *g*), a conservative estimate of $r = 0.7$ was used (Estrada et al., 2019). The direction of *g* was

standardised for ease of interpretation, such that a positive *g* denoted improvement among participants who received a third wave intervention, relative to controls (where applicable), while a negative *g* indicated deterioration among participants who received a third wave intervention or greater improvement by participants in control groups. For each *g*, 95% confidence intervals and *p*-values were calculated to determine effect size precision and statistical significance, respectively.

Individual effect sizes were grouped by study design (i.e., within- and between-groups) and outcome (e.g., depression, anxiety). Prior to pooling, each *g* was weighted by its corresponding study's inverse variance. Studies that provided multiple effect estimates per outcome were combined and averaged before pooling to ensure independence of data. To accommodate for between-study heterogeneity (e.g., study design, outcome measures, participants' mental health diagnoses), effect sizes were pooled using a random effects model. Heterogeneity was assessed using the I^2 statistic, which captures the proportion of observed variance that reflects true effect variance rather than sampling error (Borenstein et al., 2017). Other heterogeneity indices included tau (τ), which represents the standard deviation of *g*, and prediction intervals, which provide an indication of the dispersion of effect sizes (Higgins et al., 2022).

Given the tendency for significant results to be published at a higher rate than non-significant results (i.e., the file drawer problem; Rosenthal, 1979), publication bias was evaluated using a funnel plot with the 'trim and fill' method (Duval & Tweedie, 2000). The funnel plot was visually inspected for asymmetry and then checked with the 'trim and fill' method which statistically estimates the number of missing studies. Missing studies were subsequently imputed into the funnel plot until symmetry was attained (Shi & Lin, 2019).

Moderator analyses were also undertaken to examine the extent to which certain sample and therapy characteristics influenced the effectiveness of third wave interventions on mental health outcomes (combined as a whole). A univariate meta-regression was performed to explore the relationship between intervention duration (i.e., number of weeks the intervention ran for) and mental health symptom severity. The degree to which variance was accounted for (i.e., *Q* model statistics) and unaccounted for by the model (i.e., *Q* residual statistics) were both considered (Borenstein et al., 2009). Given the final sample of studies were all DBT-based interventions, a post-hoc subgroup analysis was conducted between studies that delivered high intensity (i.e., interventions that incorporated multiple therapeutic modes such as individual therapy, skills groups, and phone coaching) and low intensity DBT (e.g., skills groups). An initial plan to investigate the role of cultural context was not possible as there were insufficient studies to permit a sufficiently powered comparison between studies from Western and non-Western cultural backgrounds (Fu et al., 2011).

Results

Study Selection

The systematic literature search yielded 11,937 potentially relevant records, of which 7,048 were screened after duplicates were automatically removed. Off-topic records were excluded based on their title or abstract, leaving 1,575 records remaining. The full texts of the remaining 1,575 records were subsequently assessed, with 16 studies meeting all eligibility criteria.

Given the breadth of mental health outcomes reported in eligible studies, a post-hoc decision was made to focus on outcomes reported by at least three studies to allow for meta-analyses, namely: depression, anxiety, emotional regulation, suicidal ideation, self-harm, and BPD symptoms. The corresponding authors of two studies (Jaite et al., 2020; Schmeck et al., 2023) were contacted for additional data, although neither responded. As Jaite et al. (2020) focused on eating disorder related outcomes (which were not seen in any other eligible studies), this study was excluded. Of the 15 remaining studies, five had overlapping samples. For the purpose of this review, these five studies (*k*; number of studies) were counted as two in analyses; data were considered from all five studies as different mental health outcomes were sometimes reported. Thus, this resulted in a final sample of 12 independent studies (see Figure 1).

Study Characteristics

All studies were published in the last 15 years (see Table 2), reflecting the growing interest in third wave interventions in recent times (Hayes & Hofmann, 2021). The majority were conducted in highly developed, Western countries, namely the United States ($k = 5$) and the United Kingdom ($k = 2$). Most were described as pilot or feasibility studies and adopted a single group, pre-post design ($k = 9$). Of the three controlled trials, only two randomly

assigned participants to groups. Control conditions included treatment-as-usual (i.e., weekly CBT or psychodynamically-oriented therapy; Mehlum et al., 2014), supportive therapy (McCauley et al., 2018), and a manualised psychodynamic intervention, known as Adolescent Identity Treatment (Schmeck et al., 2023).

A total of 21 well-established and psychometrically sound outcome measures were used by the 12 included studies. Depression was the most common outcome of interest (k = 11), followed by BPD symptoms (k = 7) and emotional regulation (k = 5). Most studies (k = 11) utilised self-reported data (e.g., Beck Depression Inventory, Suicidal Ideation Questionnaire), with seven studies incorporating a clinical interview (e.g., Self-Injurious Thoughts and Behaviours Interview, International Personality Disorder Examination).

Sample Characteristics

The 12 studies comprised a total of 584 adolescents (M_{age} = 15.63, SD_{age} = 0.61, 89% female), with individual studies typically characterised by small sample sizes (N < 50; k = 7). Most adolescents (99%) had a history of suicidal or self-harm behaviour and met criteria for full or subthreshold BPD. Some also displayed antisocial personality traits (Uliaszek et al., 2014), while a few met diagnostic criteria for a dissociative disorder (Das et al., 2020). Comorbid mental health diagnoses, where reported (k = 4), included depressive disorders (e.g., 84% comorbidity; McCauley et al., 2018), followed by anxiety disorders (e.g., 58% comorbidity; Khalid-Khan et al., 2016). Notably, adolescents with substance abuse problems or more complex mental health presentations (i.e., schizophrenia, bipolar disorder) were typically excluded from studies, given the impacts these conditions would have had on treatment accessibility and responsiveness (Darrow et al., 2022; Uliaszek et al., 2014). Among the three controlled trials (McCauley et al., 2018; Mehlum et al., 2014; Schmeck et al., 2023), both intervention and control groups were statistically comparable on key

sociodemographic (e.g., gender, ethnicity) and clinical variables (e.g., BPD diagnosis, mental health comorbidities) at baseline.

Intervention Characteristics

Despite focusing on a range of third wave interventions in our search strategy, the final sample of 12 studies all evaluated DBT. Consistent with standard adult DBT protocols (Linehan, 1993b), skills targeted in the interventions included mindfulness, distress tolerance, emotion regulation, and interpersonal effectiveness. Interventions also incorporated modifications set out by Miller and colleagues (1997; 2007; Rathus & Miller, 2015) to better meet the developmental needs of adolescents. Key modifications included: reducing treatment length to 6 months or less (k = 10); including other stakeholders in weekly group skills training (e.g., parents, caregivers; k = 9); supplementing group sessions with individual family therapy (k = 7); simplifying training content to enhance learning accessibility (k = 12); and introducing a new module ("Walking the Middle Path") on validation, behavioural modification, and dialectics to target skill deficits commonly seen in adolescents with BPD symptomology and their families (k = 9).

Interventions were time-limited, with most completed within 6 months (M = 26.2 weeks, SD = 13.6 weeks). Two studies assessed a longer, 12-month course of DBT (James et al., 2008; James et al., 2011). The majority of studies (k = 10) only offered one cycle of treatment, with Hiller and Hughes (2022) and Darrow et al. (2022) offering additional cycles of DBT if therapy goals had not yet been achieved. Both studies also offered a graduate program, whereby treatment completers could attend weekly group sessions to aid skills generalisation, strengthen treatment gains, and reduce relapse risk.

Most studies (k = 9) delivered DBT in a high intensity format, incorporating multiple therapeutic modalities, namely: weekly individual therapy (~ 1 hour), weekly group skills

training (1.25–2 hours), family therapy (as required), and phone coaching (usually within business hours, as required). There were exceptions to this: James et al. (2011) provided skills training and support to other individuals involved in adolescents' lives (e.g., carers, school staff), all of whom were living in out-of-home care. Extensive (24 hour) telephone support with a therapist or crisis support numbers was also offered (Asarnow et al., 2021; Berk et al., 2020; Hiller & Hughes, 2022).

Three studies evaluated a low intensity DBT intervention via weekly (1.5–2.5 hours) skills groups. Uliaszek et al. (2014) additionally required adolescents to be engaged in a form of individual therapy (e.g., CBT, ACT, DBT) throughout the course of their skills group. Khalid-Khan et al.'s (2016) modified DBT skills intervention ("Managing Powerful Emotions") was distinct in that it incorporated material drawn from psychodynamic and attachment theories.

All DBT interventions were delivered by mental health professionals (e.g., psychologists, psychiatrists, social workers), with three including trainee (student) therapists (Berk et al., 2020; Hiller & Hughes, 2022; Uliaszek et al., 2014). Most therapists (k = 8) had received prior training in DBT from experienced practitioners, whilst four studies included therapists who were already well-versed in DBT practices (Das et al., 2020; James et al., 2008; James et al., 2011; Khalid-Khan et al., 2016). Intervention fidelity was monitored via weekly team meetings (k = 7) or weekly individual supervision (k = 4), and reviewing of audio or video recordings of sessions by lead therapists or supervisors (k = 6). Less common practices included the provision of structured feedback in line with Fruzzetti's (2012) DBT Therapist Rating and Feedback Form (k = 1), and use of the DBT Adherence Rating Scale (Linehan & Korslund, 2003) to quantify the extent to which sessions adhered to the DBT model (k = 4). The latter indicated good DBT adherence (M = 4.09 (out of 5), SD = 0.07, where scores > 4.0 are considered adherent).

The average attrition (dropout) rate across the 12 studies was moderate (M = 25.9%, SD = 8.46). Reasons for participant withdrawal, where reported (k = 2), included depressed mood and hopelessness (James et al., 2011), beliefs that treatment was not necessary (Uliazsek et al., 2014), and adverse treatment reactions (i.e., one caregiver experienced significant emotional dysregulation during a DBT skills group; Uliazsek et al., 2014). James et al. (2011) made concerted efforts to enhance participant engagement by selecting convenient community-based locations for therapy, providing transport to therapy, and offering meals at therapy sessions.

Risk of Bias in Studies

The overall methodological quality of the 12 included studies, based on their QualSyst ratings, was strong (M = 0.89, SD = 0.10; see Table 3). All studies clearly outlined their rationale and objectives (item 1: 100%), although two studies with a single group design (Das et al., 2020; Geddes et al., 2013) framed their respective research questions as testing the effect of DBT in an ideal research setting (i.e., efficacy) as opposed to its effect in the real world (i.e., effectiveness; item 2: 83%). Recruitment methods for participants were generally appropriate, with inclusion and exclusion criteria being fully described (item 3: 100%). Key sample parameters (e.g., age range, gender, ethnicity, mental health diagnoses; item 4: 75%) were also provided, aside from three studies which would have benefited from additional detail (e.g., reporting standard deviation or age range for participants; Khalid-Khan et al., 2016).

Given that most studies (k = 9) were single group designs, items 5 to 7 were only evaluated for the three controlled trials. It was not feasible to randomly allocate participants to either intervention (i.e., DBT or Adolescent Identity Treatment; item 5) in Schmeck et al.'s (2023) multi-site trial, with each location having expertise in one intervention. Blinding of assessors in this same trial may have been possible, although was not carried out (item 6).

Randomisation and blinding of assessors in the two RCTs (McCauley et al., 2018; Mehlum et al., 2014) was sound; however, as is typical in psychotherapy research, participants were fully informed of the intervention that they received (item 7; Gluud, 2006).

Studies utilised well-established and reliable outcome measures, consistent with the inclusion criteria for this review (item 8: 92%). Nevertheless, it was not clear how one variable of interest, namely substance use, was measured in Khalid-Khan et al.'s (2016) study. Only four studies conducted an a priori power analysis to justify their sample size (Hiller & Hughes, 2022; McCauley et al., 2018; Mehlum et al., 2014; Schmeck et al., 2023; item 9: 67%). Four single group studies (Das et al., 2020; Geddes et al., 2013; Khalid-Khan et al., 2016; Uliaszek et al., 2014) were also underpowered to detect a significant large effect (N = 15, power = 0.8, $\propto$ = 0.05; G*Power; Faul et al., 2007). Notably, these studies were described as pilot interventions specifically designed to test feasibility.

Appropriate statistical analyses (e.g., pre-post treatment comparisons; item 10: 100%) were undertaken, and estimates of variance were reported (e.g., standard deviations, 95% confidence intervals; item 11: 100%). Furthermore, all three controlled trials (McCauley et al., 2018; Mehlum et al., 2014; Schmeck et al., 2023) adopted intent-to-treat analysis, considered 'gold standard' for the interpretation of such studies (McCoy, 2017). Only six studies attempted to control for potential sample confounds through random allocation or statistical procedures (e.g., subgroup analysis, multivariate models; item 12: 25%). Most studies routinely reported results for all outcomes; exceptions included James et al. (2011) who only provided qualitative results for some outcomes and Schmeck et al. (2023) who did not report immediate post-intervention data even though this was pre-specified (item 13: 75%). Finally, conclusions and clinical implications drawn were an accurate reflection of each study's results (item 14: 100%). Overall, while most studies were single group designs and displayed some concerns regarding bias, methodological quality was generally high.

Publication Bias

Inspection of the funnel plot showed minor asymmetry, with two studies being imputed (see Figure 2). The inclusion of these two studies did not, however, markedly change the magnitude of the overall effect ($g_{observed} = 0.70$, $g_{imputed} = 0.61$).

Effectiveness of Third Wave Interventions

Eleven studies reported post-intervention outcomes to allow the calculation of short-term effects, with five studies also providing follow-up data, ranging from 3 months to 3 years (Asarnow et al., 2021; Geddes et al., 2013; James et al., 2008; Mehlum et al., 2016; Mehlum et al., 2019; Schmeck et al., 2023). Effect size data for these studies are grouped by mental health symptom domain and study design (i.e., within- and between-groups; see Table 4).

Depression

Ten studies assessed depression, with eight reporting significant and medium to large immediate improvements in symptom severity. The resulting pooled effect was large and significant. These results were, however, associated with high levels of inconsistency between studies, even among those that adopted the same study design ($I^2 > 80\%$). Indeed, DBT was found to have a significant, positive, and medium effect on depression ($g = 0.66$, $p = 0.005$, 95% CI = [0.20, 1.11]) in comparison to usual care (Mehlum et al., 2014), yet a negligible, non-significant effect ($g = 0.02$, $p = 0.896$, 95% CI = [-0.28, 0.32]) emerged when DBT was compared to supportive therapy (Asarnow et al., 2021).

There was evidence of a sustained and positive impact of DBT on depressive symptoms, as suggested by the large and significant pooled effect at follow-up. Individual studies did, however, range in the magnitude of their effects – from medium gains (e.g., $g = 0.52$; Schmeck et al., 2023) to very large (e.g., $g = 1.41$; Geddes et al., 2013). When

compared to other treatments (k = 3; Asarnow et al., 2021; Mehlum et al., 2019; Schmeck et al., 2023), DBT had a negligible effect on depression. For instance, Asarnow et al. (2021) reported that DBT and supportive therapy had similar effects on depressive symptoms at 6 months post-intervention (g = 0.11, p = 0.489, 95% CI = [-0.19, 0.40]).

Anxiety

Three single group studies examined the impact of DBT on anxiety, with two reporting positive and significant effects (Das et al., 2020; Geddes et al., 2013). This finding does, however, need to be interpreted cautiously given the associated wide confidence interval – likely a reflection of the small sample sizes for these studies (i.e., all $N < 10$). The wide-ranging prediction interval also highlights that the effects of future DBT interventions on anxiety may vary markedly, from negative to very large and positive treatment effects. The effect size for anxiety remained large and significant at follow-up. Again, however, this finding needs to be treated cautiously as it was based on a single study (Geddes et al., 2013).

Emotional Regulation

All five studies that assessed individuals' capacity to regularly use proactive strategies to diffuse or moderate negative emotions reported large and significant improvements with DBT (Asarnow et al., 2021; Berk et al., 2020; Das et al., 2020; Geddes et al., 2013; Hiller & Hughes, 2022). Effect estimates were relatively consistent in magnitude across these studies (I^2 = 0%). Notably, the large-scale RCT (N = 173) by Asarnow et al. (2021) produced a medium, significant effect: DBT was superior to supportive therapy in enhancing youth emotion regulation.

The pooled effect size at follow-up remained large and significant, although this finding was only based on two studies (Asarnow et al., 2021; Geddes et al., 2013). Notably,

Asarnow et al. (2021) reported continued gains in emotion regulation with DBT throughout their follow-up periods (i.e., 3 and 6 months post-intervention).

Suicidal Ideation

Three studies examined the immediate effects of DBT on suicidal ideation, contributing to a large and significant pooled effect (Berk et al., 2020; Darrow et al., 2022; Mehlum et al., 2014). DBT was also more effective at reducing suicidal ideation than usual care and supportive therapy (k = 2; McCauley et al., 2018; Mehlum et al., 2014). However, there was inconsistency in reported treatment gains (I^2 > 60%). In particular, DBT had a large effect on suicidal ideation when compared to usual care (g = 0.80; Mehlum et al., 2014), yet only a small effect emerged when compared to supportive therapy (g = 0.33; McCauley et al., 2018). Longer-term reductions in suicidal ideation at follow-up remained large and significant, although this was based on limited data (k = 1; Mehlum et al., 2019). When compared to usual care, the between-group difference at 6 months post-intervention was small and non-significant (Mehlum et al., 2019).

Self-Harm

Two single-group studies examined the number of self-harm episodes with DBT, with both reporting significant decreases – resulting in a large, pooled effect size (James et al., 2008; James et al., 2011). Treatment effects remained large and significant at follow-up (k = 2), although between-study heterogeneity was evident (I^2 > 80%), with reductions in self-harm ranging from medium (g = 0.50; Schmeck et al., 2023) to very large (g = 1.20; James et al., 2008). The single controlled trial (Schmeck et al., 2023) reported a medium, non-significant effect at 5 months post-intervention; the associated confidence interval also indicates the true effect size may not differ from zero.

BPD Symptoms

Six studies examined the impact of DBT on BPD symptoms such as impulsivity, affective instability, and identity disturbance, with all reporting significant and immediate symptom reduction post-intervention. Between-study differences were, however, evident. For example, Asarnow et al. (2021) reported a medium effect on BPD symptoms (g = 0.60), while James et al. (2008) found a very large effect (g = 1.76). Moreover, the associated prediction interval suggests that the mean effect size among 95% of future comparable studies could fall anywhere between a negligible effect (i.e., marginally above the value of zero) and 2.04. The effect reported by the two between-group studies was medium in magnitude, but still significant (Asarnow et al., 2021; Mehlum et al., 2014).

There was evidence of sustained positive impacts of DBT on BPD symptoms, with a significant, medium-to-large pooled effect at follow-up (k = 3; Asarnow et al., 2021; Mehlum et al., 2019; Schmeck et al., 2023). However, given the prediction interval contained zero, this suggests that effect sizes may not be as promising in future DBT studies. The three controlled trials reported a pooled non-significant effect. For example, Schmeck et al.'s (2023) trial found that DBT and Adolescent Identity Treatment had comparable effects on BPD symptoms at 5 months post-intervention (g = -0.61, p = 0.057, 95% CI = [-1.24, 0.02]).

Subgroup Analysis: High vs. Low Intensity DBT

A comparison of high intensity DBT interventions (k = 8) with low intensity interventions (k = 3) revealed no significant difference between the two subgroups ($Q_{Between}(1) = 0.14$, p = 0.713; see Table 5). The associated confidence intervals for each subgroup's effect estimate also overlapped. Thus, intervention intensity did not moderate the relationship between DBT and mental health outcomes, with large and statistically significant improvements reported across all programs. The associated wide prediction intervals,

however, suggest that the reverse (i.e., negative) effect may occur in future, comparable studies examining low intensity DBT.

Meta-Regression: DBT Duration

A univariate meta-regression determined that intervention duration did not significantly contribute to treatment outcomes ($Q_{Model}(1) = 0.00$, $p = 0.965$; $k = 11$; see Figure 3 for scatterplot). Indeed, the total length of the DBT intervention (number of weeks) made no impact on the extent to which adolescents' mental health changed over the course of the intervention ($Q_{Residual}(9) = 74.13$, $p < 0.001$, $I^2 = 87.86$, $\tau = 0.29$).

Discussion

This systematic review and meta-analysis evaluated the efficacy and effectiveness of third wave interventions for adolescents with mental health disorders. Although the study's definition of third wave treatments included a broad range of approaches (e.g., MBSR, MBCT, ACT), only studies that evaluated DBT were assessed after screening using inclusion and exclusion criteria.

Across the 12 studies included in this review, there was clear evidence that DBT can improve a range of mental health outcomes in clinical adolescent populations. Indeed, when examining within-group change from pre- to post-intervention, large and significant effect sizes were found for all outcomes assessed, namely: depression, anxiety, emotional regulation, suicidal ideation, self-harm, and BPD symptoms. These findings align with a previous meta-analytic review on third wave interventions with adolescents, in which significant large-sized improvements in emotional symptoms/internalising problems were observed in clinical settings (Perkins et al., 2023). Although DBT outperformed two other active treatments (i.e., usual care, supportive therapy) at post-intervention in the current review, the magnitude of the effects were less pronounced in between-group designs, which is a common finding in intervention research (Bakker et al., 2019). Follow-up analyses demonstrated that DBT continued to provide ongoing benefit for adolescents past the intervention phase, with significant large effects for all mental health outcomes assessed. However, when DBT was compared with other active interventions at follow-up, pooled effect estimates tended to be small and non-significant. Similar findings have been found in other meta-analyses, whereby differences between third wave interventions and active comparator treatments (e.g., CBT) at post-intervention or follow-up have not been statistically significant (Fang & Ding, 2020; Linardon et al., 2017).

Even though this review found that DBT was generally effective at bringing about mental health improvement in clinical adolescent populations, there was significant variability in treatment response across studies. Indeed, post-intervention effect size estimates for depression (g_{range} = 0.16–1.18), anxiety (g_{range} = 0.33–1.53), and BPD symptoms (g_{range} = 0.60–1.76) varied considerably. Given DBT did not always lead to large improvements in mental health outcomes, it is possible that other third wave interventions (e.g., ACT, MBCT) may provide greater benefit to adolescents with mental health disorders, particularly when tailored to the needs and preferences of this cohort (Beck et al., 2010; Swift et al., 2011). A more tailored approach to treatment may also increase engagement and reduce attrition (Beck et al., 2010; Swift et al., 2011), which was moderate across the 12 studies in this review. Understanding the reasons why certain adolescents withdraw from DBT, which was typically not examined and/or reported in the studies assessed, may also help to improve therapy retention in future studies.

Moderator analyses revealed that the intensity and duration of the DBT intervention did not significantly contribute to treatment outcomes. Such findings, however, need to be interpreted cautiously as the analysis between high intensity and low intensity DBT interventions was underpowered (Fu et al., 2011), and based on very few RCTs. Thus, the comparative effectiveness of different intensities of DBT for adolescents with mental health disorders warrants further examination. Given the time, resources, and costs associated with implementing intensive DBT programs (Uliaszek et al., 2014), greater understanding of the effectiveness of less intensive DBT interventions (e.g., skills groups) with clinical adolescent populations would be particularly helpful.

Methodological Considerations, Limitations, and Directions for Future Research

A notable finding from the current review is that all included studies were DBT interventions, with no studies focusing on other third wave interventions (e.g., ACT, MBCT). Although on one hand it may appear there have been few studies examining third wave treatments beyond DBT, the stringent inclusion criteria for the current review must be acknowledged, given all adolescents had to meet full criteria for a mental health disorder (with the exception of personality disorders) and interventions had to target a specific mental health disorder. Indeed, an initial systematic literature search identified studies that applied ACT and MBIs with adolescents with mental health difficulties (e.g., Biegel et al., 2009; Díaz-González et al., 2018; Hayes et al., 2011; Swain et al., 2015), although these studies were ultimately excluded as they did not meet all inclusion criteria. As one of the criterions for this review was for interventions to target a specific mental health disorder, it is not surprising that studies involving ACT or MBIs were excluded given such approaches are often applied to presentations transdiagnostically (Dindo et al., 2017; Geurts et al., 2021). Nevertheless, greater research into the effect of these third wave interventions on specific mental health disorders in adolescents (e.g., ACT and social anxiety disorder) would benefit researchers and clinicians alike.

The current study has limitations. The review's search strategy focused on established third wave therapies originally designed for adults (e.g., DBT, MBSR, ACT); therefore, potentially relevant studies that targeted adolescents with mental health disorders (e.g., studies examining more recent third wave interventions that were specifically developed for adolescents) may have been missed. Another key limitation of the current review is that many of the included studies were quasi-experimental designs. Given these studies lacked certain aspects of methodological rigour (e.g., lack of random allocation, absence of control group), this may have compromised some of the analyses, thereby affecting estimates of treatment

effectiveness. It is important to acknowledge, however, that inclusion of quasi-experimental studies was deemed necessary due to the lack of RCTs on third wave interventions in adolescent samples. Sample sizes also tended to be small (i.e., $N < 50$), with most participants being female and presenting with full or subthreshold BPD, thereby limiting the generalisability of results. As all studies included in this review evaluated DBT, it makes sense that majority of studies focused on BPD as this is what DBT was originally designed to treat (Linehan, 1993a). The fact that females are more commonly diagnosed with BPD (APA, 2022), as well as more likely to seek psychological support (Harris et al., 2015), are two factors that may partially explain the significant gender imbalance observed in the current review. Therefore, it is vital that future research explores the effect of third wave interventions in more diverse adolescent samples, including those with different gender identities (e.g., male, transgender) and clinical presentations (e.g., major depressive disorder, social anxiety disorder, obsessive compulsive disorder). Another limitation that needs to be acknowledged is that a pre-post correlation estimate ($r = 0.7$) was utilised when calculating effect sizes, which may have influenced results obtained.

Only a small number of studies incorporated follow-up assessments; thus, the conclusions drawn from this review regarding longer-term outcomes of DBT should be regarded as preliminary. Furthermore, it was not possible to more closely examine maintenance effects of the intervention (i.e., from post-intervention to follow-up) due to one study (Schmeck et al., 2023) not reporting post-intervention outcomes. Given the current review only included articles written in English (or available in English translation), findings may have also been affected by language or cultural bias. Indeed, only one study from the current review was conducted in a non-Western cultural context, namely India (Das et al., 2020). As cultural factors can significantly impact therapy processes and outcomes (Huang & Zane, 2016; Koç & Kafa, 2019), it is critical that more research examines DBT (and other

third wave interventions) with adolescents in non-Western cultural settings to determine whether similar results emerge. The findings of the current review were also impacted by publication bias, as evidenced by the funnel plot analysis. While including grey literature (e.g., theses/dissertations) may have reduced the extent of publication bias and provided more accurate effect size estimates (Mahood et al., 2014; Paez, 2017), incorporating such literature in the review would have created additional pressures on time and resources, along with challenges in executing a systematic, transparent, and replicable review.

In line with recent shifts towards the processes and mechanisms of change in psychotherapy (Hayes & Hofmann, 2021), future research should explore how and why third wave interventions lead to positive changes in adolescents' mental health. One mechanism that may warrant further exploration is emotion regulation, one of the core skills taught in DBT interventions (Linehan, 1993b); some initial research has occurred in this space, with Asarnow et al. (2021) finding that improved emotion regulation mediated the effect of DBT on self-harm remission in adolescents with full or subthreshold BPD. Such research could be expanded to other mental health outcomes (e.g., depression, anxiety). Overall, greater research into change processes across different third wave interventions will aid therapists in the creation of more tailored, person-centred approaches (Hayes & Hofmann, 2021), which may result in better treatment outcomes for adolescents. Nevertheless, the current study, with its focus on reviewing and synthesising the evidence for third wave interventions for adolescents with mental health disorders, was deemed a necessary first step to improved clinical decision making.

Conclusion

This review found that DBT is effective in improving psychological functioning and mental health symptoms in clinical adolescent populations. However, the extent to which

DBT can provide sustained, longer-term benefits to adolescents' mental health is less clear. Additional RCTs with greater methodological rigour, larger sample sizes, and the inclusion of follow-up assessments are needed to draw more confident conclusions about the efficacy of DBT for adolescents with mental health disorders. More empirical attention also needs to be given to other third wave interventions (e.g., ACT, MBCT, CFT); however, the stringent inclusion criteria (e.g., requiring adolescents to have a current mental health diagnosis) precluded the examination of some studies. Overall, the evaluation of third wave interventions in more diverse adolescent samples (e.g., males, non-Western cultures, other mental health diagnoses beyond BPD) is urgently needed.

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Figure 1

PRISMA flow diagram outlining study selection process (Page et al., 2021)

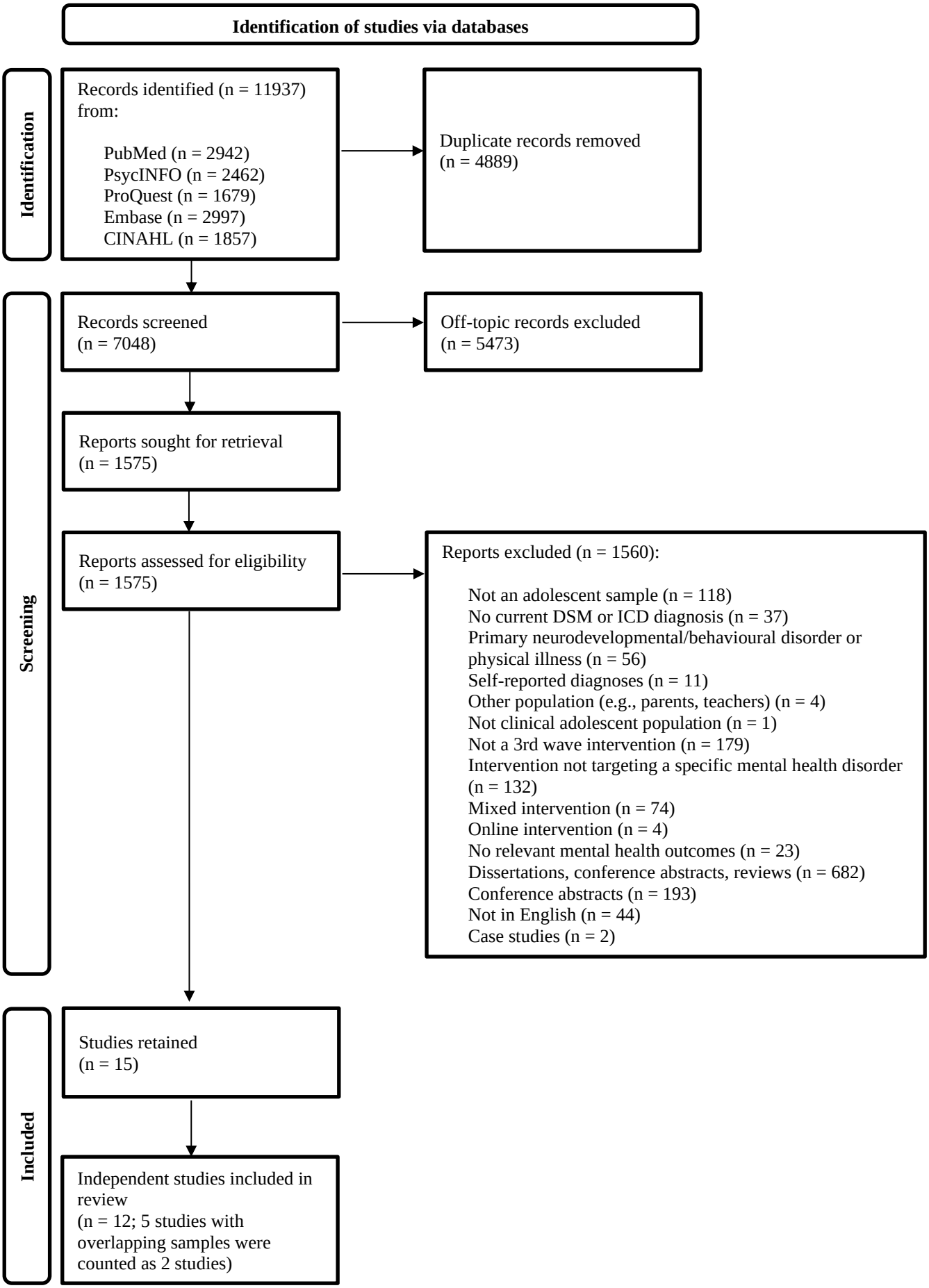
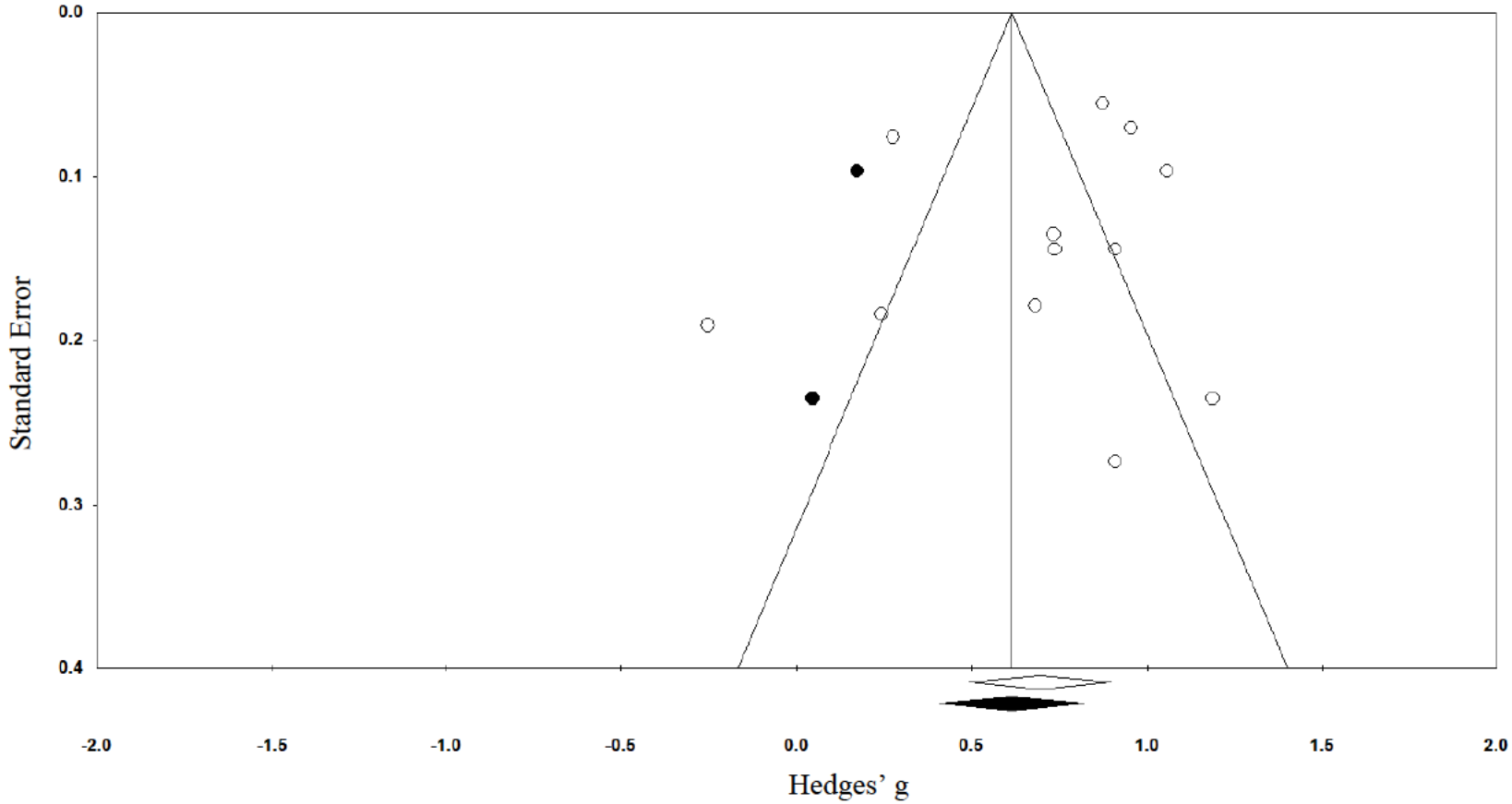


Figure 2

Funnel plot of standard error by Hedges' g



Note. ○ = observed samples; ● = imputed samples; ◇ = observed effect estimate; ◆ = imputed effect estimate

Figure 3

Univariate meta-regression for DBT duration

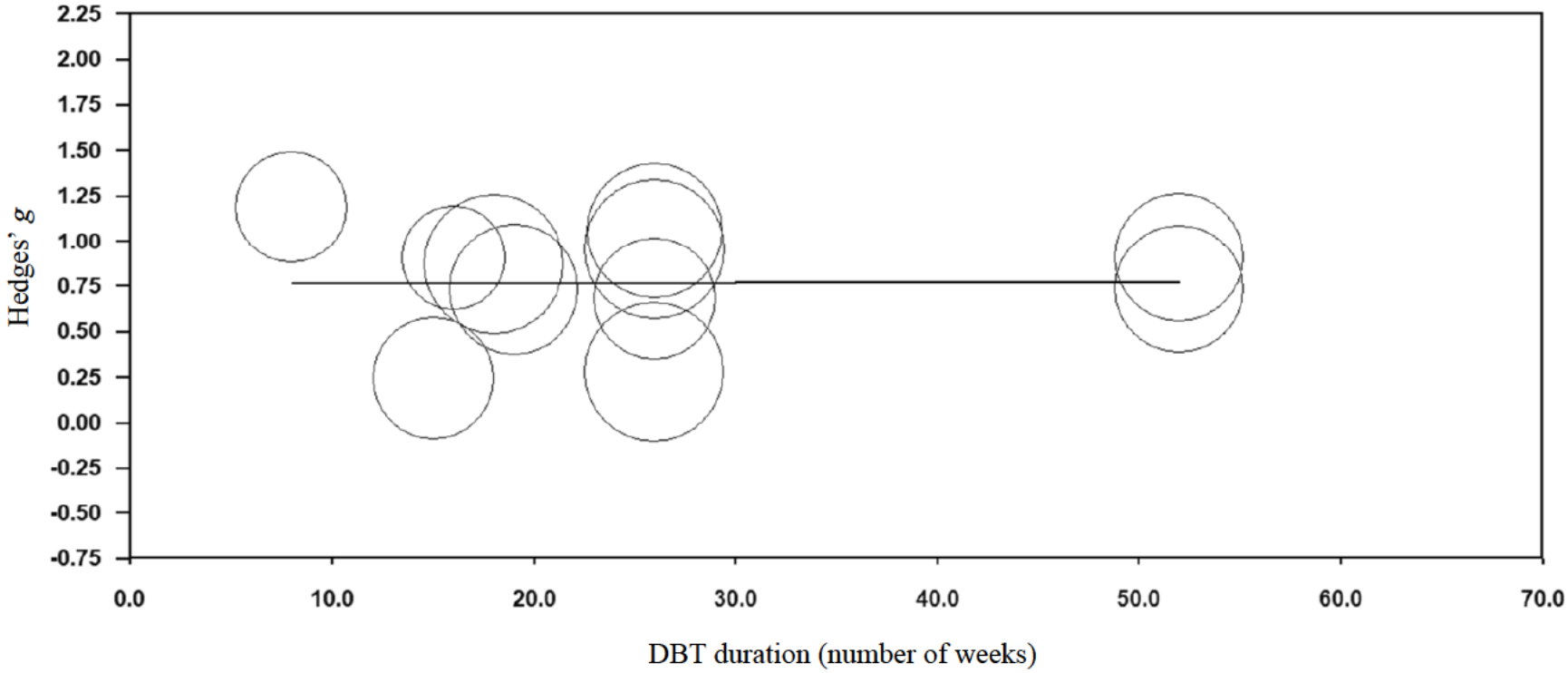


Table 1

Logic grids for databases

Database	Adolescents	Third Wave Interventions
PubMed	"adolescent"[mh] OR adolescen*[tiab] OR teen*[tiab] OR youth[tiab] OR young person*[tiab] OR young people[tiab] OR middle school student*[tiab] OR high school student*[tiab] OR secondary school student*[tiab]	"mindfulness"[mh] OR "acceptance and commitment therapy"[mh] OR "dialectical behavior therapy"[mh] OR third wave[tiab] OR 3rd wave[tiab] OR mindful*[tiab] OR "acceptance and commitment therapy"[tiab] OR acceptance based[tiab] OR mindfulness based stress reduction[tiab] OR MBSR[tiab] OR mindfulness based cognitive therapy[tiab] OR MBCT[tiab] OR dialectical behavior therapy[tiab] OR dialectical behaviour therapy[tiab] OR DBT[tiab] OR compassion focused therapy[tiab] OR compassion focussed therapy[tiab] OR CFT[tiab] OR compassionate mind training[tiab] OR compassion based[tiab]
PsycINFO	adolescen*.ti,ab OR teen*.ti,ab OR youth.ti,ab OR young person*.ti,ab OR young people.ti,ab OR middle school students.sh OR junior high school students.sh OR high school students.sh OR (student* adj2 (middle school OR high school OR secondary school)).ti,ab	mindfulness.sh OR mindfulness-based interventions.sh OR "acceptance and commitment therapy".sh OR dialectical behavior therapy.sh OR third wave.ti,ab OR 3rd wave.ti,ab OR mindful*.ti,ab OR "acceptance and commitment therapy".ti,ab OR acceptance based.ti,ab OR mindfulness based stress reduction.ti,ab OR MBSR.ti,ab OR mindfulness based cognitive therapy.ti,ab OR MBCT.ti,ab OR dialectical behavio?r therapy.ti,ab OR DBT.ti,ab OR compassion focused therapy.ti,ab OR compassion focussed therapy.ti,ab OR CFT.ti,ab OR compassionate mind training.ti,ab OR compassion based.ti,ab
ProQuest	mainsubject(adolescen*) OR TI,AB(adolescen* OR teen* OR youth OR "young person*" OR "young people" OR student* N/2 ("middle school" OR "high school" OR "secondary school"))	mainsubject(mindfulness) OR mainsubject.Exact("acceptance & commitment therapy") OR TI,AB("third wave" OR "3rd wave" OR mindful* OR "acceptance and commitment therapy" OR "acceptance based" OR "mindfulness based stress reduction" OR MBSR OR "mindfulness based cognitive therapy" OR MBCT OR "dialectical behavio?r therapy" OR DBT OR "compassion focused therapy" OR "compassion focussed therapy" OR CFT OR "compassionate mind training" OR "compassion based")

Database	Adolescents	Third Wave Interventions
Embase	exp adolescent OR adolescen*.ti,ab OR teen*.ti,ab OR youth.ti,ab OR young person*.ti,ab OR young people.ti,ab OR middle school student.sh OR high school student.sh OR (student* adj2 (middle school OR high school OR secondary school)).ti,ab	exp mindfulness OR mindfulness-based stress reduction.sh OR "acceptance and commitment therapy".sh OR dialectical behavior therapy.sh OR third wave.ti,ab OR 3rd wave.ti,ab OR mindful*.ti,ab OR "acceptance and commitment therapy".ti,ab OR acceptance based.ti,ab OR mindfulness based stress reduction.ti,ab OR MBSR.ti,ab OR mindfulness based cognitive therapy.ti,ab OR MBCT.ti,ab OR dialectical behavio?r therapy.ti,ab OR DBT.ti,ab OR compassion focused therapy.ti,ab OR compassion focussed therapy.ti,ab OR CFT.ti,ab OR compassionate mind training.ti,ab OR compassion based.ti,ab
CINAHL	MH adolescence+ OR TI (adolescen* OR teen* OR youth OR "young person*" OR "young people") OR AB (adolescen* OR teen* OR youth OR "young person*" OR "young people") OR MH "Students, High School" OR MH "Students, Middle School" OR TI (student* N2 ("middle school" OR "high school" OR "secondary school")) OR AB (student* N2 ("middle school" OR "high school" OR "secondary school"))	MH mindfulness+ OR MH "acceptance and commitment therapy" OR MH "dialectical behavior therapy" OR TI ("third wave" OR "3rd wave" OR mindful* OR "acceptance and commitment therapy" OR "acceptance based" OR "mindfulness based stress reduction" OR MBSR OR "mindfulness based cognitive therapy" OR MBCT OR "dialectical behavio#r therapy" OR DBT OR "compassion focused therapy" OR "compassion focussed therapy" OR CFT OR "compassionate mind training" OR "compassion based") OR AB ("third wave" OR "3rd wave" OR mindful* OR "acceptance and commitment therapy" OR "acceptance based" OR "mindfulness based stress reduction" OR MBSR OR "mindfulness based cognitive therapy" OR MBCT OR "dialectical behavio#r therapy" OR DBT OR "compassion focused therapy" OR "compassion focussed therapy" OR CFT OR "compassionate mind training" OR "compassion based")

Table 2

Study characteristics

Lead author (year)	Country	Study design	Sample characteristics				DBT characteristics			Outcome measures^	Control group
			Total *N* (I:C)	Mean age (in years) ± SD *or* range	Gender (M:F:O)	Diagnosis	Intensity	Duration	Frequency		
Berk (2020)	United States	Single group	24	15.2 ± 1.3	2:22:0	Subthreshold or full BPD	High	6 months	Individual: weekly Group: 1.25 hrs/week Family: as needed Phone: as needed	SIQ-JR DERS BDI-II SCID-II	N/A
Darrow (2022)	United States	Single group	87	15.9 ± 1.2	12:70:5	Subthreshold or full BPD	High	6 months	Individual: weekly Group: weekly Family: as needed Phone: as needed	SIQ BDI-II	N/A
Das (2020)	India	Single group	5	16.0 ± 1.0	2:3:0	Dissociative disorder	Low	8 weeks	Group: 18 sessions	CDI 2 HAM-A CERQ	N/A
Geddes (2013)	Australia	Single group	6	15.1 (14.6-15.7)	0:6:0	Subthreshold or full BPD	High	26 weeks	Individual: weekly Group: 2 hrs/week for 18 weeks Family: as needed Phone: as needed	TSCC-D TSCC-A MACS-A	N/A
Hiller (2022)	United States	Single group	91	15.2 ± 1.6	11:76:4	Subthreshold or full BPD	High	16-20 weeks	Individual: 1 hr/week Group: 2 hrs/week Family: as needed Phone: as needed	BDI-II SCID-II LPI-ED	N/A
James (2008)	United Kingdom	Single group	16	16.4 ± 1.2	0:16:0	BPD	High	1 year	Individual: 1 hr/week Group: 1.5 hrs/week Phone: as needed	SCID-II BDI Self-harm	N/A
James (2011)	United Kingdom	Single group	25	15.5 ± 1.5	3:22:0	BPD	High	1 year	Individual: 1 hr/week Group: 2 hrs/week Phone: as needed	BDI Self-harm	N/A

Lead author (year)	Country	Study design	Sample characteristics			DBT characteristics				Outcome measures^	Control group
			Total *N* (I:C)	Mean age (in years) ± SD *or* range	Gender (M:F:O)	Diagnosis	Intensity	Duration	Frequency		
Khalid-Khan (2016)	Canada	Single group	7	16.9 (not reported)	1:6:0	Subthreshold or full BPD	Low	15 weeks	Group: 2.5 hrs/week Parent/Caregiver Only: 6 sessions	BDI-Y CDI 2 BAI-Y MASC	N/A
McCauley (2018) & Asarnow (2021)*	United States	RCT	173 (86:87)	14.9 ± 1.5	10:163:0	Subthreshold or full BPD	High	6 months	Individual: weekly Group: weekly Family: as needed Phone: as needed	SIQ-JR CES-D BPFS-C DERS	Supportive Therapy
Mehlum (2014, 2016, 2019)*	Norway	RCT	77 (39:38)	15.6 ± 1.5	9:68:0	Subthreshold or full BPD	High	19 weeks	Individual: 1 hr/week Group: 2 hrs/week Family: as needed Phone: as needed	SIQ-JR SMFQ MADRS BSL	Usual Care
Schmeck (2023)	Germany & Switzerland	Controlled trial	60 (37:23)	15.8 ± 1.4	4:56:0	Subthreshold or full BPD	High	6-8 months	Individual: weekly over 25 weeks Group: weekly over 20 weeks Family: 5-8 sessions Phone: as needed	SCID-II SITBI BDI-II	Adolescent Identity Treatment
Uliaszek (2014)	United States	Single group	13	15.0 ± 1.6	2:11:0	Subthreshold or full BPD & ASPD	Low	16 weeks	Group: 2 hrs/week	IPDE	N/A

Note. * = Five studies with overlapping samples considered as two independent studies for this review; ^ = Measures specific to this review; I = Number of participants in intervention group; C = Number of participants in control group; M = Number of male participants; F = Number of female participants; O = Number of participants belonging to other gender identities; RCT = Randomised controlled trial; SD = Standard deviation; DBT = Dialectical Behaviour Therapy; BPD = Borderline Personality Disorder; ASPD = Antisocial Personality Disorder; BDI = Beck Depression Inventory; BDI-II = Beck Depression Inventory Second Edition; BDI-Y = Beck Depression Inventory for Youth; BAI-Y = Beck Anxiety Inventory for Youth; DERS = Difficulties in Emotion Regulation Scale; CES-D = Center for Epidemiologic Studies Depression Scale; BPFS-C = Borderline Personality Features Scale for Children; SIQ = Suicidal Ideation Questionnaire; SIQ-JR = Suicidal Ideation Questionnaire Junior; SMFQ = Short Mood and Feelings Questionnaire; MADRS = Montgomery–Åsberg Depression Rating Scale; BSL = Borderline Symptom List; CDI 2 = Children's Depression Inventory 2; HAM-A = Hamilton Anxiety Rating Scale; CERQ = Cognitive Emotion Regulation Questionnaire; TSCC-D = Trauma Symptom Checklist for Children – Depression scale; TSCC-A = Trauma Symptom Checklist for Children – Anxiety scale; MACS-A = Modified Affective Control Scale for Adolescents; SCID-II = Structured Clinical Interview for DSM-IV Axis II Disorders; LPI-ED = Life Problems Inventory – Emotional Dysregulation scale; IPDE = International Personality Disorder Examination; MASC = Multidimensional Anxiety Scale for Children; SITBI = Self-Injurious Thoughts and Behaviours Interview.

Table 3

Risk of bias within studies (Kmet et al., 2004)

Lead author (year)	1. Question/ objective sufficiently described	2. Study design evident and appropriate	3. Methods of subject/comparison group selection or source of information/input variables described/appropriate	4. Subject (and comparison group) characteristics sufficiently described	5. Random allocation to treatment group described	6. Blinding of investigators to intervention	7. Blinding of subjects to intervention	8. Outcome(s) well defined and robust to measurement/ misclassification bias. Means of assessment reported.	9. Sample size appropriate	10. Analytic methods described/ justified/ appropriate	11. Some estimate of variance reported	12. Controlled for confounding	13. Results reported in sufficient detail	14. Conclusions supported by results	Total score (raw score/ total possible)
Berk (2020)	2	2	2	2	N/A	N/A	N/A	2	2	2	2	0	2	2	0.91
Darrow (2022)	2	2	2	2	N/A	N/A	N/A	2	2	2	2	1	2	2	0.95
Das (2020)	2	1	2	1	N/A	N/A	N/A	2	1	2	2	0	2	2	0.77
Geddes (2013)	2	1	2	1	N/A	N/A	N/A	2	1	2	2	0	2	2	0.77
Hiller (2022)	2	2	2	2	N/A	N/A	N/A	2	2	2	2	1	2	2	0.95
James (2008)	2	2	2	2	N/A	N/A	N/A	2	2	2	2	0	2	2	0.91
James (2011)	2	2	2	2	N/A	N/A	N/A	2	2	2	2	0	1	2	0.86
Khalid-Khan (2016)	2	2	2	1	N/A	N/A	N/A	1	0	2	2	0	1	2	0.68
McCauley (2018) & Asarnow (2021)*	2	2	2	2	2	2	N/A	2	2	2	2	2	2	2	1.00
Mehlum (2014, 2016, 2019)*	2	2	2	2	2	2	N/A	2	2	2	2	2	2	2	1.00
Schmeck (2023)	2	2	2	2	N/A	0	N/A	2	2	2	2	2	1	2	0.88
Uliaszek (2014)	2	2	2	2	N/A	N/A	N/A	2	1	2	2	1	2	2	0.91
% that met criterion	100%	83%	100%	75%	100%	67%	–	92%	67%	100%	100%	25%	75%	100%	–

Note. * = Five studies with overlapping samples considered as two independent studies for this review; 2 = Criterion met; 1 = Criterion partially met; 0 = Criterion not met; N/A = Criterion not applicable.

Table 4

Pooled effect size estimates for each outcome variable based on study design

Outcome	Comparison	*k*	Pooled/combined effect size					Heterogeneity	
			g	95% CI	Prediction interval	SE	*p*	I^2	τ
Depression	Within group change (post-intervention)	10	0.77	[0.63, 0.90]	[0.39, 1.14]	0.07	< 0.001***	49.33	0.15
	Between group difference (post-intervention)	2	0.31	[-0.31, 0.93]	–	0.32	0.323	80.95	0.40
	Within group change (follow-up)	5	0.94	[0.70, 1.18]	[0.21, 1.66]	0.12	< 0.001***	55.76	0.19
	Between group difference (follow-up)	3	-0.03	[-0.38, 0.31]	[-3.52, 3.45]	0.18	0.847	46.53	0.21
Anxiety	Within group change (post-intervention)	3	0.86	[0.19, 1.53]	[-6.55, 8.27]	0.34	0.012*	65.03	0.47
	Between group difference (post-intervention)	0	–	–	–	–	–	–	–
	Within group change (follow-up)	1	1.71	[0.72, 2.69]	–	0.50	0.001**	–	–
	Between group difference (follow-up)	0	–	–	–	–	–	–	–
Emotional regulation	Within group change (post-intervention)	5	0.91	[0.79, 1.03]	–	0.06	< 0.001***	0.00	0.00
	Between group difference (post-intervention)	1	0.48	[0.18, 0.78]	–	0.15	0.002**	–	–
	Within group change (follow-up)	2	1.18	[0.85, 1.51]	–	0.17	< 0.001***	26.12	0.15
	Between group difference (follow-up)	1	0.57	[0.27, 0.88]	–	0.15	< 0.001***	–	–
Suicidal ideation	Within group change (post-intervention)	3	0.95	[0.80, 1.09]	–	0.08	< 0.001***	0.00	0.00
	Between group difference (post-intervention)	2	0.53	[0.08, 0.99]	–	0.23	0.021*	64.04	0.26
	Within group change (follow-up)	1	0.85	[0.57, 1.13]	–	0.14	< 0.001***	–	–
	Between group difference (follow-up)	1	0.20	[-0.24, 0.64]	–	0.23	0.375	–	–

Outcome	Comparison	*k*	Pooled/combined effect size					Heterogeneity	
			g	95% CI	Prediction interval	SE	*p*	I^2	τ
Self-harm	Within group change (post-intervention)	2	0.90	[0.60, 1.21]	–	0.16	< 0.001***	0.00	0.00
	Between group difference (post-intervention)	0	–	–	–	–	–	–	–
	Within group change (follow-up)	2	0.83	[0.15, 1.51]	–	0.35	0.017*	80.10	0.44
	Between group difference (follow-up)	1	0.49	[-0.18, 1.16]	–	0.34	0.148	–	–
BPD symptoms	Within group change (post-intervention)	6	1.02	[0.71, 1.33]	[0.00, 2.04]	0.16	< 0.001***	82.39	0.33
	Between group difference (post-intervention)	2	0.48	[0.04, 0.93]	–	0.23	0.034*	63.55	0.26
	Within group change (follow-up)	3	0.70	[0.46, 0.94]	[-1.98, 3.37]	0.12	< 0.001***	63.16	0.17
	Between group difference (follow-up)	3	-0.03	[-0.49, 0.43]	[-5.18, 5.12]	0.23	0.903	68.83	0.33

Note. *k* = number of studies contributing to pooled estimate; *g* = Hedges' *g* effect size; 95% CI = 95% confidence intervals; SE = standard error; *p* = significance value for *g*; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$; I^2 = proportional estimate of true effect variance over sampling error observed; τ = tau or standard deviation of underlying true effects across studies; BPD = Borderline Personality Disorder.

Table 5

Moderator analyses for DBT intensity

Subgroup	*k*	Pooled/combined effect size						Heterogeneity		
		g	95% CI	Prediction interval	SE	*p*	Q/Q_B	*p*	I^2	τ
High intensity	8	0.88	[0.80, 0.95]	[0.71, 1.05]	0.04	< 0.001***	10.50	0.162	33.36	0.06
Low intensity	3	0.76	[0.16, 1.37]	[-6.53, 8.06]	0.31	0.014*	10.97	0.004**	81.76	0.48
						Test for subgroup differences	**0.14**	**0.713**		

Note. *k* = number of studies contributing to pooled estimate; *g* = Hedges' *g* effect size; 95% CI = 95% confidence intervals; SE = standard error; *p* = significance value for *g* and *Q*; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$; *Q* = ratio of observed variation to within-study error; Q_B = between subgroups heterogeneity (based on ANOVA); I^2 = proportional estimate of true effect variance over sampling error observed; τ = tau or standard deviation of underlying true effects across studies.

Appendix A

Instructions to Authors

Types of article

All manuscripts must clearly and explicitly be of relevance to CBS. Articles should fall into one of six categories:

1. Empirical research (up to 6000 words)
2. Brief empirical reports (up to 3000 words)
3. Review articles (up to 10,000 words)
4. Conceptual articles (up to 6000 words)
5. Practical innovations (up to 6000 words)
6. Commentaries (up to 3000 words)
7. Registered reports (see instructions below)

Word limits exclude references, tables and figures but include the abstract.

Review articles. Manuscripts reviewing a wide range of topics are encouraged as long as their content is directly relevant to CBS. Systematic reviews and meta-analyses are particularly welcome. For meta-analyses and systematic reviews, JCBS requires submissions follow PRISMA guidelines (http://www.prisma-statement.org/).

Submission checklist

You can use this list to carry out a final check of your submission before you send it to the journal for review.

Ensure that the following items are present:

One Cover Page (with author details; if has been designated as the corresponding author with contact details:

- E-mail address
- Full postal address

All necessary files have been uploaded:

Title Page (with author details):

- Include title, names, affiliations, contact information, acknowledgments, author note indicating a data sharing statement (“Data is available upon reasonable request”) or study registration link to access data directly, and funding information.

Cover Page (with author details; if applicable): Location of shared data and materials (if applicable) Justifications for deviations to author guideline requirements (e.g., word length, data sharing author’s note, etc) Justifications for deviations to pre-registered analysis plan (if applicable) Clarification if the manuscript is based on previously published data (i.e., secondary analysis)

Manuscript (without author details):

- Include keywords
- All identifying author information removed
- Pre-registration identifier and location of registration (e.g., Clinicaltrials.gov NCTXXXXXX)
- Include a statement on **both** ethical approval and informed consent for research involving human subjects
- All figures (include relevant captions)
- All tables (including titles, description, footnotes)
- Indicate clearly if color should be used for any figures in print

Highlights

Conflict of Interest: Authors who are on the Journal of Contextual Behavioral Science editorial board must include an editor statement acknowledging their role.

Response to Reviewers (without author details; for resubmissions)

Further considerations

- Manuscript has been 'spell checked' and 'grammar checked'
- Manuscripts should be prepared in APA style (7th edition)
- All references mentioned in the Reference List are cited in the text, and vice versa
- Permission has been obtained for use of copyrighted material from other sources (including the Internet)
- A competing interests statement is provided, even if the authors have no competing interests to declare
- Journal policies detailed in this guide have been reviewed
- Referee suggestions and contact details provided, based on journal requirements

Authors should prepare their manuscript for double anonymized review, so that only the handling editors have access to author details. Authors must take special care to delete all potentially identifying information from any files that are not the Title Page with author details and the Cover Letter. Note: these two documents are submitted separately to the main manuscript. Any potential author identifying information including, but not limited to, name(s), affiliation(s), geographic location(s), identifying acknowledgments, author notes or funding details, should be removed from all other files. In-text citations to previous work by the authors should be presented in such a way that it is not clear that it was written by the same authors or should be removed for masking with a note (e.g., "citation removed for anonymized review"). For authors resubmitting revisions of manuscripts, please ensure that the "Response to reviewers" is also free from author identifying information. Manuscripts that are not appropriately anonymized will be rejected without a full content review, although in many cases authors will be Study and Analysis Registration to re-submit manuscripts without author identifying information. This process will, however, delay review and manuscript processing times and should be avoided if at all possible.

Study and Analysis Registration

A study is considered pre-registered if study details are registered in a repository prior to when the study began. Some examples of repository sites include ClinicalTrials.gov and Open Science

Framework, but there are others. For instructions on how to mask your registration details for peer-review, see “Double Anonymized Review” under Preparation.

For all pre-registered studies, authors are required to provide information on where to access it (such as trial registration number) in the manuscript. Deviations from the registration should be noted in the main manuscript (with no identifying information), as well as highlighted in the cover letter along with a justification for doing so.

Declaration of conflicts of interest

All authors must disclose any financial and personal relationships with other people or organizations that could inappropriately influence (bias) their work. Examples of potential conflicts of interest include employment, consultancies, stock ownership, honoraria, paid expert testimony, patent applications/ registrations, and grants or other funding. Authors must disclose any conflicts of interest (or lack thereof) as a separate conflict of interest document in their submission. If there are no interests to declare then please state this: ‘Declaration of conflicts of interest: none’. This summary statement will be ultimately published if the article is accepted.

Language (usage)

Please write your text in good English (American or British usage is accepted, but not a mixture of these).

Double anonymized review

This journal uses double anonymized review, which means the identities of the authors are concealed from the reviewers, and vice versa. More information is available on our website. To facilitate this, please include the following separately:

Title page (with author details): This should include the title, authors’ names, affiliations, acknowledgements and funding information, and a complete address for the corresponding author including an e-mail address.

Cover letter (with author details): This should include unanonymized registration details and note where to access this information (such as trial registration number). For authors that have a compelling reason, this should include justification for a registration exception or registration deviations. It is expected that all authors who publish in the Journal of Contextual Behavioral Science will share data upon reasonable request. Therefore, we ask authors who do not already have their data openly available to the public to include an author note indicating “Data is available upon reasonable request.” Authors can request to leave this note out if they can provide an adequately strong justification for not doing so in the cover letter.

Anonymized manuscript (no author details): The main body of the paper (including the references, figures, and tables) should be anonymized during the review process (i.e., no identifying information, such as the authors’ names or affiliations). When available, pre-registration information or shared data identifiers should also be listed in the Method section without identifiers. We recommend using text such as “The study was pre-registered at _____ (insert name of repository, trial identification number and/or link to study registration).” For those with deviations from the registration, author should also note this in the methods section. All anonymized information in the manuscript body will be asked to be unanonymized upon final acceptance of the submission.

Use of word processing software

It is important that the file be saved in the native format of the word processor used. The text should be in single-column format. Keep the layout of the text as simple as possible. Most formatting codes will be removed and replaced on processing the article. In particular, do not use the word processor's options to justify text or to hyphenate words. However, do use bold face, italics, subscripts, superscripts etc. When preparing tables, if you are using a table grid, use only one grid for each individual table and not a grid for each row. If no grid is used, use tabs, not spaces, to align columns. The electronic text should be prepared in a way very similar to that of conventional manuscripts (see also the Guide to Publishing with Elsevier). To avoid unnecessary errors you are strongly advised to use the 'spell-check' and 'grammar-check' functions of your word processor.

Article structure

Subdivision - unnumbered sections

Divide your article into clearly defined sections. Each subsection is given a brief heading. Each heading should appear on its own separate line. Subsections should be used as much as possible when crossreferencing text: refer to the subsection by heading as opposed to simply 'the text'.

Appendices

If there is more than one appendix, they should be identified as A, B, etc. Formulae and equations in appendices should be given separate numbering: Eq. (A.1), Eq. (A.2), etc.; in a subsequent appendix, Eq. (B.1) and so on. Similarly for tables and figures: Table A.1; Fig. A.1, etc.

Essential title page information

- *Title.* Concise and informative. Titles are often used in information-retrieval systems. Avoid abbreviations and formulae where possible.
- *Author names and affiliations.* Please clearly indicate the given name(s) and family name(s) of each author and check that all names are accurately spelled. You can add your name between parentheses in your own script behind the English transliteration. Present the authors' affiliation addresses (where the actual work was done) below the names. Indicate all affiliations with a lowercase superscript letter immediately after the author's name and in front of the appropriate address. Provide the full postal address of each affiliation, including the country name and, if available, the e-mail address of each author.
- *Corresponding author.* Clearly indicate who will handle correspondence at all stages of refereeing and publication, also post-publication. This responsibility includes answering any future queries about Methodology and Materials. Ensure that the e-mail address is given and that contact details are kept up to date by the corresponding author.
- *Present/permanent address.* If an author has moved since the work described in the article was done, or was visiting at the time, a 'Present address' (or 'Permanent address') may be indicated as a footnote to that author's name. The address at which the author actually did the work must be retained as the main, affiliation address. Superscript Arabic numerals are used for such footnotes.

Highlights

Highlights are mandatory for this journal as they help increase the discoverability of your article via search engines. They consist of a short collection of bullet points that capture the novel results of your research as well as new methods that were used during the study (if any).

Highlights should be submitted in a separate editable file in the online submission system. Please use 'Highlights' in the file name and include 3 to 5 bullet points (maximum 85 characters, including spaces, per bullet point).

Abstract

A concise and factual abstract is required. The abstract should state briefly the purpose of the research, the principal results and major conclusions. An abstract is often presented separately from the article, so it must be able to stand alone. For this reason, References should be avoided, but if essential, then cite the author(s) and year(s). Also, non-standard or uncommon abbreviations should be avoided, but if essential they must be defined at their first mention in the abstract itself.

Keywords

Immediately after the abstract, provide a maximum of 6 keywords, using American spelling and avoiding general and plural terms and multiple concepts (avoid, for example, 'and', 'of'). Be sparing with abbreviations: only abbreviations firmly established in the field may be eligible. These keywords will be used for indexing purposes.

Figure captions

Aim to use the following fonts in your illustrations: Arial, Courier, Times New Roman, Symbol, or use fonts that look similar.

Ensure that each illustration has a caption. Supply captions separately, not attached to the figure. A caption should comprise a brief title (**not** on the figure itself) and a description of the illustration. Keep text in the illustrations themselves to a minimum but explain all symbols and abbreviations used.

Tables

Please submit tables as editable text and not as images. In accordance with APA style, tables should be placed on separate page(s) at the end of the manuscript. Number tables consecutively in accordance with their appearance in the text and place any table notes below the table body. Be sparing in the use of tables and ensure that the data presented in them do not duplicate results described elsewhere in the article. Please avoid using vertical rules and shading in table cells.

References

Citation in text

Please ensure that every reference cited in the text is also present in the reference list (and vice versa). Any references cited in the abstract must be given in full. Unpublished results and personal communications are not recommended in the reference list. If these references are included in the reference list they should follow the standard reference style of the journal and should include a

substitution of the publication date with either ‘Unpublished results’ or ‘Personal communication’. Citation of a reference as ‘in press’ implies that the item has been accepted for publication.

Web references

As a minimum, the full URL should be given and the date when the reference was last accessed. Any further information, if known (DOI, author names, dates, reference to a source publication, etc.), should also be given. Web references can be listed separately (e.g., after the reference list) under a different heading if desired, or can be included in the reference list.

Data references

This journal encourages you to cite underlying or relevant datasets in your manuscript by citing them in your text and including a data reference in your Reference List. Data references should include the following elements: author name(s), dataset title, data repository, version (where available), year, and global persistent identifier. Add [dataset] immediately before the reference so we can properly identify it as a data reference. The [dataset] identifier will not appear in your published article.

Preprint references

Where a preprint has subsequently become available as a peer-reviewed publication, the formal publication should be used as the reference. If there are preprints that are central to your work or that cover crucial developments in the topic, but are not yet formally published, these may be referenced. Preprints should be clearly marked as such, for example by including the word preprint, or the name of the preprint server, as part of the reference. The preprint DOI should also be provided.

References in a special issue

Please ensure that the words ‘this issue’ are added to any references in the list (and any citations in the text) to other articles in the same Special Issue.

Appendix B

PRISMA Checklist (Page et al., 2021)

Section and Topic	Item #	Checklist item	Page(s) where item is reported
TITLE			
Title	1	Identify the report as a systematic review.	5
ABSTRACT			
Abstract	2	See the PRISMA 2020 for Abstracts checklist.	6
INTRODUCTION			
Rationale	3	Describe the rationale for the review in the context of existing knowledge.	8-13
Objectives	4	Provide an explicit statement of the objective(s) or question(s) the review addresses.	12-13
METHODS			
Eligibility criteria	5	Specify the inclusion and exclusion criteria for the review and how studies were grouped for the syntheses.	14-17, 19
Information sources	6	Specify all databases, registers, websites, organisations, reference lists and other sources searched or consulted to identify studies. Specify the date when each source was last searched or consulted.	14
Search strategy	7	Present the full search strategies for all databases, registers and websites, including any filters and limits used.	58-59
Selection process	8	Specify the methods used to decide whether a study met the inclusion criteria of the review, including how many reviewers screened each record and each report retrieved, whether they worked independently, and if applicable, details of automation tools used in the process.	14-17, 55
Data collection process	9	Specify the methods used to collect data from reports, including how many reviewers collected data from each report, whether they worked independently, any processes for obtaining or confirming data from study investigators, and if applicable, details of automation tools used in the process.	17
Data items	10a	List and define all outcomes for which data were sought. Specify whether all results that were compatible with each outcome domain in each study were sought (e.g. for all measures, time points, analyses), and if not, the methods used to decide which results to collect.	17-18
	10b	List and define all other variables for which data were sought (e.g. participant and intervention characteristics, funding sources). Describe any assumptions made about any missing or unclear information.	17-18
Study risk of bias assessment	11	Specify the methods used to assess risk of bias in the included studies, including details of the tool(s) used, how many reviewers assessed each study and whether they worked independently, and if applicable, details of automation tools used in the process.	18
Effect measures	12	Specify for each outcome the effect measure(s) (e.g. risk ratio, mean difference) used in the synthesis or presentation of results.	18-19

Section and Topic	Item #	Checklist item	Page(s) where item is reported
Synthesis methods	13a	Describe the processes used to decide which studies were eligible for each synthesis (e.g. tabulating the study intervention characteristics and comparing against the planned groups for each synthesis (item #5)).	18-19
	13b	Describe any methods required to prepare the data for presentation or synthesis, such as handling of missing summary statistics, or data conversions.	17
	13c	Describe any methods used to tabulate or visually display results of individual studies and syntheses.	18-20
	13d	Describe any methods used to synthesize results and provide a rationale for the choice(s). If meta-analysis was performed, describe the model(s), method(s) to identify the presence and extent of statistical heterogeneity, and software package(s) used.	18-20
	13e	Describe any methods used to explore possible causes of heterogeneity among study results (e.g. subgroup analysis, meta-regression).	20
	13f	Describe any sensitivity analyses conducted to assess robustness of the synthesized results.	N/A
Reporting bias assessment	14	Describe any methods used to assess risk of bias due to missing results in a synthesis (arising from reporting biases).	19
Certainty assessment	15	Describe any methods used to assess certainty (or confidence) in the body of evidence for an outcome.	19
RESULTS			
Study selection	16a	Describe the results of the search and selection process, from the number of records identified in the search to the number of studies included in the review, ideally using a flow diagram.	21, 55
	16b	Cite studies that might appear to meet the inclusion criteria, but which were excluded, and explain why they were excluded.	21
Study characteristics	17	Cite each included study and present its characteristics.	21-25, 60-61
Risk of bias in studies	18	Present assessments of risk of bias for each included study.	25-26, 62
Results of individual studies	19	For all outcomes, present, for each study: (a) summary statistics for each group (where appropriate) and (b) an effect estimate and its precision (e.g. confidence/credible interval), ideally using structured tables or plots.	63-64
Results of syntheses	20a	For each synthesis, briefly summarise the characteristics and risk of bias among contributing studies.	27-30
	20b	Present results of all statistical syntheses conducted. If meta-analysis was done, present for each the summary estimate and its precision (e.g. confidence/credible interval) and measures of statistical heterogeneity. If comparing groups, describe the direction of the effect.	27-30, 63-64
	20c	Present results of all investigations of possible causes of heterogeneity among study results.	30-31, 57, 65

Section and Topic	Item #	Checklist item	Page(s) where item is reported
	20d	Present results of all sensitivity analyses conducted to assess the robustness of the synthesized results.	N/A
Reporting biases	21	Present assessments of risk of bias due to missing results (arising from reporting biases) for each synthesis assessed.	27, 56
Certainty of evidence	22	Present assessments of certainty (or confidence) in the body of evidence for each outcome assessed.	27-31, 63-65
DISCUSSION			
Discussion	23a	Provide a general interpretation of the results in the context of other evidence.	32-33
	23b	Discuss any limitations of the evidence included in the review.	33-36
	23c	Discuss any limitations of the review processes used.	34-36
	23d	Discuss implications of the results for practice, policy, and future research.	33-37
OTHER INFORMATION			
Registration and protocol	24a	Provide registration information for the review, including register name and registration number, or state that the review was not registered.	14
	24b	Indicate where the review protocol can be accessed, or state that a protocol was not prepared.	14
	24c	Describe and explain any amendments to information provided at registration or in the protocol.	N/A
Support	25	Describe sources of financial or non-financial support for the review, and the role of the funders or sponsors in the review.	N/A
Competing interests	26	Declare any competing interests of review authors.	5
Availability of data, code and other materials	27	Report which of the following are publicly available and where they can be found: template data collection forms; data extracted from included studies; data used for all analyses; analytic code; any other materials used in the review.	N/A