Promotion of health, wellbeing and thriving among children and adolescents: shaping psychological mindsets and the use of third wave CBT approaches

Amorette M. Perkins *(Trainee Clinical Psychologist)*

Primary Supervisor: Dr Laura Pass *(Clinical Lecturer in Clinical Psychology)*

Secondary Supervisors: Professor Richard Meiser-Stedman *(Professor of Clinical Psychology)* and Dr Gemma Bowers *(Former Clinical Lecturer)*

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University of East Anglia

Faculty of Medicine and Health Sciences

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Abstract

Background: Currently, up to 20% of young people worldwide have a diagnosable mental health condition, and an even greater proportion have subclinical symptoms and/or are at risk of developing difficulties. Universal approaches to treatment, prevention, and the promotion of positive wellbeing for youth are of growing interest.

Method: A systematic review and meta-analysis was conducted to determine the effectiveness of third wave cognitive behavioural therapies (CBT) for children and adolescents. Therapies reviewed were transdiagnostic and applicable across the continuum from ill-health to thriving. An empirical study explored the feasibility of a randomised controlled trial (RCT) for a psychologically-based mindset intervention, incorporating constructs from third wave CBT, as a universal promotive mental health tool in UK educational settings.

Results: Thirty RCTs across clinical and non-clinical settings were included in the meta-analysis. When all studies were included, behavioural difficulties/externalising problems (g=-.67), third wave processes (g=.67), and wellbeing/flourishing (g=.65) yielded significant effects. When analysing only studies rated moderate-high quality, significant effects were found for emotional symptoms/internalising problems (g=-0.34), interference from difficulties (g=-0.82), third wave processes (g=0.53), wellbeing/flourishing (g=0.51), and quality of life (g=0.49). Behavioural difficulties/externalising problems ceased to be significant, while physical health/pain

was consistently non-significant. The empirical study suggested that the mindset intervention and research design were feasible. Minimum recruitment targets were met (*N*=80). Student feedback was positive and participants appeared to understand the intervention content. Data were indicative of possible intervention effects for primary outcomes of personality mindset and psychological flexibility. Secondary outcomes of self-compassion, self-esteem, low mood, and anxiety also yielded some promising results. Maintenance was difficult to evaluate due to sample attrition.

Conclusions: Third wave CBT and psychological mindset interventions may be applicable to improve mental health and/or promote thriving among young people. There were notable limitations to both papers. Implications and directions for future research were discussed.

Table of Contents

List of Tables

List of Figures

Bridging Chapter

Empirical Study

Additional Methodology

Additional Results

Discussion and Critical Evaluation

List of Appendices

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CHAPTER ONE

Introduction to the Thesis Portfolio

Introduction to the Thesis Portfolio

The World Health Organisation (WHO) suggests that up to 20% of children and adolescents worldwide have a clinically significant mental health difficulty at present (WHO, n.d.). An even greater proportion of young people experience sub-clinical symptoms and/or have been exposed to significant risk factors for developing a mental health condition (Public Health England [PHE] & Children and Young People's Mental Health Coalition [CYPMHC], 2015). Young people with mental health difficulties are at risk of reduced educational attainment and employment prospects; are more likely to have poorer social relationships, engage in criminal activity, smoke, and misuse drugs or alcohol; and have an increased risk of physical health problems and premature mortality in adulthood (UK Department of Health [DoH] & NHS England, 2015a; PHE, 2016).

Evidence suggests that chronic and severe mental illnesses in adulthood may be preventable with early intervention. Over half of mental health conditions in adult life develop by age 14 years and 75% by age 24 years (Kim-Cohen et al., 2003; Kessler et al., 2005). Even in the absence of mental illness, promoting positive wellbeing is vital to support young people to thrive and lead good, fulfilling lives. Indeed, engendering a state of flourishing enhances an individual's resilience and their ability to cope with stressors, improves physical health and life expectancy, and reduces health risk behaviours (PHE, 2015). Promoting psychological strengths, assets, and positive emotional experiences has widespread benefits for individuals, families, communities, and economies (Kobau et al., 2011).

Nonetheless, current evidence from the UK suggests that 70% of children and adolescents who experience mental health problems do not receive appropriate intervention at an early age (Children's Society, 2008). The number of referrals to child and youth mental health services is growing, waiting-times are lengthy, funding is scarce, and the interventions provided are of mixed quality (DoH & Department for Education [DfE], 2017). Moreover, whilst strategies have been proposed to promote wellbeing among children and adolescents across the general population, for example through educational settings, these are not yet commonplace or consistent (PHE & CYPMHC, 2015; DoH & DfE, 2017). Current barriers include a lack of evidence-based interventions and tools that are appropriate to deliver with youth at a public level (PHE & CYPMHC, 2015; White, Lea, Gibb & Street, 2017).

Improvements are required across the spectrum; from the treatment of mental health problems to engendering positive public wellbeing (PHE, 2019). There has been a call to explore universal approaches to mental health prevention and promotion in particular (DoH & DfE, 2017). A recent synthesis of systematic reviews identified eight promising interventions that could be widely applied to improve child mental health and wellbeing (PHE, 2019). The eight interventions were specific programmes: FRIENDS for Children/for life, LARS&LISA, the Penn Preventative/Resiliency Programme, Promoting Alternative Thinking Strategies, the Resourceful Adolescent Programme, Substance Abuse Risk Reduction, Triple P Online, and Zippy's Friends. These interventions share similar strategies, many of which were drawn from "second wave" cognitive behavioural therapies (CBT) (PHE, 2019).

Newer, "third wave" CBT methods are promoted for their more transdiagnostic approach and relevance across the continuum from ill-health to flourishing (Hayes & Hofmann, 2017), which could make them widely applicable within schools or communities as preventative or promotive strategies (Burckhardt, Manicavasagar, Batterham & Hadzi-Pavlovic, 2016). Nevertheless, third wave CBT interventions were not identified in the PHE investigation (2019) as potential universal approaches. This may be due to the absence of any systematic review to investigate the effectiveness of third wave CBT with children and adolescents in non-clinical settings. Despite the increasing popularity of third wave CBT as a treatment strategy in child and youth mental health services (O'Brien, Larson & Murrell, 2008), no meta-analytic review has been conducted to determine its effectiveness in clinical contexts either.

Another promising area for investigation that has not yet been identified by PHE (2019) are psychologically-based mindset interventions. Mindsets are defined as a set of fundamental beliefs, attitudes, or theories about aspects of the human condition (Ryan & Mercer, 2012). The mindset literature developed from studies investigating how beliefs regarding the malleability of intelligence affect learning and predict better attainment (Dweck, Chiu & Hong, 1995). However, there has since been a growing interest in mindsets of psychological factors such as personality, thoughts, and feelings, as they have been found to underlie psychopathology and mental wellbeing (e.g. Schleider, Abel & Weisz, 2015; Schroder, Dawood, Yalch, Donnellan & Moser, 2015, 2016).

As such, psychologically-based mindset interventions for children and adolescents have begun to emerge; for example, teaching young people that all personal characteristics are malleable (Schleider & Weisz, 2016; 2018). These interventions are in their infancy

and have a number of limitations, but have shown promising results at reducing symptoms and increasing resilience among youth with mental health difficulties (Schleider & Weisz, 2016; 2018). Psychological mindsets are universally relevant, so such interventions may serve as a promotive mental health strategy that could potentially be delivered in schools. However, this has not yet been fully investigated.

This thesis aimed to address some of the aforementioned gaps in the literature. A comprehensive systematic review and meta-analysis of third wave CBT with children and adolescents across both clinical and non-clinical settings is presented in Chapter Two. Chapter Four reports a feasibility study of a randomised controlled trial (RCT), testing a psychologically-based mindset intervention as a promotive strategy in UK sixth forms and colleges. Theoretical and contextual links between these chapters are further discussed in Chapter Three. Chapters Five and Six present additional methodology and results for both the meta-analysis and empirical study. Finally, Chapter Seven provides an integration of findings from both studies alongside a discussion of implications and directions for future research. Strengths and limitations of the thesis portfolio as a whole are also presented.

CHAPTER TWO

Systematic Review and Meta-Analysis

Prepared for submission to Clinical Psychology Review

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The Effectiveness of Third Wave Cognitive Behavioural Therapies for Children and Adolescents: A Systematic Review and Meta-Analysis

Amorette M. Perkins[a]*, Richard Meiser-Stedman[a], Gemma Bowers[b], Abigail G. Perkins[a], & Laura Pass[a]

[a]Department of Clinical Psychology and Psychological Therapies, University of East Anglia, Norwich Research Park, Norwich, Norfolk, NR4 7TJ, United Kingdom

[b]Norfolk and Suffolk NHS Foundation Trust, Mary Chapman House, Hotblack Road, NR2 4HN, Norfolk, United Kingdom

*Corresponding author: amorette.perkins@uea.ac.uk

+44 (0)1603 456 161

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Abstract

Third wave cognitive behavioural therapies (CBT) are increasingly used with children and adolescents, despite the lack of a strong evidence-base. This systematic review and meta-analysis aimed to determine the effectiveness of third wave CBT for a variety of outcomes related to psychological and physical symptoms, wellbeing, and functioning in youth. Moderation and subgroup analyses were conducted to explore heterogeneity. Further aims were to estimate effect sizes at follow-up, understand variation in effectiveness, and compare third wave CBT to existing psychological therapies. Thirty randomised controlled trials were included but many were rated as poor quality. With all trials included, only behavioural difficulties/externalising problems (g=-.67), third wave processes (g=.67), and wellbeing/flourishing (g=.65) yielded significant effects. Nevertheless, when analysing only those studies rated moderate-high quality, third wave CBT yielded significant superiority effects compared to controls for emotional symptoms/internalising problems (g=-0.34), interference from difficulties (g=-0.82), third wave processes (g=0.53), wellbeing/flourishing (g=0.51), and quality of life (g=0.49). Behavioural difficulties/externalising problems ceased to be significant, while physical health/pain was consistently non-significant. Widespread heterogeneity raises concerns about the generalisability of these findings. Moderation, subgroup, and follow-up analyses were limited by the availability of current research. Overall, results were promising and further high quality intervention trials are warranted.

Highlights

- We conducted a meta-analysis of third wave CBT trials for children and adolescents.
- Third wave CBT was effective for youth across a range of outcomes post-treatment.
- It remains unclear whether effects for third wave CBT are maintained at follow-up.
- Widespread heterogeneity raises queries about the generalisability of findings.
- Further high-quality trials are required, including follow-up assessments.

Keywords

Third Wave Interventions; Third Wave Cognitive Behavioural Therapy; Child and Adolescent Mental Health; Child and Adolescent Wellbeing

The Effectiveness of Third Wave Cognitive Behavioural Therapies for Children and Adolescents: A Systematic Review and Meta-Analysis

The term "third wave cognitive behavioural therapy (CBT)" refers to a group of psychological interventions that have been developed in recent years (Hayes, 2004). This third generation of CBT places more emphasis on facilitating change by altering a person's *relationship* with thoughts and emotions, as compared to earlier generations; the first being behavioural therapy, and the second being traditional cognitive behavioural therapy (Brown, Gaudiano & Miller, 2011). Third wave methods further differ to earlier CBT interventions in their more transdiagnostic approach, targeting common psychological processes relevant across the continuum from ill-health to flourishing, rather than specific models of disorder or disease (Hayes & Hofmann, 2017). Consequently, third wave therapies have gained increasing interest over the past twenty years and have been applied across various diagnostic classifications, whilst also in non-clinical settings to enhance day-to-day mental health and wellbeing (Gilbert, 2010; Hayes & Ciarrochi, 2015; Hayes & Hofmann, 2017).

Several therapies have been classified under the umbrella term of "third wave" approaches, including: Acceptance and Commitment Therapy (ACT; Hayes, Strosahl & Wilson, 1999), Cognitive Behavioural Analysis System of Psychotherapy (CBASP; McCullough, 2000), Compassion Focused Therapy (CFT; Gilbert, 2010), Dialectical Behaviour Therapy (DBT; Linehan, 1993), Functional Analytic Psychotherapy (FAP; Kohlenberg & Tsai, 1991), Mindfulness-Based Cognitive Therapy (MBCT; Segal, Williams & Teasdale, 2002), Mindfulness-Based Stress Reduction (MBSR; Kabat-Zinn,

1982), Metacognitive Therapy (MCT; Wells, 2009), and Schema Therapy (Young, 1990). There has been much debate over which therapies should be categorised as third wave CBT, and which lie more comfortably in "adapted CBT" or "integrative" psychological approaches.

Whilst there is no definitive agreement, the current article considered meta-cognitive, mindfulness, and acceptance-based interventions, utilising both cognitive and behavioural techniques that are applicable across diagnostic classifications and along the continuum from ill-health to flourishing, as key interests when investigating third wave CBT for children and adolescents. To this end, ACT, CFT, MBCT, and MCT were included. These therapies have many common methods and processes, including meta-cognition, mindfulness, acceptance, decentering, self-compassion, values-focused behaviour, and perspective taking (Brown, Gaudiano & Miller, 2011; Neff & Tirch, 2013). CBASP, DBT, FAP, and Schema Therapy were excluded given their unique interpersonal and/or psychoanalytic focus, whilst MBSR was excluded for a lack of cognitive behavioural techniques beyond mindful meditation.

Over the past few decades, third wave CBT has become increasingly popular. Recent meta-analyses suggest that it is effective across various adult populations for both reducing pathology and promoting positive wellbeing (e.g. Chiesa & Serretti, 2011; Normann, van Emmerisk & Morina, 2014; A-tjak et al., 2015; Kirby, Tellegen & Steindl, 2017). Many have argued that third wave CBT can also be applied to children and adolescents (Greco & Hayes, 2008). The processes targeted in third wave CBT (e.g. compassion, mindfulness) are predictive of trajectories and outcomes for young people (e.g. Bluth & Blanton, 2014), suggesting it is theoretically applicable. Moreover, whilst

third wave CBT can require complex cognitive skills, it also entails experiential compared to didactic learning; this makes it potentially accessible to children and young people (O'Brien, Larson & Murrell, 2008).

Research has emerged that explores the use of ACT, CFT, MBCT, and MCT with children and adolescents, across physical (e.g. Wicksell, Melin, Lekander & Olsson, 2009) and mental health settings (e.g. Makki, Hill, Bounds, McCammon, McFall-Johnsen & Delaney, 2018). Various conditions or presentations have been explored amongst youth, including pain (e.g. Wicksell et al., 2009), diabetes (e.g. Moazzezi, Moghanloo, Moghanloo & Pishvaei, 2015), substance use (e.g. Thurstone, Hall, Timmerman & Emrick, 2017), anxiety (e.g. Esbjorn, Normann, Christiansen & Reinholdt-Dunne, 2015), depression (e.g. Livheim et al., 2015), ADHD (e.g. Haydicky, Shecter, Wiener & Ducharme, 2015), Autism (e.g. Pahnke, Lundgren, Hursti & Hirvikoski, 2014), learning difficulties (e.g. Veysi, Rostami, Zangooi & Beldachi, 2015), acquired brain injury (e.g. Brown, Whittingham, Boyd, McKinlay & Sofronoff, 2014), and trauma (e.g. Bowyer, Wallis & Lee, 2014). Third wave CBT has also been investigated in general school samples as a method of promoting wellbeing and flourishing (e.g. Burckhardt, Manicavasagar, Batterham & Hadzi-Pavlovic, 2016).

In line with this research, there are a growing number of clinical manuals available for third wave CBT with children and adolescents (e.g. Semple & Lee, 2011; Hayes & Ciarrochi, 2015). In some settings, third wave CBT is being incorporated with, or even displacing, pre-existing therapeutic approaches (Horowitz, 2014). Yet, there are concerns that clinicians are getting "ahead of the data" (Corrigan, 2001). Much of the existing research to support third wave CBT with children and adolescents is of low quality, such

as from uncontrolled studies with small samples (e.g. Hetrick, Cox, Witt, Bir & Merry 2016). Many have used low grade interventions, for example, that are not manualised, comprehensive, or developmentally-adapted (e.g. Hayes & Ciarrochi, 2015), making it further difficult to evaluate third wave CBT. Results have been heterogenous and it is not clear whether any post-treatment effects are maintained over time (e.g. Fjorback, Arendt, Ornbol, Fink & Walach, 2011).

Whilst a handful of reviews exist in this area, it has been difficult to draw firm conclusions for a number of reasons. Firstly, reviews have focused on particular populations and outcomes (e.g. Kanstrup, Kemani, Holmstrom & Olsson, 2015). It has therefore been impossible to assess third wave CBT as a transdiagnostic approach. It also remains unclear whether third wave CBT is effective across development (i.e. childhood and adolescence) and for varied outcomes from symptom reduction to flourishing (O'Brien, Larson & Murrell, 2008). Secondly, existing reviews have not included multiple types of third wave CBT (e.g. Swain et al., 2015) and thus differences between them with regard to effectiveness has not been explored. Whilst third wave CBT approaches share similarities, there are also arguable differences; for example, in the degree they pathologise difficult psychological experiences and are problem- as opposed to value-focused. There is also variation within the delivery of third wave CBT that requires investigation (e.g. group versus individual therapy, through child, parent, or joint sessions).

Thirdly, no existing review of third wave CBT for children and adolescents incorporates meta-analytic techniques. Such therapies have only been included in meta-analyses alongside other types of therapy for youth, like traditional CBT or mindfulness-

only interventions, and so the effectiveness of third wave approaches specifically remains unknown (Hetrick et al., 2016; Zenner, Herrnleben-Kurz & Walach, 2014; Kallapiran, Koo, Kirubakaran & Hancock, 2015; Zoogman, Goldberg, Hoyt & Miller, 2015; Caldwell et al., 2019). There are no known existing meta-analyses to explore ACT, CFT, MBCT, or MCT separately, nor combine and compare these therapies, for child and adolescent populations. Further, no meta-analyses have explored how third wave CBT performs among youth in comparison to inactive versus active controls, such as pharmacology or earlier waves of CBT (e.g. Swain, Hancock, Dixon & Bowman, 2015).

Given the increasing popularity of third wave CBT for this age range across clinical and non-clinical settings, it is essential to establish effectiveness by pooling data from randomised controlled trials (RCTs) within meta-analyses. The primary aim of this review was to use a meta-analytic approach to determine the effectiveness of third wave CBT for children and adolescents for the following range of outcomes: 1) emotional symptoms/internalising problems, 2) behavioural difficulties/externalising problems, 3) interference from (emotional or physical) difficulties, 4) third wave processes (e.g. acceptance/mindfulness/self-compassion), 5) wellbeing/flourishing, 6) quality of life, and 7) physical health/pain. The impact of study quality was also assessed. Secondary aims were to: 1) explore variation in effectiveness amongst types of third wave CBT, settings, populations, control conditions, and formats of delivery (e.g. group versus individual therapy); 2) determine effectiveness for specific outcomes of depression, anxiety, acceptance, and mindfulness; 3) estimate effect sizes at follow-up; and 4) compare third wave CBT to alternative psychological therapies.

Method

A protocol for this review was preregistered with PROSPERO (CRD42019156796).

Literature search

PsycINFO, PubMed, Scopus, and the Cochrane CENTRAL Trials Register were searched from inception to November 11th 2019 (initial searches were conducted in April 2019 then updated in November 2019). The search strategy was: ("acceptance and commitment therapy" OR "compassion focus* therapy" OR "compassionate mind training" OR "mindfulness based cognitive therapy" OR "metacognitive therapy") OR (("third wave" OR "new wave") AND therap*) AND ("child*" OR "adolescen*" OR "teen*" OR "parent*" OR "school" OR "youth*" OR "young people"). The first 200 results of both Google Scholar and a university library database were also searched.

Inclusion criteria comprised the following: 1) primary empirical studies that used a randomised controlled design (individual or cluster randomisation); 2) investigating ACT, CFT, MBCT, or MCT (including where these therapies were used in combination with other approaches) compared to a control group (which could be: no intervention, waitlist, or treatment as usual/an active intervention, as long as it was not one of the included third wave therapies); 3) with at least one outcome measure for children and adolescents under 18 years old; 4) that offered sufficient data in the paper (or by contacting authors) to calculate effect sizes required for meta-analysis, of at least one outcome; and 5) were reported in an English language within a peer-reviewed journal.

This review included studies conducted in any setting (e.g. schools, general hospitals, or mental health clinics), using any outcome related to physical or mental health, wellbeing, or functioning (e.g. depression, pain, acceptance, school behaviour). The outcomes studied were intentionally broad given that the chosen forms of third wave CBT were promoted as transdiagnostic and relevant to thriving as well as pathology (Hayes & Hofmann, 2017). Diagnosis/presentation or mode of delivery (e.g. face-to-face, online, telephone) did not serve as exclusion criteria. Interventions were included whether they were delivered to children and/or via parents/carers/significant others, as long as the child was the reason for accessing the intervention and there was a child-focused outcome measure.

Third wave interventions that were not consistently classified as CBT in previous research were excluded; for example, integrative third wave psychodynamic or analytical psychotherapies, and mindfulness-only interventions (O'Brien, Larson & Murrell, 2008; O'Connor, Munnelly, Whelan & McHugh, 2018). CBASP, DBT, FAP, MBSR, and Schema Therapy were among those excluded.

Eligible studies

The initial search produced 1373 results after duplicates were removed, plus there were an additional 400 records from alternative sources. The primary author (AMP) screened titles and abstracts for eligibility. Full-text articles of 233 potentially eligible studies were retrieved and examined against inclusion and exclusion criteria. Any uncertainties regarding eligibility at this stage were resolved by discussion with a second or third reviewer (AGP/LP). Thirty papers met inclusion criteria. When the searches were

re-run in November 2019, 142 new results were retrieved; 24 were assessed at full-text and two were eligible for inclusion. This resulted in a total of 34 papers, describing 30 studies (Figure 2.1).

Figure 2.1: Flow diagram of searches

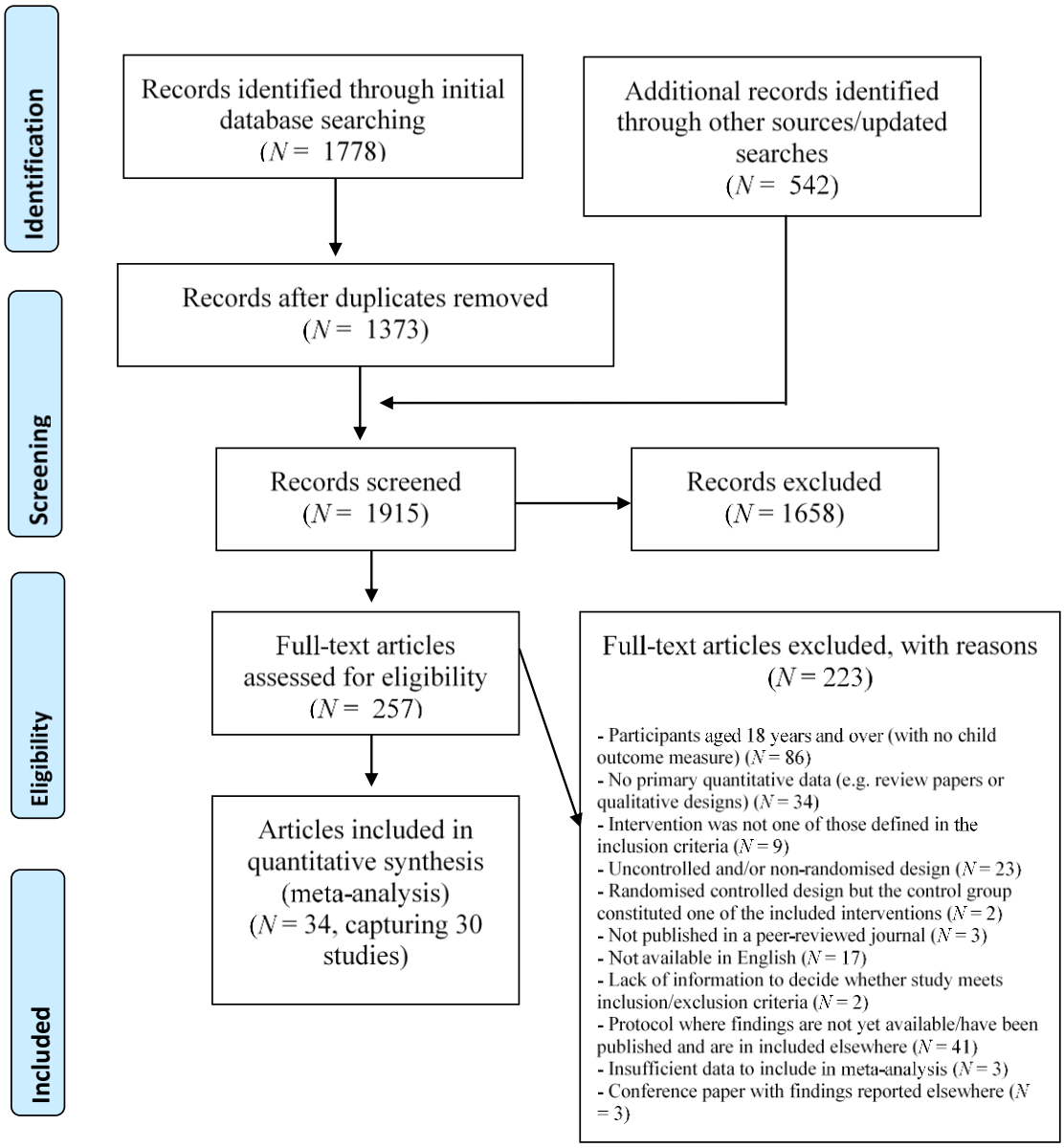

Figure 2.1. Diagram adapted from PRISMA, detailing flow of studies retrieved from searches through to inclusion. *N* = Number of articles.

Data extraction

The primary author (AMP) extracted demographic and methodological information using a pre-piloted table (see supplementary material).

Data for meta-analyses were also extracted following pre-determined rules: 1) post-intervention data were used in the primary analyses; 2) where data fitted with primary and/or secondary outcomes investigated by this review, it was extracted accordingly, regardless of whether the outcome was the primary target for the RCT; 3) where there were multiple measures within a trial for a single outcome, a pre-determined procedure was used to select one (see Appendix B); 4) follow-up data were extracted separately to post-intervention data (if multiple follow-ups were completed, the furthest time point was chosen); 5) if there were multiple comparison groups, a non-active control was chosen for the primary analyses in the first instance, followed by a non-psychological then psychological intervention, given the primary research aim was to determine the effectiveness of third wave CBT, not to compare it to other interventions (a separate, additional pool of data was extracted for secondary analyses comparing third wave CBT to other psychological interventions, which had to be recognised and specific, like first or second wave CBT); 6) data from intention-to-treat samples were included in analyses as preference, followed by data from subsets (e.g. assessment/treatment completers).

Where there were any uncertainties during extraction, they were resolved through the involvement of a second reviewer (AGP).

Quality assessment

Study quality was assessed using the Cochrane risk-of-bias tool for randomised trials (Sterne et al., 2019; Version 2), supplemented with extended items from the NICE quality appraisal checklist for quantitative intervention studies (NICE, 2012). These additional items focused on quality of reporting, sampling (including generalisability and power), and the intervention quality (namely, whether third wave CBT was manualised, comprehensive, developmentally adapted, and/or distinct rather than combined with non-relevant interventions). Intervention quality was deemed especially important to consider *a priori,* given there are concerns that third wave CBT has often been poorly applied to children and adolescents (Hayes & Ciarrochi, 2015), and the need to consider the impact of this in a review and meta-analysis aiming to determine effectiveness. The Cochrane tool for cluster-randomised designs was used where appropriate (Eldridge et al., 2016). All papers were assigned quality ratings by two independent reviewers (AMP and AGP); where there were discrepancies, agreement was reached through discussion.

See supplementary material for the quality assessment tool.

Data analysis

A meta-analysis was conducted to estimate effect sizes of third wave CBT at post-intervention for each of the seven primary outcomes. The impact of study quality and cluster randomised trials was assessed with sensitivity analyses. As per protocol, the outcome categories were decided collaboratively by the research team prior to data extraction, based on clinical knowledge and literature (see Appendix C).

Analyses were conducted using Meta-Analysis via Shiny, which applies R programming language (MAVIS Version 1.1.3; Hamilton, Aydin, Mizumoto, Coburn & Zelinsky, 2017). Between-group means and standard deviations or effect sizes were entered into random-effects models to account for heterogeneity; for example, resulting from intervention, sampling, and measurement differences. Effect sizes of 0.2, 0.5, and 0.8 were interpreted as small, moderate, and large, respectively (Fritz, Morris & Richler, 2012). I^2 was used to estimate the percentage of heterogeneity between studies that were not attributable to random sample error alone; values of 0%, 25%, 50% and 75% reflected nil, low, moderate, and high levels of heterogeneity, respectively. Heterogeneity was also examined using the *Q*-statistic; if significant ($p < .05$), it indicated that heterogeneity exceeded that expected by chance alone.

Heterogeneity was subsequently explored using moderation and subgroup analyses. Moderation was tested using the *Qb* statistic, which is the level of variation explained by a covariate. Subgroup analyses were important to interpret any significant moderation effects. Subgroup analyses were still conducted when moderation was non-significant, as high levels of heterogeneity within each subgroup could have led to a non-significant moderation effect, even when subgroups vastly differed with regard to average effect size. To maintain reliability, moderation and subgroup analyses were not conducted if there were fewer than four studies per group. To correct for multiple comparisons, the Holm-Bonferroni method was used for all moderator and subgroup analyses (Holm, 1979).

Moderators were: 1) type of third wave CBT (ACT versus MBCT only, due to insufficient comparators for the other therapies); 2) setting (clinical [physical or mental

health settings] versus non-clinical [i.e. school or community, e.g. summer camps]); 3) type of control condition (active versus inactive); 4) participant age group (child versus adolescent); 5) delivery format (group versus individual therapy); and 6) parental involvement (child-only versus joint/parent sessions). Further subgroup analyses were conducted to explore differences between clinical physical and mental health settings where there were sufficient studies.

Secondary analyses were conducted to explore: 1) specific outcomes of depression, anxiety, acceptance, and mindfulness; 2) effects at follow-up; and 3) studies comparing third wave CBT to alternative psychological therapies. Moderation and subgroup analyses were conducted for follow-up data where there were a sufficient number of studies (>4 per subgroup).

Funnel plots were created and publication bias was assessed in two steps. First, rank correlation tests for asymmetry were performed; a high and significant correlation ($p < .05$) indicated that the funnel plot was asymmetric and thus there was potential for bias. Second, visual inspection and trim-and-fill methods were used to estimate whether there were any missing studies that account for significant asymmetric distribution (Higgins & Green, 2011).

Results

Sample size and characteristics

Thirty studies were included, comprising 3179 participants (see supplementary material for a table of included studies). Sample sizes ranged from 11 - 586, with a median

of 59.5 (*IQR* 33 - 96.5). Studies were published between 2006 and 2019. Nineteen investigated ACT, eight investigated MBCT, two investigated CFT, and one investigated MCT. Intervention duration ranged from a single, 30 minute session to 24 hours of therapy over 12 weeks. Twenty-two studies were group interventions and eight comprised individual therapy. Twenty-one delivered the intervention directly with the child or adolescent, four with parent/carers only, and five with both the parent/carer and young person.

Thirteen studies utilised an inactive control group (no intervention/waitlist), 12 used an active control group (namely treatment as usual or alternative interventions), and five made multiple comparisons (three compared to both an inactive and active control group [data for the inactive controls were extracted for primary analyses], and two compared to two different active conditions). Six studies had a control group comprising a specific, alternative psychological therapy, which were: second wave CBT (2), second wave CBT plus SSRI medication (1), second wave CBT (plus family therapy for one participant) (1); cognitive restructuring from second wave CBT (1), and narrative exposure and response prevention (1).

Interventions were delivered in schools or communities (19), clinical physical health settings (7), and clinical mental health settings (4). Studies were conducted across various countries, including: Iran (9), Australia (7), Sweden (4), Belgium (3), China (2), USA (2), Finland (1), Germany (1), and the Philippines (1). Populations or conditions studied were related to: physical health (7), anxiety/stress (7), depression (4), behaviour (2), family/care situations (2), learning/neurodevelopmental difficulties (2), and mixed internalising and/or externalising difficulties (2). The remaining studies (4) were

conducted with general samples (e.g. exploring interventions as preventative or promotive wellbeing strategies). The age of participants ranged from 2 – 18 (*M* = 12.70*, SD* = 3.54); although, these statistics do not include four studies in which average age was unreported. Fourteen studies collected follow-up data in addition to post-intervention effects; follow-up length ranged from 1 – 24 months (*Mdn* = 4.5, *IQR* 2 – 6.5).

Study quality and attrition

Seven studies were rated as high quality, 10 moderate quality, and 13 low quality. Of note, only four studies were rated highly with regard to quality of the intervention. See the table of included studies in the supplementary material for further details.

Five studies did not report sample attrition; of those that did, dropout rates ranged from 0 - 85.29% at the last point of data collection, with a median of 17% (*IQR* 6.96 – 22.52). Attrition at follow-up specifically was 18.65% (*IQR* = 7.67 – 46.17). Half of the studies did not specify whether data represented all participants randomised at outset or only a subset (e.g. assessment/treatment completers); for these papers, it was assumed that data represented the completer sample for each time point. For 13 studies, the intention-to-treat sample was extracted. For two studies, there was no attrition and thus a complete sample.

Meta-analysis findings

Primary outcomes

Main effects for the primary outcomes at post-intervention are presented in Table 2.1. Sixteen studies at post-intervention compared to inactive controls while the remainder

used active conditions (see Table 2.1 for a breakdown per outcome). Active comparisons included: alternative psychological interventions (4), school pastoral/counsellor/nurse support (4), broad "treatments as usual", such as multi-disciplinary care (2), educational/activity-based groups (2), medication (1), and the usual school curriculum (1). Overall, significant and medium-sized effects were found favouring third wave CBT for behavioural difficulties/externalising problems, third wave processes, and wellbeing/flourishing; non-significant effects were observed for other primary outcomes. For all variables, there was significant heterogeneity.

Impact of study quality. Sensitivity analyses excluding low quality studies were performed and are presented alongside main effects in Table 2.1. Results indicated that study quality had a substantial impact on many outcome variables. Emotional symptoms/internalising problems, interference from difficulties, and quality of life now yielded small-large significant effects. Third wave processes and wellbeing/flourishing remained significant and continued to yield moderate effect sizes. Behavioural difficulties/externalising problems ceased to be significant, while physical health/pain was consistently non-significant.

Effects of cluster randomised trials. There was no considerable change, other than slightly raised effect sizes, when studies using cluster randomisation techniques were excluded (see supplementary material).

Moderation and subgroup analyses. Moderation with subgroup analyses are presented to aid interpretation of heterogeneity (Table 2.1). Although, these should be interpreted with caution as studies rated as low quality were included (excluding low

quality research would have reduced the number of available comparators, making most moderation and subgroup analyses impossible).

Table 2.1

Main effects with moderation and subgroup analyses for the primary outcome variables

		k	*g*	95% CI	p-value	Heterogeneity I^2 (*Q with p-value*)
Emotional symptoms and internalising problems *(A=13, I=13)*						
Overall effect (*N* = 2355)		26	-0.26	-0.63 to 0.12	.185	95% (155.87, <.001)
Excluding low quality studies (*N* =1640)		**16**	**-0.34**	**-0.54 to -0.15**	**<.001**	67% (41.87, <.001)
Moderators	Subgroups[1]					
Intervention type (*Qb* = 0.84, *p* = .358)	ACT	16	-0.38	-0.69 to -0.06	.019***	84% (94.74, <.001)
	MBCT	7	-0.11	-0.57 to 0.35	.626	89% (54.52, <.001)
Setting (*Qb* = 9.75, *p* = .002)	**Clinical**	**10**	**-0.73**	**-1.10 to -0.35**	**< .001**	83% (54.23, <.001)
	PH	6	-1.01	-1.72 to -0.29	.006***	88% (42.43, <.001)
	MH	4	-0.40	-1.29 to 0.49	.381	74% (11.74, .008)
	Non-clinical	16	0.01	-0.26 to 0.28	.929	81% (78.97, <.001)
Control condition (*Qb* = 2.12, *p* = .145)	Active	13	-0.07	-0.40 to 0.25	.652	75% (48.37, <.001)
	Inactive	13	-0.42	-0.75 to -0.09	.013***	87% (92.35, <.001)
Delivery (*Qb* < 0.00, *p* = .967)	Individual	8	-0.25	-0.70 to 0.20	.271	77% (29.82, <.001)
	Group	18	-0.24	-0.52 to 0.04	.094	86% (125.59, <.001)
Parental involvement (*Qb* =0.20, *p* = .656)	Child-only	18	-0.21	-0.49 to 0.07	.146	87% (135.30, <.001)
	Parents involved	8	-0.32	-0.75 to 0.10	.134	56% (15.84, .027)
Behavioural difficulties and externalising problems *(A=5, I=10)*						
Overall effect (*N* = 1313)		**15**	**-0.67**	**-1.13 to -0.21**	**.004**	93% (110.23, <.001)
Excluding low quality studies (*N* = 1080)		9	-0.38	-0.87 to 0.12	.134	92% (59.08, <.001)
Moderators	Subgroups					
Intervention type (*Qb* = 0.36, *p* = .547)	ACT	9	-0.60	-1.12 to -0.07	.027***	81% (43.13, <.001)
	MBCT	5	-0.87	-1.58 to -0.16	.017***	94% (66.52, <.001)
Setting (*Qb* = 0.47, *p* = .492)	Clinical	5	-0.80	-1.41 to -0.19	.011***	72% (14.22, .007)

		k	*g*	95% CI	p-value	Heterogeneity I^2 (*Q with p-value*)
	Non-clinical	10	-0.54	-0.96 to -0.11	.014***	89% (82.37, <.001)
Control condition (*Qb* = 1.05, *p* = .306)	Active	5	-0.38	-0.94 to 0.17	.178	90% (39.67, <.001)
	Inactive	**10**	**-0.75**	**-1.18 to -0.33**	**< .001**	80% (44.96, <.001)
Delivery	Individual	-	-	-	-	
	Group	-	-	-	-	
Parental involvement (*Qb* = 0.69, *p* = .406)	**Child-only**	**10**	**-0.75**	**-1.22 to -0.29**	**.002**	91% (100.87, <.001)
	Parents involved	5	-0.42	-1.05 to 0.21	.195	44% (7.15, .128)
Interference from difficulties *(A=9, I=3)*						
Overall effect (*N* = 1392)		12	-0.58	-1.23 to 0.07	.080	96% (228.98, <.001)
Excluding low quality studies (*N* = 1294)		**9**	**-0.82**	**-1.55 to -0.10**	**.027**	97% (209.51, <.001)
Moderators	Subgroups					
Intervention type	ACT	-	-	-	-	
	MBCT	-	-	-	-	
Setting (*Qb* = 15.73, *p* < .001)	**Clinical**	**5**	**-1.52**	**-2.13 to -0.92**	**< .001**	95% (75.68, <.001)
	Non-clinical	7	0.07	-0.44 to 0.57	.792	60% (15.13, .019)
Control condition[2]	Active	-	-	-	-	
	Inactive	-	-	-	-	
Delivery	Individual	-	-	-	-	
	Group	-	-	-	-	
Parental involvement (*Qb* = 7.04, *p* = .008)	Child-only	6	0.07	-0.61 to 0.75	.839	66% (14.80, .011)
	Parents involved	**6**	**-1.23**	**-1.91 to -0.55**	**< .001**	96% (124.32, <.001)
Third wave processes *(A=6, I=5)*						
Overall effect (*N* = 1234)		**11**	**0.67**	**0.32 to 1.01**	**<.001**	85% (49.94, <.001)
Excluding low quality studies (*N* =1179)		**9**	**0.53**	**0.22 to 0.83**	**<.001**	80% (34.19, <.001)
Moderators	Subgroups					
Intervention type (*Qb* = 0.51, *p* = .476)	**ACT**	**6**	**0.78**	**0.32 to 1.24**	**.001**	88% (43.37, <.001)

		k	*g*	95% CI	p-value	Heterogeneity I^2 (*Q with p-value*)
	MBCT	4	0.52	-0.02 to 1.06	.061	42% (5.19, .158)
Setting	Clinical	-	-	-	-	
	Non-clinical	-	-	-	-	
Control condition (*Qb* = 0.32, *p* = .596)	**Active**	**6**	**0.56**	**0.19 to 0.94**	**.004**	87% (37.61, <.001)
	Inactive	**5**	**0.73**	**0.29 to 1.17**	**.001**	0% (1.10, .900)
Delivery	Individual	-	-	-	-	
	Group	-	-	-	-	
Parental involvement	Child-only	-	-	-	-	
	Parents involved	-	-	-	-	
Wellbeing and flourishing *(A=4, I=8)*						
Overall effect (*N* = 713)		**12**	**0.65**	**0.22 to 1.08**	**.003**	85% (61.78, <.001)
Excluding low quality studies (*N* = 601)		**9**	**0.51**	**0.09 to 0.92**	**.017**	80% (37.74, <.001)
Moderators	Subgroups					
Intervention type	ACT	-	-	-	-	
	MBCT	-	-	-	-	
Setting (*Qb* = 3.01, *p* = .083)	**Clinical**	**5**	**1.03**	**0.45 to 1.60**	**< .001**	87% (31.64, <.001)
	Non-clinical	7	0.37	-0.09 to 0.83	.118	57% (13.93, .031)
Control condition (*Qb* = 0.03, *p* = .864)	Active	4	0.60	-0.09 to 1.28	.088	98% (29.59, <.001)
	Inactive	8	0.67	0.16 to 1.18	.010***	74% (26.45, <.001)
Delivery	Individual	-	-	-	-	
	Group	-	-	-	-	
Parental involvement (*Qb* = 0.18, *p* = .670)	Child-only	7	0.73	0.16 to 1.29	.012***	84% (36.98, <.001)
	Parents involved	5	0.54	-0.12 to 1.20	.107	84% (24.52, <.001)
Quality of life *(A=8, I=5)*						
Overall effect (*N* = 946)		9	0.87	-0.08 to 1.82	.071	97% (71.66, <.001)
Excluding low quality studies (*N* = 877)		**7**	**0.49**	**0.16 to 0.83**	**.004**	74% (23.28, <.001)

		k	*g*	95% CI	p-value	Heterogeneity I^2 (*Q with p-value*)
Moderators	Subgroups					
Intervention type	ACT	-	-	-	-	
	MBCT	-	-	-	-	
Setting (*Qb* = 4.04, *p* = .044)*	**Clinical**	**4**	**1.33**	**0.57 to 2.08**	**< .001**	93% (40.76, <.001)
	Non-clinical	5	0.31	-0.34 to 0.95	.351	65% (11.54, .021)
Control condition (*Qb* = 7.61, *p* = .006)	Active	4	0.05	-0.59 to 0.70	.875	29% (4.20, .240)
	Inactive	**5**	**1.31**	**0.69 to 1.93**	**< .001**	90% (38.13, <.001)
Delivery	Individual	-	-	-	-	
	Group	-	-	-	-	
Parental involvement (*Qb* = 1.27, *p* = .260)	**Child-only**	**5**	**1.11**	**0.29 to 1.93**	**.008**	93% (55.17, <.001)
	Parents involved	4	0.42	-0.45 to 1.29	.341	81% (15.47, .002)
Physical health and pain^ *(A=3, I=3)*						
Overall effect (*N* = 935)		6	1.01	-0.07 to 2.09	.068	98% (166.22, <.001)
Excluding low quality studies (*N* = 903)		5	0.71	-0.38 to 1.80	.203	98% (145.17, <.001)

Note: *A* = number of active controls in the main analysis for overall effect; *I* = number of inactive controls in the main analysis for overall effect; *N* = participants included in analysis (based on intention-to-treat samples where available); k = number of studies in subgroup; *g* = Hedges' *g*; CI = confidence interval; p-val = significance; where "-" is observed, moderation and subgroup analyses were not possible due to an insufficient number of studies (< 4) per subgroup; significant moderators and subgroups yielding a superiority effect for third wave CBT are denoted in bold (alphas adjusted per variable to account for multiple comparisons)

[1]CFT and MCT were excluded from all moderation and subgroup analyses of intervention type given that there were an insufficient number of studies (< 4) investigating these types of third wave CBT. There were also too few comparators to explore participant age (<4 for children versus adolescents) as a moderator variable for any outcome. It was not possible to compare physical and mental health settings due to the limited number (<4) of studies for all variables except emotional symptoms/internalising problems

^No moderator or subgroup analyses could be performed, due to insufficient studies (<4). All studies for this outcome investigated ACT interventions

*Whilst p <.05, this was non-significant following correction using the Holm-Bonferroni method

Secondary analyses

Secondary outcomes. Main effects and sensitivity analyses for the specific outcomes of depression, anxiety, acceptance, and mindfulness are presented in Table 2.2. When all studies were included, a significant and medium-sized effect was found for mindfulness; all other effects were non-significant. When studies rated as low quality were excluded, depression and anxiety also yielded significant, small effects. There was significant heterogeneity for all variables.

Effects at follow-up. Main effects with moderation and subgroup analyses at follow-up are presented in Table 2.3. For interference from difficulties, quality of life, and physical health/pain, all studies used an active control, while for the other outcomes, 55-80% of studies used active comparisons (see Table 2.3 for a breakdown per outcome). No significant overall effects favouring third wave CBT were observed. There was significant heterogeneity for six of seven outcome variables. There were an insufficient number of studies to conduct the planned moderator/subgroup analyses, except those presented in the table.

Comparison to other psychological therapies. For emotional symptoms and internalising problems, there was a non-significant, negligible difference to alternative psychological therapies (k = 6, g = 0.12, 95% CI -0.48 to 0.71, p = .701). For interference from difficulties, there was a non-significant but small superiority effect for third wave CBT (k = 4, g = -0.20, 95% CI -0.53 to 0.14, p = .245). There were too few comparisons to explore the other outcome variables.

Table 2.2

Main effects for secondary outcomes

	k	*g*	95% CI	p-value	Heterogeneity I^2 (*Q with p-value*)
Depression *(A=10, I=8)*					
Overall effect (*N* = 1779)	18	-0.50	-1.08 to 0.08	.090	97% (138.39, <.001)
Excluding low quality studies (*N* = 1212)	**12**	**-0.42**	**-0.68 to -0.15**	**.002**	75% (40.83, <.001)
Anxiety *(A=8, I=6)*					
Overall effect (*N* = 1351)	14	-0.22	-0.48 to 0.04	.095	76% (49.01, <.001)
Excluding low quality studies (*N* = 1153)	**8**	**-0.34**	**-0.65 to -0.03**	**.034**	82% (34.32, <.001)
Acceptance *(A=5, I=2)*					
Overall effect (*N* = 996)	7	-0.29	-0.91 to 0.33	.360	94% (42.90, <.001)
Excluding low quality studies (*N* = 941)	5	-0.45	-1.00 to 0.10	.111	93% (28.21, <.001)
Mindfulness *(A=3, I=4)*					
Overall effect (*N* = 346)	**7**	**0.79**	**0.22 to 1.35**	**.006**	83% (28.02, <.001)
Excluding low quality studies (*N* = 321)	**6**	**0.59**	**0.13 to 1.04**	**.011**	73% (17.07, .004)

Note: *A* = number of active controls in the main analysis for overall effect; *I* = number of inactive controls in the main analysis for overall effect; *N* = participants included in analysis (based on intention-to-treat samples where available); k = number of studies in subgroup; *g* = Hedges' *g*; CI = confidence interval; p-val = significance; significant superiority effects (p <.05) for third wave CBT are denoted in bold

Table 2.3

Meta-analyses of follow-up data

		k	*g*	95% CI	p-value	Heterogeneity I^2 (*Q with p-value*)
Emotional symptoms and internalising problems *(A=8, I=4)*						
Overall effect (*N* = 1388)		12	-0.33	-0.71 to 0.06	.096	90% (70.45, <.001)
Excluding low quality studies (*N* = 933)		9	-0.47	-1.08 to 0.13	.122	94% (60.18, <.001)
Moderators	Subgroups					
Setting (*Qb* = 0.23, *p* = .630)	Clinical	7	-0.39	-0.87 to 0.09	.111	86% (43.38, <.001)
	Non-clinical	5	-0.22	-0.70 to 0.25	.354	85% (26.91, <.001)
Control condition (*Qb* = 0.20, *p* = .652)	Active	8	-0.24	-0.64 to 0.16	.234	85% (45.89, <.001)
	Inactive	4	-0.39	-0.91 to 0.13	.139	53% (6.45, .092)
Delivery (*Qb* = 0.51, *p* = .475)	Individual	4	-0.53	-1.23 to 0.17	.140	83% (17.58, <.001)
	Group	8	-0.24	-0.61 to 0.13	.203	86% (51.77, <.001)
Parental involvement (*Qb* = 0.24, *p* = .626)	Child-only	6	-0.39	-0.85 to 0.08	.104	88% (43.37, <.001)
	Parents involved	6	-0.22	-0.71 to 0.27	.380	81% (26.36, <.001)
Behavioural difficulties and externalising problems *(A=5, I=4)*						
Overall effect (*N* = 834)		9	-0.39	-0.80 to 0.02	.061	83% (46.42, <.001)
Excluding low quality studies (*N* = 751)		7	-0.33	-0.77 to 0.10	.134	83% (36.02, <.001)
Moderators	Subgroups					
Setting (*Qb* = 0.24, *p* = .623)	Clinical	4	-0.53	-1.23 to 0.16	.131	82% (16.99, <.001)
	Non-clinical	5	-0.32	-0.84 to 0.20	.234	85% (27.16, <.001)
Control condition (*Qb* = 0.21, *p* = .648)	Active	5	-0.29	-0.82 to 0.23	.270	86% (27.85, <.001)
	Inactive	4	-0.47	-1.04 to 0.10	.103	59% (7.39, .061)
Interference from difficulties^ *(A=6, I=0)*						
Overall effect (*N* = 795)		6	-1.66	-3.33 to 0.02	.053	99% (282.90, <.001)

	k	*g*	95% CI	p-value	Heterogeneity I^2 (*Q with p-value*)
Third wave processes^ *(A=4, I=1)*					
Overall effect (*N* = 819)	5	-0.63	-1.69 to 0.43	.243	98% (53.48, <.001)
Wellbeing and flourishing^ *(A=4, I=1)*					
Overall effect (*N* = 170)	5	0.56	-0.37 to 1.49	.235	87% (31.78, <.001)
Quality of life^ *(A=4, I=0)*					
Overall effect (*N* = 576)	4	0.17	-0.08 to 0.42	.194	38% (4.50, .212)
Physical health and pain^ *(A=5, I=0)*					
Overall effect (*N* = 744)	5	0.80	-0.60 to 2.19	.262	98% (187.59, <.001)

Note: *A* = number of active controls in the main analysis for overall effect; *I* = number of inactive controls in the main analysis for overall effect; *N* = participants included in analysis (based on intention-to-treat sample where available); k = number of studies in subgroup; *g* = Hedges' *g*; CI = confidence interval; p-val = significance
^All studies at follow-up were rated moderate-high quality

Publication bias

Rank correlation tests for funnel plot asymmetry were non-significant for all primary and secondary outcomes, except for behavioural difficulties/externalising problems and wellbeing/flourishing. Nonetheless, visual analysis of funnel plots for these outcomes was not suggestive of bias and no missing studies were estimated. Whilst it was estimated that there were three missing null studies for interference from difficulties, asymmetry was non-significant.

See supplementary material for rank correlation tests and funnel plots.

Discussion

Main findings

Thirty RCTs were included in this meta-analysis. Most involved ACT and MBCT interventions, delivered to groups within school or community settings. For meta-analyses focused on earlier generations of CBT, a greater proportion of included trials have been conducted in clinical settings exploring individual therapy (e.g. Olatunji, Davis, Powers & Smits, 2012). This difference may be because third wave CBT is promoted as a more transdiagnostic approach, applicable across the spectrum from ill-health to flourishing (Hayes & Hofmann, 2017). It could also reflect decreased research funding for individual psychological treatment trials since second wave CBT was classified as the "gold-standard" intervention (David, Cristea & Hofmann, 2018). There is now a call for research to explore universal interventions that are applicable across the population and along the spectrum from mental health treatment to prevention and promotion of wellbeing (UK

Department of Health [DoH] & Department for Education [DfE], 2017; Public Health England [PHE], 2019).

Main analyses yielded significant, moderate effects at post-treatment in favour of third wave CBT compared to control conditions (which were a mixture of inactive and active comparisons) for measures of behavioural difficulties/externalising problems, third wave processes, and wellbeing/flourishing; alongside non-significant findings for the other outcomes. Nonetheless, a substantial number of the identified studies were rated as low quality, perhaps reflective of scarce funding and/or the relatively novel application of third wave CBT to children and adolescents (David, Cristea & Hofmann, 2018).

Interestingly, sensitivity analyses excluding low quality studies yielded more promising results. Significant, small effects were observed for emotional symptoms/internalising problems and quality of life; significant, moderate effects for third wave processes and wellbeing/flourishing; and significant, large effects for interference from difficulties. On the other hand, behavioural difficulties/externalising problems ceased to be significant, while physical health/pain was consistently non-significant. These results suggested that the inclusion of poor quality research could have masked true effects.

Additional analyses revealed that, for some variables, effectiveness may vary according to type of third wave CBT, setting, control comparison, and whether parents were involved. Nonetheless, the reliability and validity of any significant moderators or apparent subgroup differences needs to be carefully considered, given that low quality studies were included in these analyses; thus, effects may be explained by quality rather than the moderator or subgroup variable itself.

With regard to secondary outcomes, a significant, large effect was found favouring third wave CBT for mindfulness, alongside non-significant results for depression, anxiety, and acceptance. The subsequent exclusion of low quality studies yielded significant, small effects for depression and anxiety; significant, moderate effects for mindfulness; while non-significant effects for acceptance. Again, this suggested low quality research may have biased initial results.

At follow-up, all primary outcomes yielded non-significant effects, though they were of varying magnitude (small-large) in favour of third wave CBT; the only exception was quality of life, where the effect size was negligible. Similar findings were observed when low quality studies were excluded. Given that only a minority of trials at post-treatment included follow-up assessments, it was difficult to evaluate maintenance effects. Power may have been limited and the composition of studies for post-intervention versus follow-up analyses differed. For example, for all outcomes at follow-up, a greater proportion of studies utilised active control groups, relative to those studies that comprised post-treatment comparisons. Another difference was that most studies at follow-up were conducted in clinical settings, whereas most studies at post-treatment were conducted in non-clinical settings. Unfortunately, few moderator and subgroup analyses could be performed due to the limited number of comparators.

Analyses specifically comparing third wave to alternative psychological therapies at post-treatment failed to show any significant differences.

Clinical and research implications

It is essential to ensure the quality of interventions offered within child and adolescent services, as well as investigate universal approaches that can be used more widely as preventative and promotive public health strategies for youth (DoH & DfE, 2017; PHE, 2019). Until now, no meta-analysis existed to determine the effectiveness of third wave CBT for young people, despite its applicability across diagnostic categories and along the spectrum from ill-health to flourishing, and its increasing popularity within clinical and non-clinical settings.

Overall, the present results suggest that third wave CBT is a promising intervention. Whilst there were a number of low quality studies, excluding these yielded significant post-treatment effects across a range of outcomes from symptomatology to thriving. Sample size remained high for these sensitivity analyses, increasing confidence in the findings. It is worth noting, nonetheless, that widespread heterogeneity raised queries about generalisability, and that maintenance effects were difficult to evaluate. More high-quality trials investigating effects at both post-treatment and follow-up are therefore needed.

Larger effects were found for interference from difficulties, third wave processes, and wellbeing/flourishing, relative to clinical symptomatology and quality of life. This pattern fits with the premise of third wave CBT, which primarily aims to change how an individual relates to distress, whilst symptom reduction is secondary (Hayes, 2004). This may explain non-significant findings for physical health/pain; perhaps third wave CBT is ineffective for reducing such symptoms, but decreases the interference or emotional

impact of these difficulties. Indeed, most studies conducted on physical health samples targeted broad health and wellbeing outcomes.

Findings suggested that third wave CBT may not be effective for behavioural difficulties/externalising problems when study quality was considered. Interestingly, there was no effect on the specific outcome of acceptance. This finding needs further exploration in terms of the suitability of acceptance-related measures, and acceptance-based intervention strategies, with children and adolescents. It is also important to note that there was considerable heterogeneity for physical health/pain, behavioural difficulties/externalising problems, and acceptance, with small-moderate overall effect sizes favouring third wave CBT; this heterogeneity suggests that conclusions may not be generalisable and that further investigation is warranted.

Although attempts to explore heterogeneity were made, findings should be interpreted with caution given the inclusion of low quality research in moderation and subgroup analyses. There was indication that third wave CBT may be effective when delivered in clinical settings, but not within schools or communities, for emotional symptoms/internalising problems, interference from difficulties, wellbeing/flourishing, and quality of life.

Nonetheless, when low quality research was excluded, main effects for these outcomes were significant despite most studies being conducted within non-clinical settings. This suggests that third wave CBT could be effective within schools and communities, but poor quality studies may have biased results; it is plausible that interventions and research methods are not as rigorous in non-clinical as opposed to clinical spaces, skewing effects. Moreover, measures used in clinical research may be

insensitive to meaningful change in community populations (PHE, 2019) and there was potential for floor or ceiling effects. Further still, it remains difficult to judge third wave CBT as a universal strategy to promote public health (PHE, 2019); whilst many studies were conducted in community or school settings, most selected participants with (sub)clinical symptomatology, while only four used general samples.

Other moderator analyses yielded inconsistent findings and/or were limited by the number of available studies. For interference from difficulties, significant effects were apparent when parents were involved but not when the intervention was delivered with youth-only, whereas the opposite pattern was observed for behavioural difficulties/externalising problems and quality of life. For third wave processes, ACT but not MBCT yielded significant effects. This finding might be expected as ACT targets a wider range of processes captured within this outcome category (e.g. acceptance, defusion), while MBCT focuses mainly on mindfulness (O'Brien, Larson & Murrell, 2008). Differences between types of third wave CBT, physical and mental health settings, the impact of group versus individual delivery, and participant age, could not be thoroughly explored due to a limited number of available comparators.

Where subgroup analyses were possible, third wave CBT was not found to significantly differ from active control groups specifically, except for third wave processes, where third wave CBT outperformed controls. Consistently, alternative treatments may be expected to target symptoms, wellbeing, and quality of life, whilst not outcomes such as acceptance, defusion, and mindfulness. It is possible that alternative treatments outperform third wave CBT for their intended mechanisms (e.g. cognitive bias in traditional CBT), but this was not explored.

Whilst comparisons were limited, findings suggested that third wave CBT did not perform significantly differently from alternative psychological therapies, namely traditional CBT, in particular. At present, second wave CBT is viewed as the gold standard treatment in clinical guidelines such as NICE (David, Cristea & Hofmann, 2018). This review indicated that third wave interventions could be similarly effective, though this clearly requires further research, as only two of the seven primary outcomes could be explored and study numbers were limited. Cost-effectiveness analyses are required in future research; the majority of included studies conducted relatively short, group interventions, suggesting the possibility that third wave CBT may be an inexpensive, clinically-effective alternative to current treatments.

There were no significant superiority effects for third wave CBT at follow-up, even when low quality studies were excluded. This may, nonetheless, be because most studies with follow-up data utilised active controls, with which third wave CBT performed similarly. However, effects remained non-significant for emotional symptoms/internalising problems and behavioural difficulties/externalising problems within subgroup analyses utilising only inactive controls, suggesting that third wave CBT may be ineffective at inducing long-term change for these outcomes at least.

Moderation and subgroup analyses were not possible for the other variables due to a limited number of comparators. Thus, it remains unclear whether third wave CBT fails to outperform inactive conditions across all domains at follow-up. It should also be noted that all main and moderator/subgroup analyses at follow-up could have been underpowered. Effect sizes remained small-large in favour of third wave CBT (except for quality of life), and there was significant heterogeneity that could not be thoroughly

explored due to a limited availability of current research. More high-quality RCTs are needed to increase the scope and power of any future meta-analyses assessing follow-up effects.

Strengths and limitations

This was a comprehensive review including a range of outcomes and a high number of participants across both clinical and non-clinical settings. It enabled a thorough investigation into the effectiveness of third wave CBT for children and adolescents, from the treatment of symptomatology to promotion of thriving, as well as a rigorous evaluation of study quality.

Publication bias was limited but many studies were identified as poor quality. Whilst sensitivity analyses were conducted for main effects, results may have still been biased, as only seven of 30 studies were rated as high quality overall, and only four were rated highly with regard to the quality of the intervention. Many used interventions that were non-specific (i.e. combined with non-relevant therapies), unstandardised, or incomplete (e.g. exploring only defusion from ACT). In general, the degree of adaption for children and adolescents was difficult to evaluate, and possibly deficient; for example, no studies exploring ACT reported using the developmentally-adapted model, DNA-V (Hayes & Ciarrochi, 2015). If high-quality interventions were delivered, effects at post-treatment and follow-up may have differed, and would have possibly increased. It was apparent that third wave CBT, as a relatively new construct, has not yet been investigated to the same standard as other therapies.

Investigation of heterogeneity through moderation and subgroup analyses was a strength of this study. It enabled exploration of what particular interventions might be effective, for whom, and in what settings, which is of clinical importance. There were a high number of variables but these were decided a priori (see protocol) and corrections for multiple comparisons were made.

Nonetheless, significant moderation or subgroup findings should be interpreted with caution, given that they may be explained by the inclusion of low quality studies, rather than the moderator or subgroup variable itself. Moreover, moderator and subgroup variables were not explored in conjunction, but it is possible they account for one another; for example, differences between clinical and non-clinical settings could be explained by mode of delivery (group versus individual therapy), and parental involvement may be explained by child age.

Several planned analyses were not possible due to a limited number of comparators. For example, no moderator or subgroup analyses could be conducted for physical health and pain, and it was not possible to compare CFT and MCT. Important moderators (e.g. therapist versus teacher delivery) may have been missed from this review, although it is highly likely some of these would be difficult to explore at present given the limited research to currently draw on.

Conclusions

To date, this is the first meta-analysis known to consider the effectiveness of third wave CBT for children and adolescents. Thirty RCTs were identified, though many were of poor quality, both with regard to research design and the intervention delivered.

Excluding studies rated as low quality yielded promising results; significant effects were found for a variety of outcomes, including emotional symptoms/internalising problems, interference from difficulties, third wave processes, wellbeing/flourishing, and quality of life. Results were non-significant for behavioural difficulties/externalising problems and physical health/pain. Significant effects were also observed for specific outcomes of depression, anxiety, and mindfulness, but not acceptance. Widespread heterogeneity remained for all variables, raising queries about the generalisability of findings. Moderation and subgroup analyses yielded further complexity, though needed to be interpreted with caution. It remains unclear whether third wave CBT is effective for inducing long-term change. Further high quality research is warranted to investigate third wave CBT as both a treatment strategy and public health tool to promote thriving among young people.

Declarations of interest:

None

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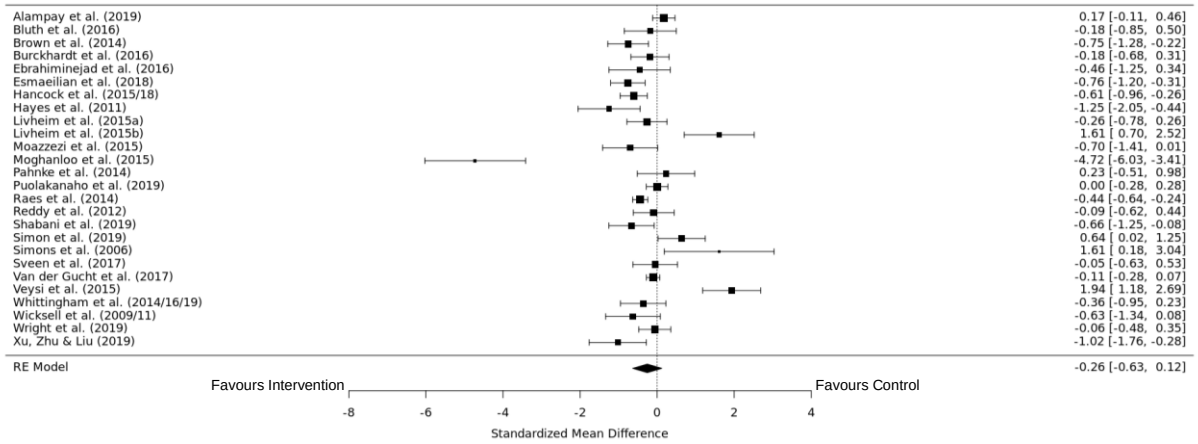
Supplementary Material

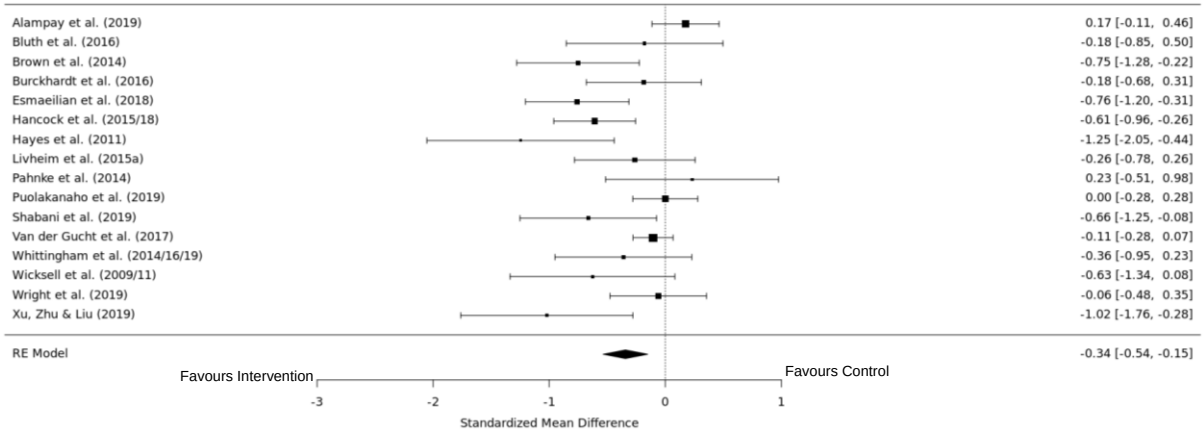
- Forest plots for the primary outcome variables
- Table of included studies
- Quality assessment process
- Exploration of cluster randomisation effects
- Inspection of publication bias

Forest Plots for Primary Outcomes (Figure 2.2)

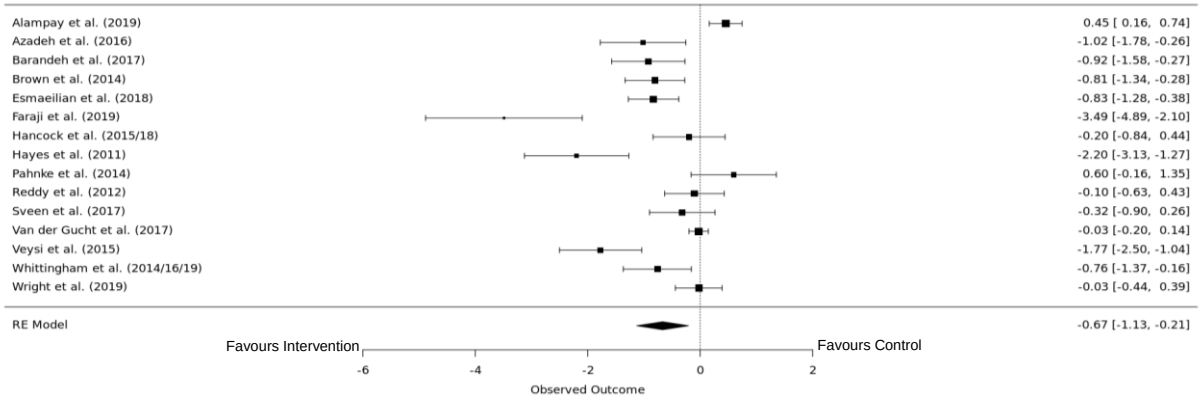
1a. Emotional Symptoms and Internalising Problems – all studies



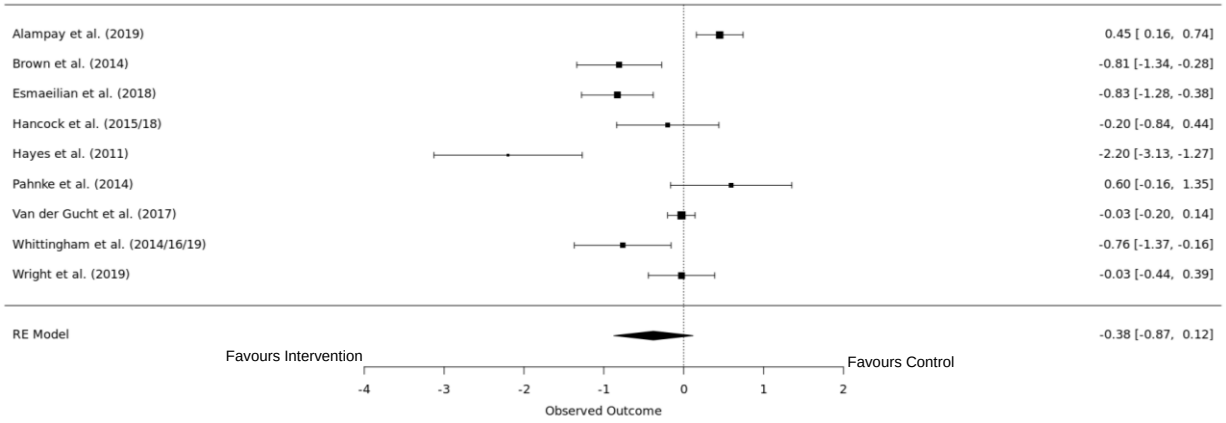
1b. Emotional Symptoms and Internalising Problems – excluding low quality studies



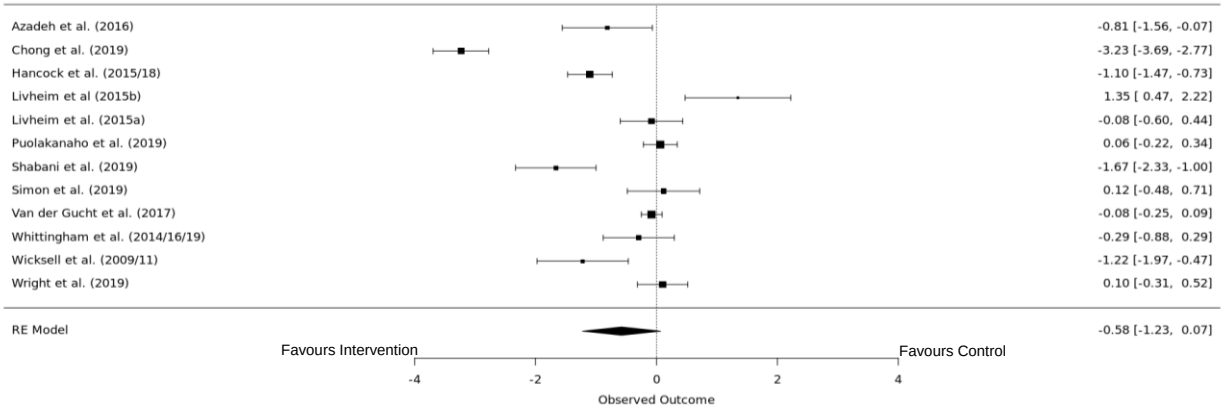
2a. Behavioural Difficulties and Externalising Problems – all studies



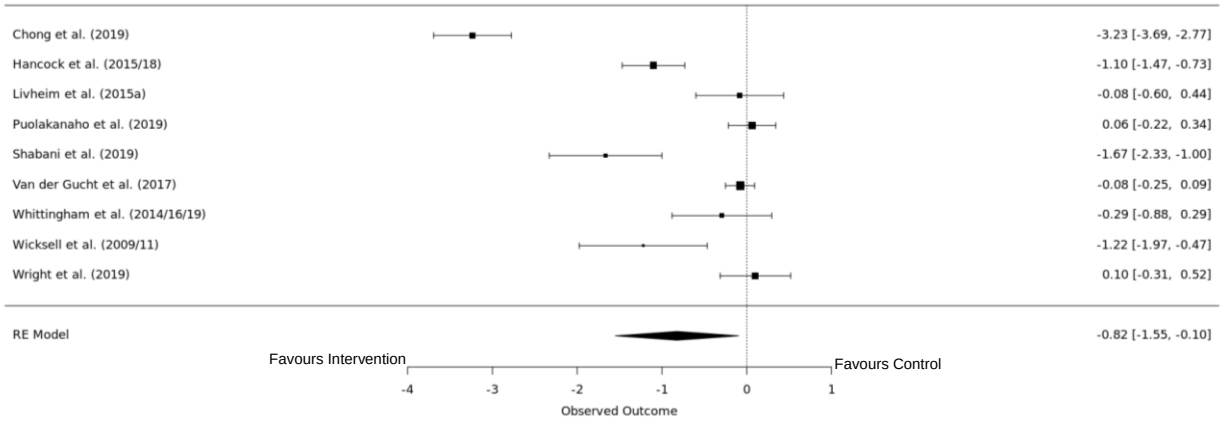
2b. Behavioural Difficulties and Externalising Problems – excluding low quality studies



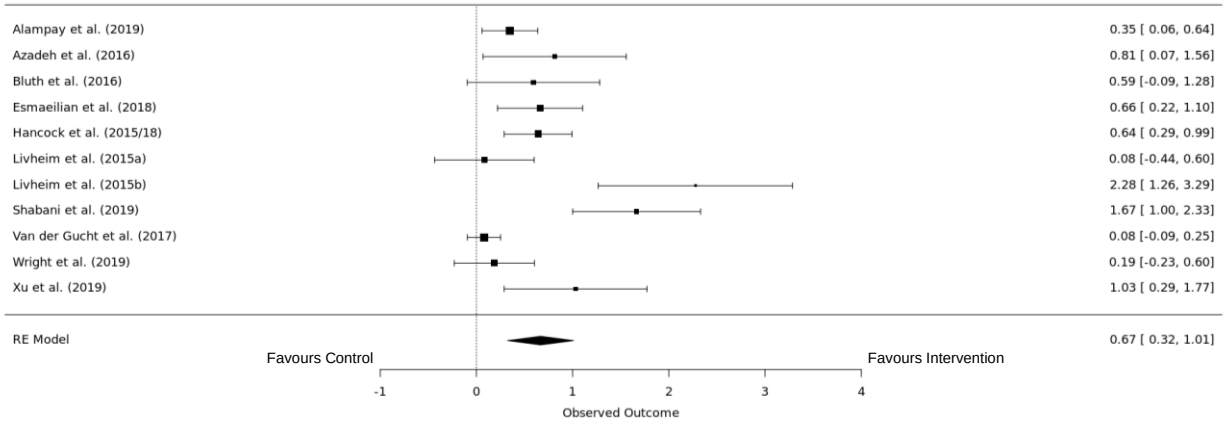
3a. Interference from Difficulties – all studies



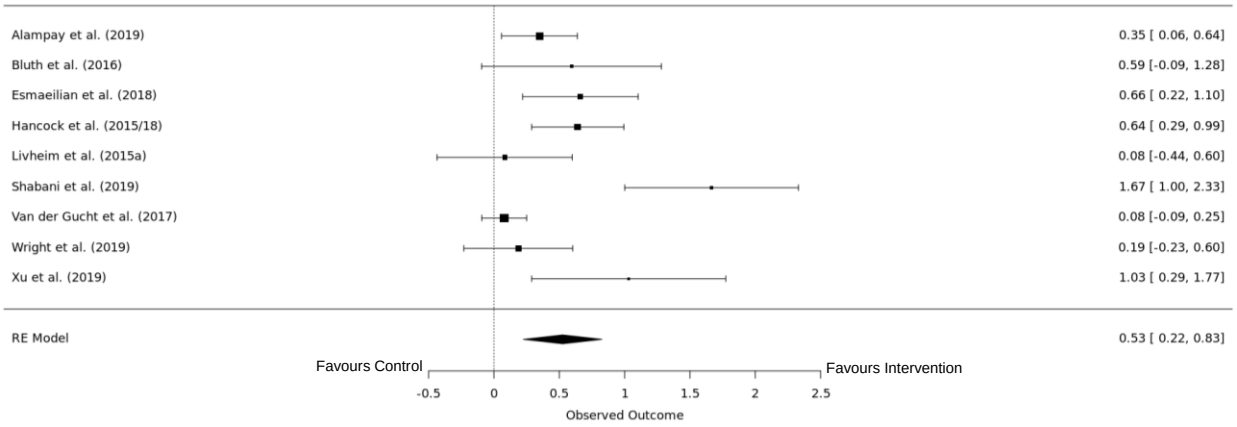
3b. Interference from Difficulties – excluding low quality studies



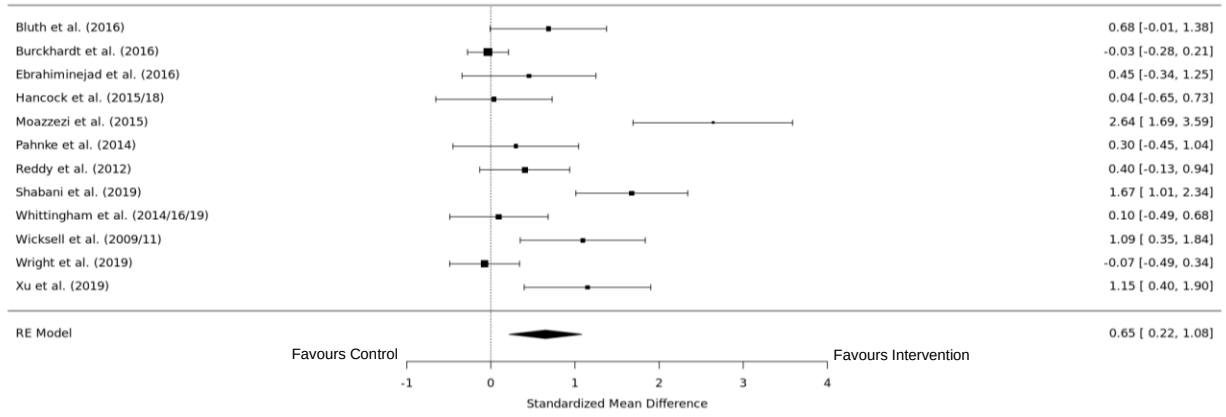
4a. Third Wave Processes – all studies



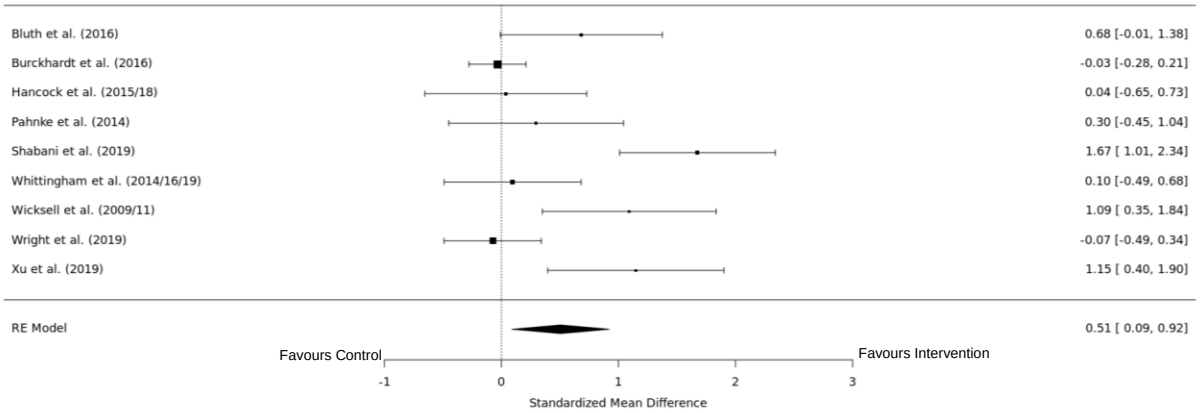
4b. Third Wave Process – excluding low quality studies



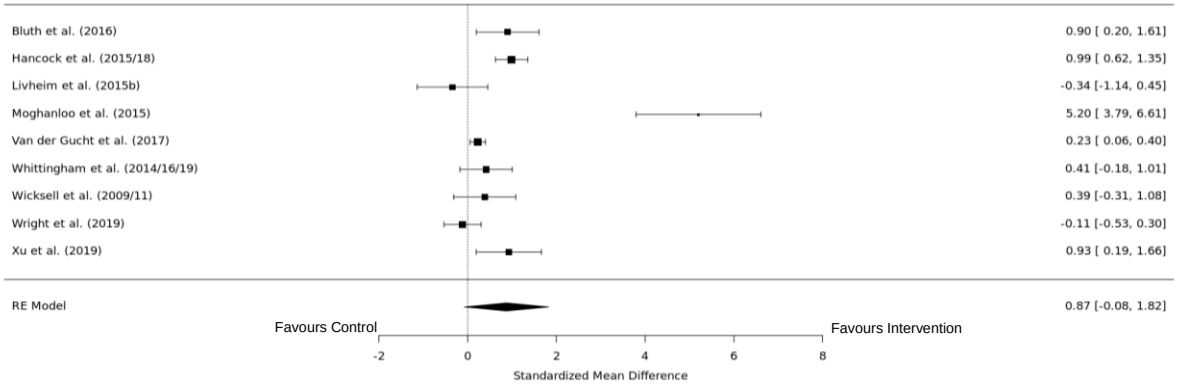
5a. Wellbeing and Flourishing – all studies



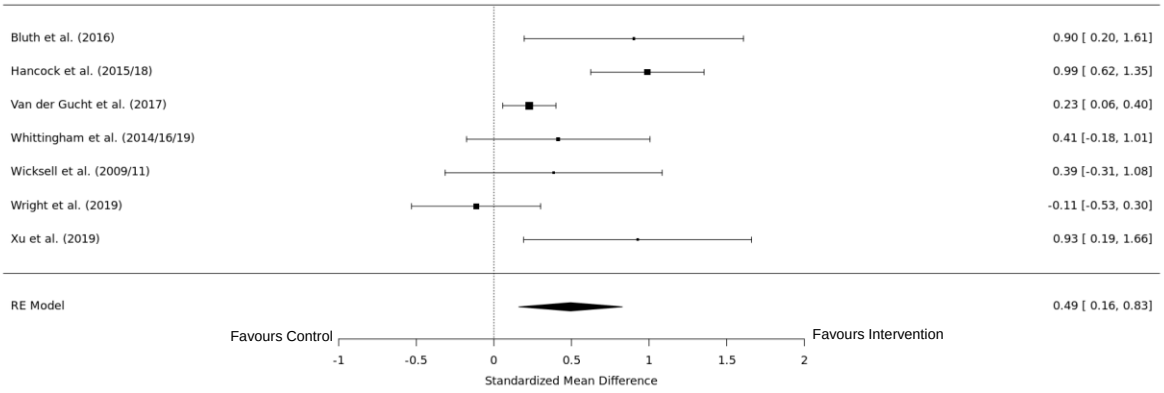
5b. Wellbeing and Flourishing – excluding low quality studies



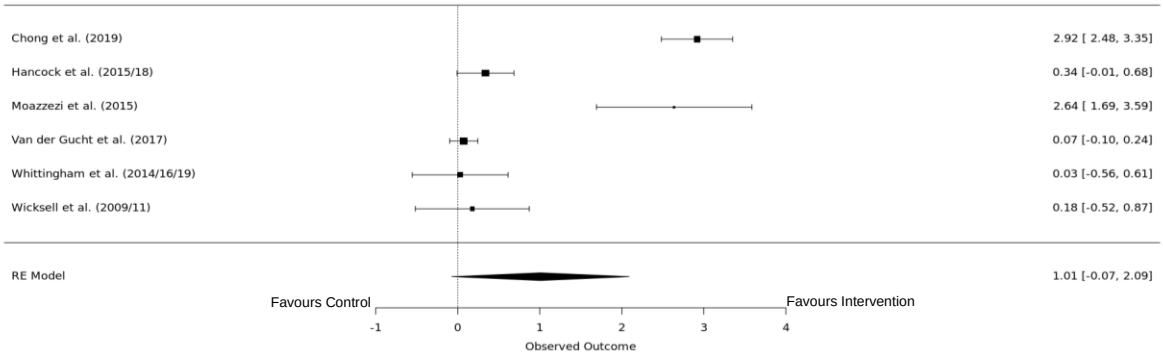
6a. Quality of Life – all studies



6b. Quality of Life – excluding low quality studies



7a. Physical Health and Pain – all studies



7b. Physical Health and Pain – excluding low quality studies

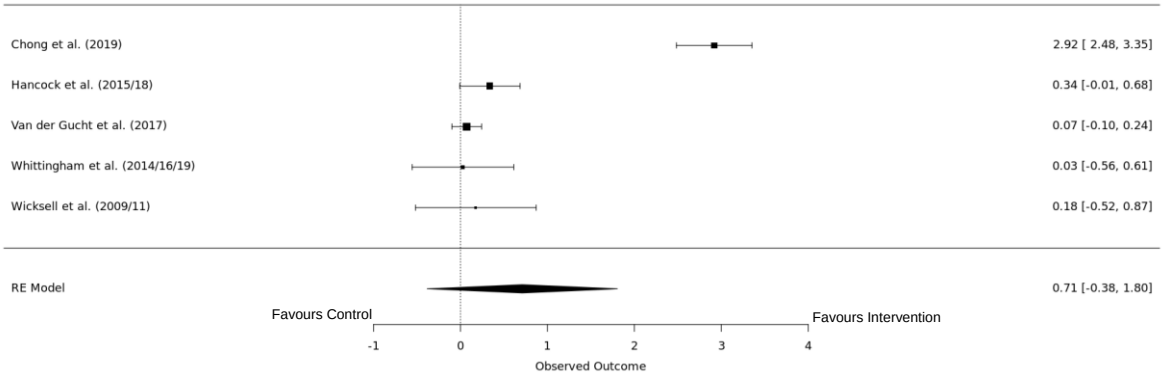


Figure 2. Forest plots detailing effect sizes with 95% confidence intervals for the seven primary outcome variables (inclusive and exclusive of low quality studies)

Table of Included Studies (Table 2.4)

Study author and year	Intervention, duration (mins per session), level of child-parent involvement, and delivery format	Conditions	Participant group[1] (M age) and target condition	N (% dropout); ITT or subset sample	Setting and country	Outcome measures (subscales) included within meta-analyses	Longest follow-up in months	Quality rating
Alampay et al. (2019)	MBCT; 8 weekly sessions (75-90); Child-only; Group sessions	1. MBCT 2. Active control (handicrafts)	Children and adolescents (11.88); Behavioural, emotional or peer difficulties	186 (21.51); ITT	School, Philippines	DERS (limited regulation strategies, impulse control, lack of emotion awareness, non-acceptance of emotions); SMFQ (depression); STAIC	2	Moderate
Azadeh, Kazemi-Zahrani & Besharat (2016)	ACT; 10 weekly sessions (90); Child-only; Group sessions	1. ACT 2. No intervention /waitlist	Adolescents (15.48); Social anxiety disorder	30 (NR); NR	School, Iran	IIP-60; AAQ-II	NA	Low
Barandeh, Shafiabadi & Farzad (2017)	ACT; 8 weekly sessions (NR); Child-only; Group sessions	1. ACT 2. Choice theory intervention 3. No intervention /waitlist	Adolescents (NR); Procrastination difficulties	60 (NR); NR	School, Iran	PASS	1	Low

Study author and year	Intervention, duration (mins per session), level of child-parent involvement, and delivery format	Conditions	Participant group[1] (M age) and target condition	N (% dropout); ITT or subset sample	Setting and country	Outcome measures (subscales) included within meta-analyses	Longest follow-up in months	Quality rating
Bluth et al. (2016)	CFT; 6 weekly sessions (90); Child-only; Group sessions	1. Mindful Self-Compassion Program 2. No intervention /waitlist	Adolescents (NR); General population	34 (85.29); NR	School/community, USA	PNAS (negative affect); SMFQ; STAI; CAMM; SCS-SF; SLSS	NA	Moderate
Brown et al. (2014)	ACT; 2 sessions (120); Parent-only; Group sessions	1. ACT + SSTP 2. TAU	Child (7.00); Acquired brain injury	59 (47.46); ITT	Clinical PH, Australia	SDQ (emotional symptoms); ECBI (intensity)	NA	Moderate
Burckhardt et al. (2016)*	ACT; 16 sessions over 3 months (30); Child-only; Group sessions	1. ACT (+ positive psychology) 2. Usual pastoral care class	Adolescents (16.36); General population	267 (17.23); ITT	School, Australia	DASS (total, depression, anxiety)	NA	Moderate
Chong, Mak, Leung, Lam & Loke (2019)	ACT; 4 weekly sessions (120); Parent-only; Group sessions	1. ACT (plus asthma education) 2. Asthma education	Children (6.80); Asthma	168 (3.57); ITT	Clinical PH, China	Days with symptoms; Days with limited activity; Emergency Department visits	6	High

Study author and year	Intervention, duration (mins per session), level of child-parent involvement, and delivery format	Conditions	Participant group[1] (M age) and target condition	N (% dropout); ITT or subset sample	Setting and country	Outcome measures (subscales) included within meta-analyses	Longest follow-up in months	Quality rating
Ebrahiminejad, Poursharifi, Roodsari, Zeinodini & Noorbakhsh (2016)	MBCT; 8 weekly sessions (90); Child-only; Group sessions	1. MBCT 2. No intervention /waitlist	Adolescents (14.95); Social anxiety	30 (17); NR	School, Iran	SPIN; RSES	NA	Low
Esmaeilian, Dehghani, Dehghani & Lee (2018)**	MBCT; 12 weekly sessions (90); Child-only; Group sessions	1. MBCT 2. No intervention /waitlist	Children and adolescents (12.13); Parental divorce	83 (14.46); NR	School, Iran	STAIC (state anxiety); CDI; STAXI-2 (trait anger); CAMM	2	High
Faraji, Talepasand & Boogar (2019)	MBCT; 12 weekly sessions (90); Child-only; Group sessions	1. MBCT 2. No intervention /waitlist	Children (NR); Bullying behaviour	20 (NR); NR	School, Iran	IBS	NA	Low
Hancock et al. (2018); Swain et al. (2015)	ACT; 10 weekly sessions (90); Joint parent-child involvement; Group sessions	1. ACT 2. CBT 3. No intervention /waitlist	Children and adolescents (11.00); Anxiety disorder	193 (18.65); ITT	Clinical MH, Australia	ADIS (diagnosis); CDI; CBCL (total problems); CALIS (parent interference); AFQ-Y17; CAMM (observe); VLQ;	3	High

Study author and year	Intervention, duration (mins per session), level of child-parent involvement, and delivery format	Conditions	Participant group[1] (M age) and target condition	N (% dropout); ITT or subset sample	Setting and country	Outcome measures (subscales) included within meta-analyses	Longest follow-up in months	Quality rating
						CHQ (psychosocial, physical)		
Hayes, Boyd & Sewell (2011)**	ACT; 21 hours of sessions average; Child-only; Individual sessions	1. ACT 2. TAU (psychotherapy/CBT)	Adolescents (14.90); Depression	38 (68.42); NR	Clinical MH, Australia	RADS-2; SDQ	3	Moderate
Livheim et al. (2015) – Study A**	ACT; 8 weekly sessions; Child-only; Group sessions	1. ACT 2. Usual school care (support from school counsellor)	Adolescents (14.6); Depression	66 (22.73); ITT	School, Australia	RADS-2; AFQ-Y8	NA	Moderate
Livheim et al. (2015) – Study B	ACT; 8 sessions over 6 weeks (90m); Child-only; Group sessions	1. ACT 2. Usual school care (sessions with school nurse)	Adolescents (NR); Stress	32 (21.88); ITT	School, Sweden	GHQ-12; DASS (depression, anxiety); AFQ-Y17; MAAS; SWLS	NA	Low
Moazzezi et al. (2015)	ACT; 10 weekly sessions (90); Child-only; Individual sessions	1. ACT 2. No intervention /waitlist	Children and adolescents (10.58); Diabetes Mellitus	40 (10); NR	Clinical PH, Iran	PSS; SH-SES	NA	Low

Study author and year	Intervention, duration (mins per session), level of child-parent involvement, and delivery format	Conditions	Participant group[1] (M age) and target condition	N (% dropout); ITT or subset sample	Setting and country	Outcome measures (subscales) included within meta-analyses	Longest follow-up in months	Quality rating
Moghanloo, Moghanloo & Moazzezi (2015)	ACT; 10 weekly sessions (90); Child-only; Group sessions	1. ACT 2. No intervention /waitlist	Children and adolescents (10.47); Diabetes Mellitus	40 (15); NR	Clinical PH, Iran	RCDS; SWLS	NA	Low
Pahnke et al (2014)*	ACT; 12 sessions over 6 weeks (40), Child-only; Group sessions	1. ACT 2. No intervention /waitlist	Adolescents (16.50); Autism	28 (0); Not relevant	School, Sweden	SDQ (emotional symptoms, hyperactivity/inattention prosocial behaviour)	2	Moderate
Puolakanaho et al. (2019)	ACT; 5 weeks of at least 6 exercises per week, plus weekly contact via SMS and 2 face-to-face sessions for iACT face-to-face group; Child-only; Individual sessions	1. iACT online plus face-to-face 2. iACT online only 3. Usual school support	Adolescents (15.27); General population	249 (4.02); ITT	School/online, Finland	SS; AcBS	NA	High
Raes, Griffith, Van der Gucht &	MBCT; 8 weekly sessions (100); Child-only; Group sessions	1. MBCT (plus MBSR)	Adolescents (15.40); Depression	408 (15.44); ITT	School, Belgium	DASS (depression)	6	Low

Study author and year	Intervention, duration (mins per session), level of child-parent involvement, and delivery format	Conditions	Participant group[1] (M age) and target condition	N (% dropout); ITT or subset sample	Setting and country	Outcome measures (subscales) included within meta-analyses	Longest follow-up in months	Quality rating
Williams (2014)*		2. No intervention /waitlist						
Reddy et al. (2012)	CFT; 2 sessions per week for 6 weeks (60); Child-only; Group sessions	1. Cognitive-based Compassion Training 2. No intervention /waitlist	Adolescents (14.70); Looked after children	71 (NR); NR	Foster care system, USA	QIDS-SR; STAI (trait anxiety); DERS; ICU (youth report)	NA	Low
Shabani et al. (2019)**	ACT; 10 weekly sessions (60); Joint parent-child involvement; Group sessions	1. ACT + SSRI 2. CBT + SSRI 3. SSRI	Adolescents (14.96); Obsessive compulsive disorder	69 (20.29); ITT	Clinical MH, Iran	CDI; CY-BOCS; AFQ-Y8; CAMM; VLQ	3	High
Simon, Driessen, Lambert & Muris (2019)	ACT; 1 session (30); Child-only; Individual sessions	1. ACT (cognitive defusion) 2. CBT (cognitive restructuring)	Children (9.33); Fear of the dark	43 (0); Not relevant	Community, Belgium	Fear of the Dark Visual Analogue Scale; Darkness Toleration (seconds)	NA	Low

Study author and year	Intervention, duration (mins per session), level of child-parent involvement, and delivery format	Conditions	Participant group[1] (M age) and target condition	N (% dropout); ITT or subset sample	Setting and country	Outcome measures (subscales) included within meta-analyses	Longest follow-up in months	Quality rating
Simons, Schneider & Herpertz-Dahlmann (2006)	MCT; up to 20 weekly sessions; Joint parent-child involvement; Individual sessions	1. MCT 2. Narrative Exposure and Response Prevention	Children and adolescents (13.96); Obsessive compulsive disorder	11 (9.09); NR	Clinical MH, Germany	CDI; CY-BOCS	24	Low
Sveen, Andersson, Buhrman, Sjoberg & Willebrand (2017)	ACT; 6 weekly internet modules; Parent-only; Individual sessions	1. ACT (and CBT components) 2. No intervention /waitlist	Children and adolescents (5.83); Burns	104 (58.65); NR	Clinical PH, Sweden	CSRF-SF; SDQ	12	Low
Van der Gucht et al. (2017)*	ACT; 4 weekly sessions (120); Child-only; Group sessions	1. ACT 2. Usual school curriculum	Adolescents (17.00); Non-clinical/mental health promotion	586 (34.98); NR	School, Belgium	YSR (somatic, affective, anxiety, ADHD); AFQ-Y17; WHO QoL-Brief (social, physical)	12	High
Veysi et al. (2015)	MBCT; 12 weekly sessions (120); Child-only; Group sessions	1. MBCT 2. No intervention /waitlist	Adolescents (13.85); Learning difficulties	40 (NR); NR	School, Iran	ACS (total, anxiety, depression)	NA	Low

Study author and year	Intervention, duration (mins per session), level of child-parent involvement, and delivery format	Conditions	Participant group[1] (M age) and target condition	N (% dropout); ITT or subset sample	Setting and country	Outcome measures (subscales) included within meta-analyses	Longest follow-up in months	Quality rating
Whittingham, Sanders, McKinlay & Boyd (2014; 2016; 2019)	ACT; 2 sessions (120); Parent-only; Group sessions	1. ACT + SSTP 2. SSTP 3. No intervention /waitlist	Children (5.30); Cerebral palsy	67 (57.35); ITT	Clinical PH, Australia	SDQ (emotional symptoms, impact, prosocial behaviour); ECBI (intensity); CP-QoL (family health); PEDI (self-care)	6	Moderate
Wicksell et al. (2009); Wicksell, Olsson & Hayes (2011)	ACT; 10 weekly child sessions (60) plus 1-2 parent sessions (90); Joint parent-child involvement; Individual sessions	1. ACT 2. TAU (MDT approach + pain medication)	Children and adolescents (14.80); Pain	32 (6.25); ITT	Clinical PH, Sweden	CES-DC; PAIRS; SES; SF-36 (mental health); FDI (child report)	6.5	Moderate
Wright, Roberts & Proeve (2019)	MBCT; 10 weekly sessions plus 2 meeting for parents and weekly emails encouraging practice; Joint parent-child involvement; Group sessions	1. MBCT 2. CBT	Children and adolescents (10.60); Internalising difficulties	89 (4.49); ITT	School, Australia	RCADS (depression, anxiety); AtCS; CYRM-12; CAMM; SDQ (prosocial behaviour – teacher report); PQoL	NA	High

Study author and year	Intervention, duration (mins per session), level of child-parent involvement, and delivery format	Conditions	Participant group[1] (M age) and target condition	N (% dropout); ITT or subset sample	Setting and country	Outcome measures (subscales) included within meta-analyses	Longest follow-up in months	Quality rating
Xu, Zhu & Liu (2019)	MBCT; 8 weeks with a minimum of 6 minutes of training per day; Child-only; Individual sessions	1. MBCT 2. No intervention /waitlist	Adolescents (16.75); Mild depression	36 (11.11); NR	School, China	BDI-II; CAMM; MILQ; ABS	NA	Moderate

[1]Children = 0-12 years; adolescents = 13-18 years
*cluster randomised studies
**studies rated highly for intervention quality
ACT = Acceptance and Commitment Therapy; CFT = Compassion Focused Therapy; MBCT = Mindfulness-Based Cognitive Therapy; MCT = Metacognitive Therapy; SSTP = Stepping Stones Triple P; TAU = Treatment as Usual; CBT = Cognitive Behavioural Therapy; MBSR = Mindfulness-Based Stress Reduction; SSRI = Selective serotonin reuptake inhibitor; M = mean; NR = not reported in study; N = number of participants; ITT = intention-to-treat; NA = not measured in study
Measures: AAQ-II = Acceptance and Action Questionnaire-II; ABS = Affect Balance Scale; AcBS = Academic Buoyancy Scale; ACS = Affective Control Scale; AtCS = Attention Control Scale; ADIS = Anxiety Disorders Interview Schedule; AFQ-Y8 = Avoidance and Fusion Questionnaire for Youth-8; AFQ-Y17 = Avoidance and Fusion Questionnaire for Youth-17; BDI-II = Beck Depression Inventory-II; CALIS = Child Anxiety Life Interference Scale; CAMM = Child and Adolescent Mindfulness Measure; CBCL = Child Behaviour Checklist; CDI = Children's Depression Inventory; CES-DC = Centre for Epidemiological Studies Depression Scale for Children; CHQ = Child Health Questionnaire; CP-QoL = Cerebral Palsy Quality of Life Child; CSRS-SF = Child Stress Reaction Checklist-Short Form; CY-BOCS = Children's Yale-Brown Obsessive Compulsive Scale; CYRM-12 = Child and Youth Resilience Measure; DASS = Depression Anxiety Stress Scales; DERS = Difficulties with Emotion Regulation Scale; ECBI = Eyberg Child Behaviour Inventory; FDI = Functional Disability Inventory; GHQ-12 = General Health Questionnaire-12; IBS = Illinois Bullying Scale; ICU = Inventory of Callous and Unemotional Traits; IIP-60 = Inventory of Interpersonal Problems; MAAS = Mindful Attention Awareness Scale; MILQ = Meaning in Life Questionnaire; PAIRS = Pain Impairment Relationship Scale; PASS = Procrastination

Assessment Scale-Student; PEDI = Pediatric Evaluation of Disability Inventory; PNAS = Positive and Negative Affect Schedule; PQoL = Pediatric Quality of Life Enjoyment and Satisfaction Questionnaire; PSS = Perceived Stress Scale; QIDS-SR = Quick Inventory of Depressive Symptomatology-Self Report; RADS-2 = Reynolds' Adolescent Depression Scale-2; RCADS = Revised Child Anxiety and Depression Scale; RCDS = Reynolds' Child Depression Scale; RSES = Rosenberg's Self-Esteem Scale; SCS-SF = Self-Compassion Scale-Short Form; SDQ = Strengths and Difficulties Questionnaire; SES = Self-Efficacy Scale; SF-36 = Short Form-36 Health Survey; SH-SES = Special Health Self-Efficacy Scale; SMFQ = Short Mood and Feelings Questionnaire; SLSS = Student's Life Satisfaction Scale; SPIN = Social Phobia Inventory; SS = Stress Scale; STAI = State-Trait Anxiety Inventory; STAIC = State-Trait Anxiety Inventory for Children; STAXI-2 = State-Trait Anger Expression Inventory-2; SWLS = Satisfaction with Life Scale; VLQ = Valued Living Questionnaire; WHO QoL-Brief = World Health Organisation Quality of Life Questionnaire-Brief; YSR = The Youth Self Report

A reference list of studies included in the meta-analysis and cited in this table but not in the main text are available in Appendix D

Quality Assessment Process

The Cochrane risk-of-bias tool for randomised trials (Version 2) (Sterne et al., 2019) or for cluster-randomised designs (Eldridge et al., 2016) were supplemented with the following appraisal items from the NICE quality appraisal checklist for quantitative intervention studies (NICE, 2012), given they capture additional information essential for consideration in meta-analyses:

1. Is the source population or source area well described?
2. Is the eligible population or area representative of the source population or area?
3. Do the selected participants or areas represent the eligible population or area?
4. Were interventions (and comparisons) well described and appropriate? Assessors were specifically asked to consider whether the intervention was: a) specific to third wave CBT or combined with other interventions not relevant to the review; b) manualised and comprehensive (e.g. covering all relevant content or methods) or unstandardised and incomplete (e.g. ACT interventions focused on defusion only); and c) carefully adapted to suit participants' developmental level or lacking evidence of this.
5. Was the study sufficiently powered to detect an intervention effect (if one exists)?

For each item from the NICE checklist, five ratings were possible:

Table 2.5: NIICE checklist ratings (obtained from NICE, 2012)

++	Indicates that for that particular aspect of study design, the study has been designed or conducted in such a way as to minimise the risk of bias.
+	Indicates that either the answer to the checklist question is not clear from the way the study is reported, or that the study may not have addressed all potential sources of bias for that particular aspect of study design.
-	Should be reserved for those aspects of the study design in which significant sources of bias may persist.
Not reported (NR)	Should be reserved for those aspects in which the study under review fails to report how they have (or might have) been considered.
Not applicable (NA)	Should be reserved for those study design aspects that are not applicable given the study design under review (for example, allocation concealment would not be applicable for case control studies).

The researchers assigned an overall NICE rating (++/+/-) based on the five individual ratings. It was decided a priori that additional weight would be given to the item capturing intervention quality, given concerns that third wave CBT has been poorly applied to children and adolescents, and the need to consider the impact of this in a review and meta-analysis aiming to determine effectiveness. For example, it has been argued that, in many instances, third wave CBT designed for adults has been "imported" to child populations without careful consideration of development (Hayes & Ciarrochi, 2015).

The overall rating from the Cochrane tool and NICE appraisal checklist were merged, and studies were given a final rating of high, moderate, or low quality:

- High: All or most of the criteria, across both the NICE and Cochrane checklists, scored well, where they have not met criteria the conclusions were very unlikely to alter or not meeting criteria were unavoidable.
- Moderate: Some of the criteria across both the NICE and Cochrane checklists have scored well, and where they have not, or haven't been adequately described, the conclusions were unlikely to alter.
- Low: Few or no checklist criteria have been fulfilled across either the NICE or Cochrane checklists, and the conclusions were likely or very likely to alter.

(Criteria were adapted from the NICE quality appraisal checklist for quantitative intervention studies, 2012).

Exploration of Cluster Randomisation Effects

Table 2.6

Main effects for primary outcomes with cluster randomised trials excluded

	k	Hedge's *g*	95% CI	Significance	Heterogeneity I^2 (*Q with p-value*)
Emotional symptoms and internalising problems	22	-0.28	-0.75 to 0.18	.236	94% (147.91, <.001)
Behavioural difficulties and externalising problems	13	-0.82	-1.31 to -0.33	.001	90% (96.52, <.001)
Interference from difficulties	11	-0.63	-1.33 to 0.08	.082	95% (208.20, <.001)
Third wave processes	10	0.74	0.38 to 1.10	<.001	79% (32.51, <.001)
Wellbeing and flourishing	10	0.77	0.27 to 1.27	.003	83% (47.67, <.001)
Quality of life	8	0.97	-0.12 to 2.06	.081	96% (64.39, <.001)
Physical health and pain	5	1.21	-0.05 to 2.46	.059	96% (115.63, <.001)

Note: k = number of studies in analysis; CI = confidence interval

Inspection of Publication Bias

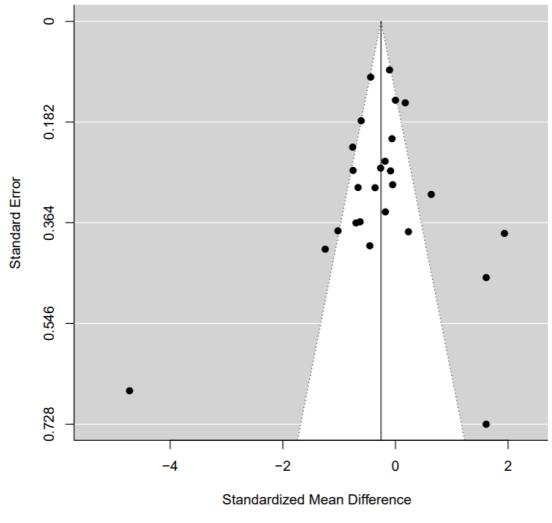
i) Table 2.7. Rank correlation tests of funnel plot asymmetry for primary and secondary outcomes at post-treatment*

Outcome	τ	Significance
Primary		
Emotional symptoms and internalising problems	-.02	.896
Behavioural difficulties and externalising problems	-.49	.011
Interference from difficulties	-.18	.459
Third wave processes	.42	.087
Wellbeing and flourishing	.55	.014
Quality of life	.39	.180
Physical health and pain	.33	469
Secondary		
Depression	-.31	.081
Anxiety	.03	.915
Acceptance	-.14	.773
Mindfulness	.43	.239

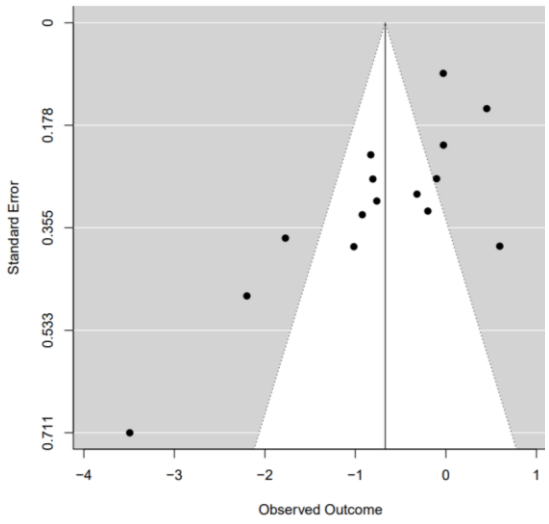
Note: τ = Kendall's tau
*Analyses were also conducted on follow-up data. These analyses yielded no significant asymmetry

ii) Figure 2.3: Funnel plots (random effects models) for primary outcome variables at post-treatment. Open circles (if any) show missing null studies estimated with the trim-and-fill method

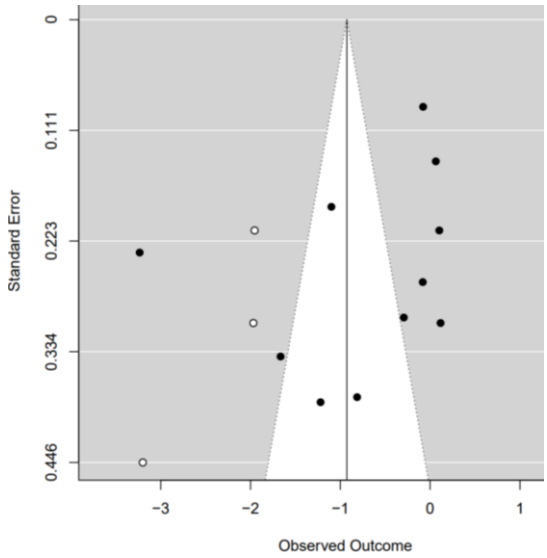
1. **Emotional Symptoms and Internalising Problems**



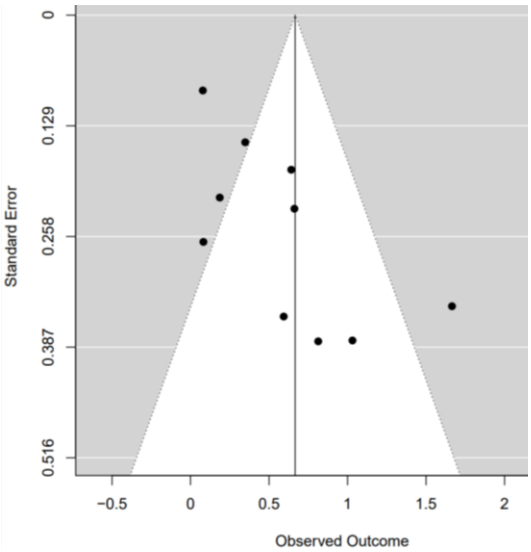
2. **Behavioural Difficulties and Externalising Problems**



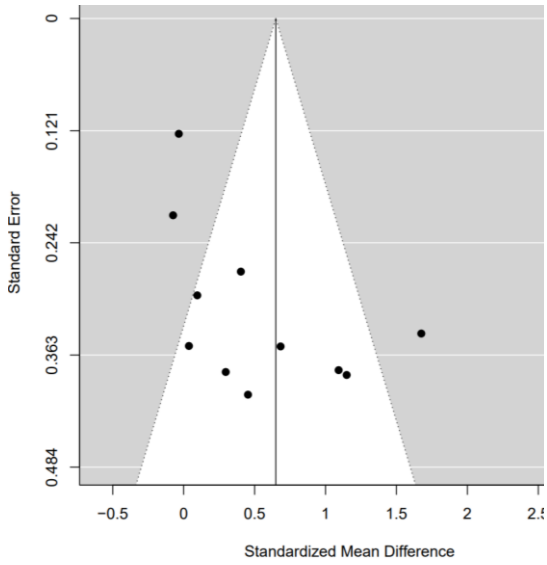
3. Interference from Difficulties



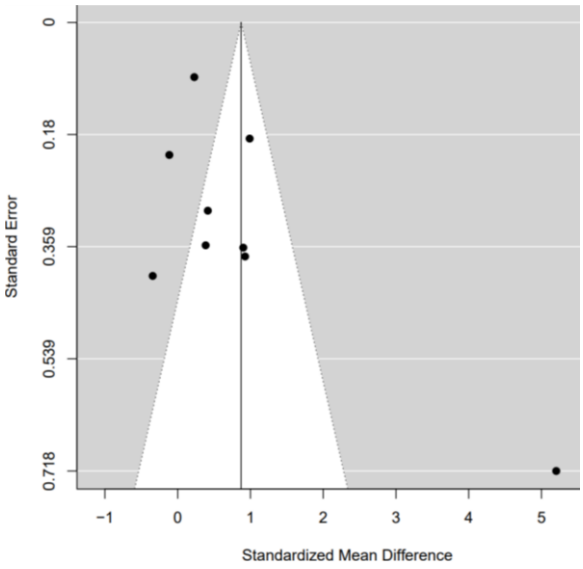
4. Third Wave Processes



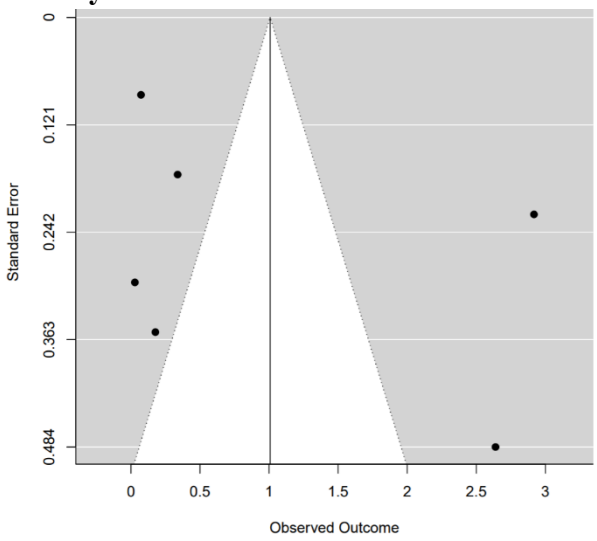
5. Wellbeing and Flourishing



6. Quality of Life



7. Physical Health and Pain



Although not presented here, there was no evidence of asymmetry or missing studies within the funnel plots for the secondary outcomes, or for the primary analyses at follow-up (except for quality of life, where it was estimated there was one missing null study).

CHAPTER THREE

Bridging Chapter

Bridging Chapter

The systematic review and meta-analysis presented in Chapter Two investigated the effectiveness of third wave cognitive behavioural therapies (CBT) for children and adolescents. Third wave CBT has been increasingly used within child and youth mental health provision, and this review was needed to ensure such treatments are effective. The interventions included were deemed applicable across diagnostic categories and along the continuum from ill-health to flourishing (Hayes & Hofmann, 2017), so a range of outcomes in both clinical and non-clinical settings were explored. The review therefore also aimed to contribute to research on universal approaches to improve health and wellbeing (Public Health England [PHE], 2019).

Overall results supported the application of third wave CBT in child and adolescent populations to target outcomes related to thriving as well as mental health symptomatology. Nonetheless, it was difficult to draw conclusions about the effectiveness of third wave CBT within non-clinical settings specifically, given potential bias from the inclusion of low quality research. Moreover, most studies delivered in communities and schools used sub-clinical or at-risk populations, rather than general samples. It was clear that more high-quality research is needed to explore the effectiveness of third wave CBT as a preventative and promotive strategy in non-clinical populations. Indeed, the review revealed that third wave CBT is often delivered to groups in a relatively short time frame, which is a promising characteristic for public interventions (PHE, 2019).

Psychologically-based mindset interventions also offer promise as a universal tool that could be delivered within educational settings to promote health and wellbeing.

Whilst currently focused on learning and the malleability of intelligence, mindset constructs have already been applied within schools (Dweck, 2007). Moreover, the psychologically-based mindset interventions that have emerged are psychoeducational, imparting knowledge to shape people's beliefs, with the view to subsequently impact behaviour (Schleider & Weisz, 2016). Psychoeducational interventions are quick, cheap to deliver, and align with existing pedagogies (Donker et al., 2009), but their use within the UK education system is yet to be investigated.

The empirical study presented in the next chapter aimed to assess the feasibility of a randomised controlled trial testing a psychologically-based mindset intervention in UK sixth forms and colleges. The intervention addressed mindsets of both trait-like and transient psychological factors (e.g. personality, thoughts, feelings); in line with previous research, it posited that neither were fixed and there is a potential for change and growth (Schroder, Dawood, Yalch, Donnellan & Moser, 2015; Schleider & Weisz, 2016). The intervention also incorporated ideas from third wave CBT approaches, including acceptance, non-judgement, and self-compassion, given evidence that malleability mindsets can increase self-blame or perfectionistic striving (Dweck, Chiu & Hong, 1995). The empirical study therefore allowed an exploration of third wave constructs within non-clinical, school samples to promote emotional health and wellbeing, which was identified as an area requiring further research in the systematic review and meta-analysis.

The intervention was dialectical and promoted balance between seemingly discordant constructs from conventional mindset interventions, such as growth and change, with those from third wave approaches, such as acceptance. Authors in the field have suggested that third wave CBT can co-exist with other therapeutic approaches and

have pushed towards integrative methods as a way to most effectively meet need (Hayes & Hofmann, 2017; 2019). The incorporation of change- and acceptance-based interventions may be particularly appropriate for young people who are developing but can feel pressured to strive towards happiness and self-actualisation (Hayes & Ciarrochi, 2015). The empirical study therefore also provides an exploration of a unique integrative and developmental approach.

CHAPTER FOUR

Empirical Study

Prepared for submission to Journal of Clinical Child and Adolescent Psychology

(Author guidelines in Appendix E)

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A Single-Session Mindset Intervention to Promote Adolescent Mental Health: A Randomised Controlled Trial Feasibility Study

Amorette M. Perkins[a], Joseph Cassidy[a], Gemma Bowers[b], Richard Meiser-Stedman[a] & Laura Pass[a]

[a]Department of Clinical Psychology and Psychological Therapies, University of East Anglia, Norwich Research Park, Norwich, Norfolk, NR4 7TJ, United Kingdom

[b]Norfolk and Suffolk NHS Foundation Trust, Mary Chapman House, Hotblack Road, NR2 4HN, Norfolk, United Kingdom

Correspondence to: Amorette Perkins

amorette.perkins@uea.ac.uk

+44 (0)1603 456 161.

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Abstract

Objective: Currently, up to 20% of young people worldwide have a diagnosable mental health condition, and an even greater proportion have sub-clinical symptoms and/or are at risk of developing difficulties. Mental health services are overstretched and there is a need to intervene early with universal interventions that can be delivered in schools. This study investigated the use of a single-session, computerised mindset intervention within educational settings to promote emotional health and wellbeing. **Method:** A feasibility study of a randomised controlled trial with parallel-groups was conducted. A general school sample of 80 adolescents were recruited (*M* age=16.63, 84% female, 81% White British) and allocated to the intervention (*n*=40) or usual educational curriculum (*n*=40). Feasibility data (e.g. uptake/attrition/participant feedback) were collected. Outcome measures were administered at baseline, post-treatment, 4-week, and 8-week follow-ups. **Results:** Minimum recruitment targets were exceeded. Attrition totalled 11% at 4-weeks then 48% at 8-weeks. Student feedback about the intervention and trial procedure was mainly positive. Participants' responses to tasks within the intervention indicated that they engaged with the content. Data were suggestive of possible intervention impacts on primary outcomes of personality mindset and psychological flexibility, with between-group differences which appeared small-large in effect size. Secondary outcomes of self-compassion, self-esteem, low mood, and anxiety also yielded some promising results. Few improvements appeared to be maintained at follow-up, but the sample was considerably reduced**.** No harms were reported. **Conclusions:** Overall, the intervention

and study design were deemed feasible, though several areas for improvement were noted. A full-scale evaluation is warranted to determine effectiveness.

A Single-Session Mindset Intervention to Promote Adolescent Mental Health: A Randomised Controlled Trial Feasibility Study

The emotional wellbeing of children and adolescents has immediate and long-term personal, social, and economic implications (World Health Organisation [WHO], 2003). Up to 20% of young people worldwide have a clinically significant mental health condition at present (WHO, n.d.) and an even greater proportion experience subclinical symptoms and/or are exposed to risk factors for developing difficulties (Public Health England [PHE] & Children and Young People's Mental Health Coalition, 2015). Evidence suggests that the majority of mental health conditions in adult life develop in childhood (Kessler et al., 2005). Consequently, there is a global agenda to protect and promote young people's mental health, to prevent conditions from developing in the first instance, and to engender positive emotional wellbeing among future generations (WHO, 2013). In the UK, government agencies have proposed adopting a "whole-school approach" to promote emotional public health using universally-applicable interventions (PHE & CYPMHC, 2015; PHE, 2019).

As yet there has been limited research of universal resources and evidence-based tools that could be used within educational settings (White, Lea, Gibb & Street, 2017; PHE, 2019). Lengthy interventions are costly, difficult to incorporate within the curriculum, and have a high risk of dropout. A recent meta-analysis suggested that mental health interventions delivered in a single-session are effective for youth, offering more accessible and cost-efficient alternatives (Schleider & Weisz, 2017). The review included self-administered interventions, which further decrease costs and enhance accessibility,

as they do not require a therapist present and could be managed by teachers. There is growing evidence supporting self-administered interventions delivered via computers in particular (Richards & Richardson, 2012; Davies, Morriss & Glazebrook, 2014).

The promise of brief mindset interventions

In a recent study by Schleider and Weisz (2016; 2018), youths age 12-15 years old, recruited from clinical and community samples in the United States for experiencing anxiety and/or depression, took part in a single-session, self-administered, computer-based personality mindset intervention. A mindset can be broadly defined as "the fundamental, core beliefs that individuals hold about the nature and malleability of various aspects of the human condition" (Ryan & Mercer, 2012, p.74). Earlier research suggested that youth who hold a "fixed" mindset, believing personal traits are unmalleable, are more likely to experience mental health problems than those with a "growth" mindset, who believe personal traits have the potential to change (e.g. Schleider, Abel & Weisz, 2015).

Thus, the psychoeducational intervention designed by Schleider and Weisz (2016) taught that personality is malleable, drawing upon evidence of neuroplasticity. Those who received the intervention reported greater improvements in perceived behavioural and emotional control, and experienced faster recovery from a social stressor post-intervention than an active control (Schleider & Weisz, 2016). At 9-month follow-up, there were no significant effects for emotional control and youth-reported anxiety, but more rapid and greater improvements in self- and parent-reported depression, alongside greater improvements in parent-reported anxiety, when compared to the control group (Schleider & Weisz, 2018).

Whilst Schleider and Weisz (2016; 2018) conducted the intervention in a laboratory environment with selected participants, mindset interventions also have potential as universal approaches to promote mental health within schools. Mindsets about self-characteristics are applicable to all and concepts like growth versus fixed are arguably easy to grasp, and therefore, accessible to young people. Indeed, these concepts were initially applied within educational settings, where the mindset literature arose two decades ago and focused on young people's beliefs about intelligence to improve learning, as opposed to their psychological beliefs to improve mental health (Dweck & Leggett, 1988; Dweck, Chiu & Hong, 1995).

In a study by Miu and Yeager (2015), school children completed a psychoeducational intervention positing that traits relating to bullying were malleable not fixed. The intervention was found to reduce the risk of developing depressive symptoms throughout the academic year. Other research has suggested that teaching high school students that socially-relevant personality characteristics are malleable, rather than fixed, may improve their ability to cope with stress (Yeager, Lee & Jamieson, 2016). Whilst having a limited scope (i.e. on bullying/socially-relevant traits), this highlights the potential of mindset interventions as promotive mental health tools within educational settings. Mindset interventions might be used to prevent a range of rigid and maladaptive self-beliefs from developing (e.g. about skills, self-worth, and character-traits), which have long been linked to the onset of mental health difficulties in leading psychological theories (e.g. Beck, 2011).

Beyond personality mindsets

Mindsets relating to emotion are just as or more highly correlated with mental health outcomes than personality mindsets (Schroder, Dawood, Yalch, Donnellan & Moser, 2015; 2016). Individuals believing that emotions are fixed are found to have slow recovery from stressors and poor coping strategies (Tamir, John, Srivastava & Gross, 2007; Kappes & Schikowski, 2013; Schroder et al, 2015). Mindsets of emotion have also been found to predict mental health outcomes overtime in US school children and college students; those believing that emotions are malleable experienced greater improvements in wellbeing, greater social adjustment, less loneliness, and fewer depressive symptoms compared to those endorsing a more fixed mindset of emotion (Tamir et al., 2007; Romero, Master, Paunesku, Dweck & Gross, 2014).

Research suggests that beliefs relating to other transient psychological experiences, such as thoughts or behavioural urges, also predict mental health (e.g. Wells & Papageorgiou, 1998; Hayes Luoma, Bond, Masuda & Lillis, 2006). Therefore, addressing mindsets relating to a broad range of transient psychological attributes in an intervention, alongside a broad range of trait-like or personality factors, could potentially produce better outcomes than having a narrow focus on one type of mindset alone. Moreover, transient and trait-like mindsets could be relatively easy to incorporate within a single intervention, given the common theme of encouraging a view of growth or flexibility.

Initial findings are promising for a school-based intervention incorporating a broad range of mindsets, namely regarding intelligence, self-control, and personality, which was developed by Schleider and colleagues whilst the current study was underway

(Schleider, Burnette, Widman, Hoyt & Prinstein, 2019). They found that their single-session intervention reduced depression over time for female adolescents from rural areas of the US. Whilst their intervention briefly mentioned the malleability of thoughts and feelings, this was not explored in depth. Moreover, the content about personality mindsets focused on self-confidence and social anxiety. Thus, there is still scope for further investigation of broader mindset interventions. Further, it is important to investigate the use of such interventions in other countries and populations.

Incorporating self-compassion and other "third wave" constructs to mitigate potential costs

There are potential costs as well as benefits to holding growth or malleability mindsets, and ways to mitigate these also need to be considered. If individuals believe personality traits are malleable but are not aware of their limitations, it could lead to perfectionistic striving and a sense of failure (Dweck, Chiu & Hong, 1995; Tamir et al., 2007). Similarly, believing that emotions are malleable can increase self-blame and decrease acceptance (Kneeland, Nolen-Hoeksema, Dovidio & Gruber, 2016). There is growing consensus in the psychological community that we cannot entirely control our transient psychological experiences, and that those who believe we can, are at greater risk of experiencing mental health difficulties (Harris, 2006).

Incorporating self-compassion within mindset interventions could therefore be beneficial. A growth mindset about trait-like factors could be promoted alongside self-kindness and acknowledgement of human imperfection or limitation. A compassionate mindset of transient factors might encourage the acceptance of difficult psychological

experiences, alongside recognition that - whilst we cannot entirely control the experiences themselves - we can choose how to respond to them (e.g. Harris, 2006; Gilbert, 2010; Neff & Tirch, 2013).

Notions about self-compassion and acceptance are pertinent within third wave therapies, which have been promoted for being transdiagnostic, applicable across the spectrum of ill-health to flourishing, and accordingly, useful within schools (Burckhardt, Manicavasagar, Batterham & Hadzi-Pavlovic, 2016; Hayes & Hofmann, 2017). Third wave interventions are also shown to be effective when brief and delivered remotely via the internet (e.g. Puolakanaho et al., 2019). Nonetheless, the possibility of integrating such constructs within computerised mindset interventions is yet to be explored.

Present study

This research study aimed to explore the feasibility of a novel mindset intervention as a promotive mental health tool for schools. Feasibility studies are the first phase of testing a novel intervention and its evaluation plan, to explore whether it can be implemented and is appealing and acceptable to participants (Orsmond & Cohn, 2015). Subsequently, judgements can be made about whether to pursue a full-scale trial, and whether there are ways to improve the intervention or study design (Bowen et al., 2009).

Specifically, the objectives of this research study were: 1) to explore whether a psychological mindset intervention incorporating transient and trait-like factors, that integrated self-compassion and other third wave constructs, was a feasible and acceptable tool to promote mental health within UK educational settings; 2) to determine whether

the proposed evaluation design for this intervention was feasible and acceptable; and 3) to investigate whether outcomes were indicative of positive change.

Method

Design

This was a feasibility study of a randomised controlled trial, with parallel groups and an intended allocation ratio of 1:1. As this was a feasibility study, the trial was not pre-registered.

Participants

Students aged 16-18 years within the UK education system were recruited, including from sixth forms and colleges. Typically, sixth forms are attached to high schools and offer advanced level qualifications, whilst colleges are separate institutions that offer a wider variety of courses, including vocational subjects. The age group was chosen given it encompasses a unique developmental period characterised by extensive change. It can be beneficial to offer interventions during times of transition (Durlak & Wells, 1997). Moreover, this is an age where a clearer sense of personal identity develops, alongside complex affective and cognitive skills (Christie & Viner, 2005).

Recruitment

To increase generalisability, multiple publicly- and privately-funded institutions were approached across two counties. Participant eligibility criteria were broad, considering the study's primary aim to assess the feasibility of an intervention that could be delivered using a whole-school approach. Mental health symptomatology and diagnosis did not serve as selection criteria. Exclusion criteria were lack of capacity and being involved in other school-based mental health research. In accordance with sample size recommendations for feasibility and pilot research, this study aimed to recruit a minimum of 50 participants (Cocks & Torgerson, 2013).

Teachers and other educational staff advertised the study to a range of classes and on their institutions' online learning portal. Students who gave consent to be contacted were provided with detailed study information and an opportunity to meet individually with the researchers to complete the consent process.

Randomisation

Participants were randomly allocated to either the control (usual school activities) or intervention using a block approach (Suresh, 2011). A person external to the research team generated an allocation sequence list from 1-80 using an online randomiser (www.sealedenvelope.com). Thus, neither the researchers nor participants were aware of group allocation until after enrollment.

Intervention

The intervention was a single, 30-minute session, delivered on the computer. Participants completed the intervention at their educational institution, during usual learning hours. They were excused from their normal timetabled activities, in which the control group remained. The intervention was self-administered and accessed through a weblink. Whilst delivered in a class setting, individuals worked independently at their own desktop.

The researchers developed the intervention with support from learning technologists, animators, and actors. Having obtained permission from Schleider and Weisz, it followed a similar format to their personality mindset intervention (2016; 2018), but with adapted and additional content, to reflect a broader focus on transient and trait-like psychological mindsets. In addition, the intervention aimed to balance ideas about change (i.e. growth mindset concepts), with ideas based in psychological models of acceptance and self-compassion (e.g. acknowledging human imperfection).

The intervention began with a 10-minute psychoeducational animation. The animation contained information about brain activity and neuroplasticity, which was based on neurological science, and supplemented with psychological theory from "first wave" and "second wave" cognitive behavioural therapies (e.g. Beck, 2011; Kays et al., 2012; Eysenck, 2013). In summary, the animation explained in simple terms that 1) thoughts, feelings, and behavioural urges result from activity between neurons; 2) there are links between thoughts, feelings, and behaviours (as well as bodily responses) that are neurologically-based; 3) we may observe patterns of the same thoughts, feelings, and behaviours, which have developed over time, influenced by our past experiences and

learning; 4) long-standing patterns can be construed as aspects of personality; and 5) changing our behaviour can change our psychological experiences, as well as patterns and our neurobiology over time.

Content from third wave cognitive behavioural approaches was integrated (e.g. Hayes et al., 2006; Gilbert, 2009). For example, the animation emphasised that change is not always possible, and compassion towards human imperfection and limitation was promoted. The intervention also encouraged acceptance of difficult psychological experiences as evolved or learnt responses intended to protect us, which are universal, inherently harmless, and result from temporary activity between neurons. Simultaneously, it was recognised that our psychological experiences can be biased and urge us to behave in ways that are unhelpful, but we can change our response to them to ensure we are living in accordance with values.

Following the animation, participants watched five minutes of videos depicting stories from fictional young people, describing how they used the content of the animation in their everyday lives or to cope with difficulties. This was followed by three multiple choice questions, which aimed to assess viewers' understanding of the content and their ability to apply it to familiar, "real-world" situations. Respondents were given feedback following each question, which reiterated the content from the animation and videos. To finish, participants were asked to type a "letter of advice" to a younger student experiencing anxiety and shyness, based on what they had learnt in the session. Participants were given approximately 15 minutes to complete the multiple choice questions and written task.

Ethics

Ethical approval for the study was granted by the University of East Anglia Faculty of Medicine and Health Sciences Research Ethics Committee (ref: 201819-045). All participants provided informed written consent. There were no gift/monetary incentives for participation. To ensure all participants could access the intervention, the control group were given an opportunity to complete it at the end of the study.

Data collection

Participant feedback and intervention responses

A structured feedback questionnaire using a 10-point Likert-type scale elicited participants' views and experiences of the intervention and trial procedure. This was created based on questionnaires from comparable trials (e.g. Ehlers et al., 2003). Participants' responses to the multiple-choice questions and written task within the intervention were recorded to further explore engagement and evaluate the mindset tool.

Outcomes

Personality mindset and psychological flexibility were primary outcomes and were measured at baseline, immediately post-treatment, then at 4-week and 8-week follow-ups. Secondary outcome measures for self-compassion, self-esteem, low mood, and anxiety were administered at baseline and follow-ups only.

Personality mindset. Three items from the Implicit Personality Theory Questionnaire (IPTQ) were used to assess respondents' views on personality as fixed or malleable (Yeager, Miu, Powers & Dweck, 2013). These self-report items were used by Schleider and Weisz (2016) to assess their mindset intervention. They were: "You have a

certain personality, and it is something that you can't do much about", "Your personality is something about you that you can't change very much", and "Either you have a good personality or you don't, and there is really very little you can do about it". Items are rated on a Likert-type scale from 1 (really disagree) to 6 (really agree), with higher scores suggesting more fixed mindsets. In Schleider and Weisz's (2016) adolescent sample, reliability for these items was reported at an average of $\alpha = 0.82$.

Psychological flexibility. The Acceptance and Fusion Questionnaire for Youth-Short Form (AFQ-Y8; Greco, Lambert & Baer, 2008) was used to assess psychological flexibility. The AFQ-Y8 is an 8-item self-report measure, rated using a Likert-type scale from 0 (not at all true) to 5 (very true). The measure does not have a clinical cut-off score. Lower total scores indicate greater psychological flexibility. The AFQ-Y8 is validated for use with adolescent populations (Greco, Lambert & Baer, 2008; Szemenyei et al., 2018). Reliability has been previously reported as $\alpha = 0.83$ (Greco, Lambert & Baer, 2008). The measure contained items reflective of the transient psychological mindsets promoted in the intervention (e.g. "I am afraid of my feelings"). It also captured third wave constructs like acceptance and values-accordant behaviour.

Self-compassion. The Self-Compassion Scale–Short Form was used (SCS-SF; Raes, Pommier, Neff & Van Gucht, 2011). It is a 12-item self-report measure, which uses a Likert-type scale ranging from 1 (almost never) to 5 (almost always). There is no cut-off score. Higher total scores indicate higher self-compassion. The 26-item version (Neff, 2003) is a valid and reliable measure among adolescents (Cunha, Xavier & Castilho, 2016). The SCS-SF is more time-efficient and has a near-perfect correlation with the 26-

item version (Raes et al., 2011). Reliability has been reported as α = 0.86 in a student sample (Raes et al., 2011).

Self-esteem. The Rosenberg Self-Esteem Scale (RSES; Rosenberg, 1965) was used, which is a 10-item self-report measure using a Likert scale ranging from 1 (strongly agree) to 4 (strongly disagree). A higher total score indicates higher self-esteem. The scale was developed for use with adolescents and has good psychometric properties (Blascovich & Tomaka, 1991). Amongst British 16-18 year olds, reliability has been reported to average α = 0.86 (Bagley & Mallick, 2001).

Low mood and anxiety. The Revised Children's Anxiety and Depression Scale-Short Version (RCADS-25; Ebesutani et al., 2012) was used to assess low mood and anxiety. This is a 25-item self-report measure, which uses a Likert-type scale from 0 (Never) to 3 (Always). Higher scores are suggestive of higher symptoms. Cut-off scores are given, to indicate sub-clinical and clinical anxiety, depression, and combined anxiety/depression. The measure is validated for ages 8-18 years and has good psychometric properties (Ebesutani et al., 2012). Reliability has been previously reported as α = 0.86 for 16-18 year olds (Piqueras, Martin-Vivar, Sandin, San Luis & Pineda, 2017). Amongst school samples, alpha is reported as 0.86 and 0.79 for the anxiety- and depression-subscales respectively (Ebesutani et al., 2012).

Analysis

Feasibility and acceptability

Feasibility indicators such as recruitment and retention rates, reasons for drop-out, and completion of the intervention were recorded. The percentage of missing data and its

randomness (indicated by the distribution of missing values) was assessed. Feedback questionnaire responses were analysed descriptively.

The percentage of correct answers to the multiple-choice questions in the intervention were calculated. Participants' responses to the written task were subject to content analysis as described by Erlingsson and Brysiewicz (2017). When the control group completed the intervention at the end of the study, their responses to the multiple choice and written tasks were pooled with those of the intervention group to increase the data sample.

Outcome data

Null-hypothesis significance-testing is inappropriate for feasibility studies as they are insufficiently powered (Orsmond & Cohn, 2015). Therefore, to determine the suitability and sensitivity of outcome measures and explore potential intervention effects, means and standard deviations were calculated, then effect sizes were estimated for between-group differences across time points. Effect sizes of 0.2, 0.5, and 0.8 were interpreted as small, moderate, and large, respectively (Fritz, Morris & Richler, 2012). If missing data were 20% or less, person mean imputation was used (Downey & King, 1998). Where participants were missing more data than this, they were omitted from the corresponding analyses. Participants were analysed according to the group they were originally assigned. When participants dropped out, however, they were excluded from analyses thereafter.

The percentage of participants in each group demonstrating reliable change, as measured using reliable change indexes, were computed (Jacobson & Truax, 1991). The percentage of participants experiencing change was also assessed using distribution- and

anchor-based methods (Lee, Whitehead, Jacques & Julious, 2014; Musoro et al., 2018). This was given suggestions that reliable change indexes may be insensitive to effects of preventative or promotive interventions in non-clinical samples (Hawley, 1995).

For distribution-based methods, the one-half standard deviation benchmark was used to indicate minimal important difference (MID) of clinical value (Revicki, Hays, Cella & Sloan, 2008). Anchor-based values were calculated by relating scores on outcome measures to participants' self-reports of change. Self-reported change was assessed at the final follow-up using a questionnaire designed by the researchers, containing nine items (or anchors) measured on a 7-point Likert-type scale, which each corresponded to a primary or secondary outcome (Johnstone et al., 2015). MID was defined as the mean difference for an outcome score that is derived from participants reporting a small degree of change on the relevant anchor(s) (Revicki et al., 2008).

Given that a limited sample size was expected, it was decided *a priori* that only where change (as assessed by reliable change indexes or distribution-/anchor-based methods) occurred for more than 10% of participants, and where groups differed by more than 10%, would it be highlighted in the text. This is because percentages <10% represented only 1-3 participants and, with such small numbers, differences over time or between groups may be more attributable to external factors rather than a result of treatment allocation.

Results

Sample characteristics

A total of 80 students consented to participate in this study. The majority were female (84%). Most were White British (81%); 9% reported mixed ethnicity, 6% were European, and 4% were Asian. The average age of participants at entry to the study was 16.63 years (*SD* = 0.56). Twenty-three percent of participants scored above clinical threshold for the total RCADS-25 scale at baseline, whereas 10% scored above threshold for the anxiety-subscale specifically, and 26% scored above threshold for the depression-subscale. Baseline characteristics per treatment arm are provided in Table 4.1.

Table 4.1

Baseline characteristics by treatment arm

	Intervention *(n* = 40*)*	Control *(n* = 40*)*
Mean child age (SD)	16.60 (0.55)	16.65 (0.58)
Ethnicity - n (%) White British	30 (75.00)	35 (87.50)
Sex - n (%) female	34 (85.00)	33 (82.50)

Feasibility

Recruitment, retention, and timescales

Thirteen sixth forms and colleges were approached to host this study. Several of these sites initially expressed interest but teaching staff were unable to provide sufficient time or resources to participate. Some thought their students were too busy to be involved. In the end, three sites provided gatekeeper consent. All were state-funded; two were sixth

forms (one located in a city and another in a rural market town) and one was a college (located in a coastal town). However, no participants were consented from the college; educational staff reported that many students were part-time and this was a barrier to participation, as they were unlikely to be on site for all follow-ups, and that the concept of research may have been unfamiliar to students completing vocational courses.

Recruitment of participants started in May 2019 and ended in October 2019 when minimum participant numbers were reached. The host sites advertised the study to students for approximately two weeks. Estimates for the total number of students who received information about the study during this time were unknown, though there were over two-thousand 16-18 year olds across the three sites. One-hundred and twenty-eight young people agreed to be contacted by the researchers. After reading the participant information sheets, 80 students remained interested in taking part and gave consent (Figure 4.1).

All participants provided baseline data. Attrition rates accumulated to 3% at post-treatment, 11% at the 4-week follow-up, then 48% at 8-weeks. One participant reported that they dropped out because they simply did not want to continue. The remaining participants did not attend follow-up but gave no reason for this. Nonetheless, 90% of the participants lost at final follow-up were from one institution; educational staff reported that this follow-up fell during the final week of teaching before the summer holiday, and that most of their students finished earlier than expected for work experience, or were attending a careers day.

The whole research process (including consent procedures, delivery of the intervention, and follow-up data collection) took participants approximately 2-3 hours.

Figure 4.1: Consort diagram

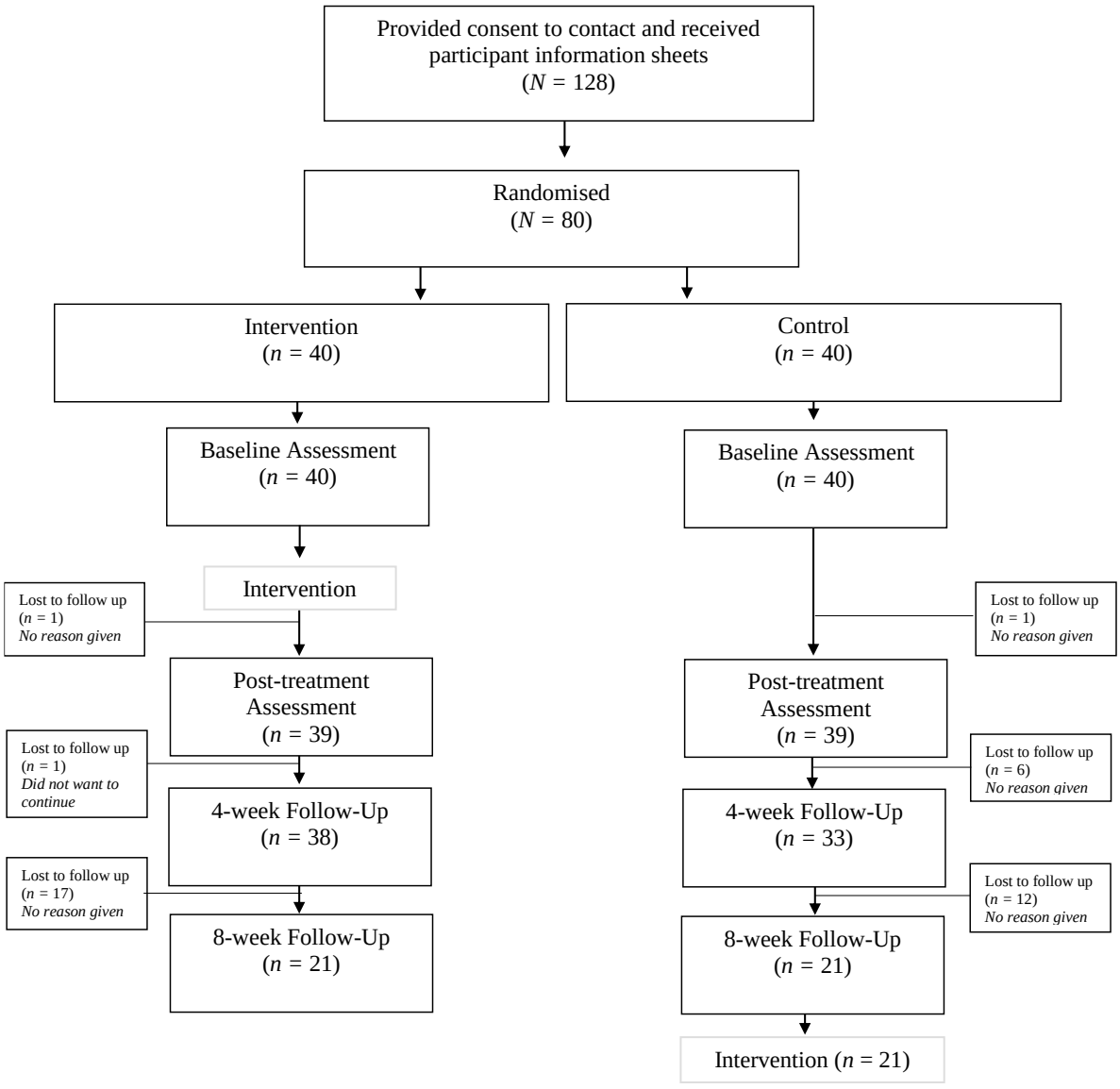

Figure 4.1. Flowchart (CONSORT) of participants

Intervention engagement and completion

All participants in the treatment arm (n = 40), and those remaining in the study at 8-weeks from the control arm (n = 21), completed the intervention. The researchers observed that all participants appeared focused and engaged whilst on the computer. Most finished the intervention within 20 to 30 minutes. There were no reports of distress or harm to participants.

Missing data

All the students who attended the final follow-up completed feedback questionnaires (n = 42). Responses to the multiple choice questions during the computer session, and the letter writing task, were available for 58 and 59 participants out of the 61 who completed the intervention, respectively; three participants had technical issues meaning they were unable to save some/all of their answers. For the outcome measures, data was missing for less than 1% of total responses across time points. It appeared randomly distributed.

Participant feedback

Average scores for items related to the intervention in the feasibility questionnaire were as follows on a scale from 1 (definitely do not agree) to 10 (definitely agree): “The mindset session made sense to me” (M = 7.76, SD = 1.46), “The mindset session was hard to complete on the computer” (M = 3.00, SD = 2.01), “I think the mindset session has been (or will be) helpful for me” (M = 6.31, SD = 1.81), “I would recommend the mindset session to a friend or family member” (M = 6.79, SD = 1.83), and “I found the mindset session boring” (M = 3.86, SD = 2.03). For research-related items, average scores were: “I understood what the questionnaires were asking me” (M = 7.86, SD = 1.70), “The

questionnaires took too long to complete" (*M* = 4.00, *SD* = 2.14), "I did not like being put in different groups at random" (*M* = 2.74, *SD* = 2.07), and "I enjoyed taking part in this research study (*M* = 7.98, *SD* = 1.49).

Comprehension checks

The large majority (97%) of responses to the multiple choice questions were correct. With regard to the writing task, the most prominent themes among participants' letters of advice were: 1) acceptance of thoughts and feelings; 2) self-determination and control; 3) change is possible; 4) doing something different is key to change; and 5) the importance of self-compassion and other people. Table 4.2 provides a descriptive summary of each theme with illustrative quotes from participants. Themes were closely aligned with the content of the animation. Participants also wrote about novel but related ideas. For example, that we are not "defined by" - but more than – our thoughts and feelings.

Table 4.2

Themes identified within participants' responses to the writing task

Theme	Description	Examples
Acceptance of thoughts and feelings	• Difficult thoughts/feelings are normal, common across humanity, and not shameful • Thoughts/feelings are not always accurate or helpful • We cannot control the thoughts/feelings that arise • Difficult thoughts/feelings are influenced by our past experiences and are our brain's way of protecting us	• "Feeling nervous or anxious is a natural response to new situations…" • "Your brain is being an overprotective friend that doesn't want you to get hurt" • "Listen to your brain's input, but don't take its word as the gospel truth." • "Don't feel ashamed about being nervous… there is nothing wrong with you…"
Self-determination and control	• We are not defined by our thoughts/feelings • We can decide who we are and what we do in life • We do not have to listen to difficult thoughts/feelings but can choose how to respond • Do what you value in life despite difficult thoughts/feelings • Seize opportunities	• "It is important to acknowledge these feelings, but you shouldn't let them define you" • "You – as a person – are more than negative emotions" • "You cannot control how your brain feels… but you CAN control the response you give towards this feeling" • "Do the things that are important to you… Life's too short"
Change is possible	• Thoughts/feelings/urges are not fixed but fleeting • Patterns and personality can change over time • We can grow • The brain is like a muscle and changes	• "Thoughts, emotions and urges come and go…" • "… we are all constantly growing and evolving" • "You can be whoever you want to be… the opportunity to recreate yourself" • "… new, stronger connections are formed between the neurons in your brain"
Doing something different is key to change	• Changing how we respond to difficult thoughts/feelings can change these thoughts/feelings over time • Doing new things can bring about personal growth	• "…sometimes the way we grow is by doing exactly what we are scared to do" • "… don't let the thoughts or feelings stop you as it is the way in which you react to them that determines how your life continues" • "Shyness and nervousness may be strong now but if you face them head on you will surely get better with time"
The importance of self-compassion and other people	•Doing different is not easy and requires us to step outside our comfort zone • Change takes time and should be approached step-by-step • Life can be hard • Change is not always possible and humans are imperfect • Seeking support from others can be helpful ● It is okay to be different • You are important and worthy	• "It [change] will be slow and laborious." • "… your brain will adapt, it will take time, and maybe sometimes it won't work…" • "Don't push yourself too hard and always be kind to yourself" • "Seek help when needed and don't be afraid to talk about it" • "You are worth it and deserve good things."

Outcomes

Between-group mean differences

Table 4.3 presents group means at baseline, post-treatment, and follow-up, alongside effect size estimations of between-group differences.

Primary outcomes. Small differences favouring the intervention group were apparent at baseline for the IPTQ and AFQ-Y8. At post-treatment, the intervention group continued to outperform the control group on the IPTQ and AFQ-Y8, yet differences were now estimated to be moderate-large in effect size. Moderate differences favouring the intervention group for the primary outcomes were maintained at 4-week follow-up. By 8-weeks, moderate differences favouring the intervention group were apparent for the IPTQ, alongside small differences for the AFQ-Y8.

Secondary outcomes. Small differences were only apparent at baseline for the RCADS-25 (total and anxiety-subscale), which favoured the intervention group. At 4-week follow-up, however, the intervention group outperformed the control group (with effect sizes that were small in magnitude) for all secondary outcomes, including the SCS-SF, RSES, and RCADS-25 (all subscales). By 8-weeks, small differences favouring the intervention group were only found for the RCADS-25 (total and anxiety-subscale).

Table 4.3

Between-group mean differences at baseline, post-treatment, and follow-ups

		Average score (SD)		ES of between-group difference (95% CI)
		Intervention	Control	
IPTQ	Baseline	9.35 (2.33)	9.97 (3.21)	**-0.22** (-0.66 to 0.22)
	Post-Trt	5.59 (2.09)	10.03 (3.03)	**-1.69** (-2.21 to -1.17)
	4-wk FU	7.58 (2.72)	9.79 (3.09)	**-0.75** (-1.24 to -0.27)
	8-wk FU	7.71 (2.78)	9.33 (2.99)	**-0.55** (-1.17 to 0.07)
AFQ-Y8	Baseline	11.68 (5.49)	13.18 (6.40)	**-0.25** (-0.69 to 0.19)
	Post-Trt	8.31 (5.14)	12.31 (6.67)	**-0.67** (-1.12 to -0.21)
	4-wk FU	10.29 (4.88)	13.70 (7.08)	**-0.56** (-1.04 to -0.09)
	8-wk FU	10.67 (5.41)	13.20 (7.19)	**-0.39** (-1.00 to 0.22)
SCS-SF*	Baseline	33.03 (7.48)	32.78 (8.45)	0.03 (-0.41 to 0.47)
	4-wk FU	36.16 (7.00)	33.00 (6.53)	**0.46** (-0.01 to 0.93)
	8-wk FU	33.00 (6.72)	33.14 (7.45)	-0.02 (-0.62 to 0.59)
RSES*	Baseline	25.49 (4.37)	24.85 (5.25)	0.13 (-0.31 to 0.57)
	4-wk FU	26.00 (4.03)	24.52 (4.04)	**0.36** (-0.11 to 0.83)
	8-wk FU	25.81 (4.90)	24.90 (4.55)	0.19 (-0.42 to 0.79)
RCADS-25	Baseline	24.43 (10.90)	27.26 (10.81)	-**0.26** (-0.70 to 0.18)
	4-wk FU	22.57 (9.71)	26.94 (10.33)	-**0.43** (-0.90 to 0.04)
	8-wk FU	25.26 (11.44)	27.67 (10.70)	**-0.21** (-0.82 to 0.39)
Anxiety	Baseline	12.50 (6.06)	14.31 (6.53)	-**0.28** (-0.72 to 0.16)
	4-wk FU	11.03 (5.38)	13.58 (6.09)	-**0.44** (-0.91 to 0.03)
	8-wk FU	12.40 (6.07)	14.33 (6.52)	**-0.30** (-0.91 to 0.31)
Depression	Baseline	11.93 (6.02)	12.95 (5.44)	-0.18 (-0.62 to 0.26)
	4-wk FU	11.49 (5.93)	13.36 (5.66)	**-0.32** (-0.79 to 0.15)
	8-wk FU	13.10 (6.17)	13.33 (5.60)	-0.04 (-0.64 to 0.57)

Note: Post-Trt = post-treatment; wk = week; FU = follow-up; ES = effect size (Hedge's g); CI = confidence interval. Small-large effect sizes are denoted in bold. For measures marked with an asterisk, a positive ES is favourable. For all other measures, a negative ES is favourable.

Baseline *n*=40 per group; post-trt *n*=39 per group; 4-wk FU intervention *n*=38/control *n*=33; 8-wk FU *n*=21 per group

Measures: IPTQ = Implicit Personality Theory Questionnaire; AFQ-Y8 = Acceptance and Fusion Questionnaire for Youth–Short Form; SCS-SF = Self-Compassion Scale–Short Form; RSES = Rosenberg Self-Esteem Scale; RCADS-25 = Revised Children's Anxiety and Depression Scale–Short Version; Anxiety = RCADS-25 Anxiety-Subscale; Depression = RCADS-25 Depression-Subscale

Assessment of change

The percentage of participants demonstrating change between baseline and post-treatment/follow-ups, as assessed by reliable change indexes and distribution-/anchor-based methods, are displayed for each treatment arm (Table 4.4).

Reliable change indexes

Primary outcomes. A greater percentage of participants in the intervention group compared to the control group demonstrated reliable improvement at post-treatment for the IPTQ and AFQ-Y8. At 4-weeks and 8-weeks, substantial differences favouring the intervention group were observed for the IPTQ only.

Secondary outcomes. At 4-weeks, a greater percentage of participants in the intervention group compared to the control group demonstrated reliable improvement on the SCS-SF. No other reliable differences (≥10%) were observed.

Distribution- and anchor-based methods

Primary outcomes. At post-treatment, a higher percentage of participants in the intervention group compared to the control group demonstrated clinically-important improvement (and less deterioration) on the IPTQ and AFQ-Y8. Differences (≥10%) favouring the intervention group continued to exist for both outcomes at 4-weeks, but only for the AFQ-Y8 at 8-weeks.

Secondary outcomes. At 4-weeks, a greater proportion of participants in the intervention group compared to the control group demonstrated clinically-important improvement (and less deterioration for some outcomes) on the

SCS-SF, RSES, and RCADS-25 (total and depression-subscale). At 8-weeks, a greater proportion of participants in the intervention group compared to the control group demonstrated improvement (and less deterioration for some outcomes) on the RSES and RCADS-25 (total and anxiety-subscale). Conversely, a higher percentage of participants in the control group compared to the intervention group demonstrated improvement, and less deterioration, on the SCS-SF at final follow-up.

Table 4.4

Percentage of participants demonstrating change per treatment arm

		Post-treatment (Intervention, *n* = 39; Control, *n* =39)						4 weeks (Intervention, *n* = 38; Control, *n* = 33)						8 weeks (Intervention, *n* = 21; Control, *n* = 21)					
		RCI		Distribution		Anchor		RCI		Distribution		Anchor		RCI		Distribution		Anchor	
		+	-	+	-	+	-	+	-	+	-	+	-	+	-	+	-	+	-
IPTQ	I	**44**	0	**77**	0	**77**	0	**24**	3	**58**	13	**58**	13	**19**	5	43	10	38	10
	C	0	0	36	**21**	15	5	3	3	30	18	30	18	5	5	38	19	38	19
AFQ	I	**21**	0	**72**	3	**87**	5	8	3	**37**	21	53	39	10	10	**48**	14	57	29
	C	3	0	14	**13**	14	**23**	6	6	27	24	52	39	5	19	29	19	52	29
SCS-SF	I							**18**	0	**47**	26	**55**	26	14	5	38	33	38*	43*
	C							6	6	27	21	39	**36**	5	0	43	24	48	29
RSES	I							0	0	**32**	29	**32**	26	5	5	**48**	29	**48**	29
	C							3	3	18	21	18	21	5	0	33	24	33	24
RCADS-25	I							8	3	**42**	18	**47**	29	10	5	24	14	**43**	14
	C							6	0	27	27	27	27	5	0	19	**29**	24	**38**
Anxiety	I							5	5	42	11	51	21	14	0	**43**	19	**67**	19
	C							3	3	42	18	42	18	10	0	24	**33**	24	**33**
Depression	I							5	5	**39**	26	**39**	26	5	0	19	24	29	29
	C							6	0	21	**42**	21	**42**	5	0	14	24	29	29

Note: I = intervention group; C = control group; n = number of participants; + = positive change/improvement; - = negative change/deterioration. The reliable change index (RCI) represents the percentage of participants where change in scores is considered statistically unlikely due to measurement error; distribution-based methods represent the percentage of participants experiencing a change in scores ≥ half a standard deviation; anchor-based methods represent the percentage of participants experiencing a change in score ≥ the mean difference derived from participants reporting a small degree of change on the corresponding anchor
Where the percentage of participants experiencing change is ≥10% and the difference between groups is ≥10%, results favouring the intervention group are denoted in bold, whilst results favouring the control group are marked with an asterisk
Measures: IPTQ = Implicit Personality Theory Questionnaire; AFQ-Y8 = Acceptance and Fusion Questionnaire for Youth–Short Form; SCS-SF = Self-Compassion Scale–Short Form; RSES = Rosenberg Self-Esteem Scale; RCADS-25 = Revised Children's Anxiety and Depression Scale–Short Version; Anxiety = RCADS-25 Anxiety-Subscale; Depression = RCADS-25 Depression-Subscale

Discussion

Feasibility

The findings of the current feasibility trial suggested that a single-session psychological mindset intervention which incorporates transient and trait-like factors, while emphasising third wave constructs such as acceptance and self-compassion, could be a feasible and acceptable tool for whole-school implementation to promote mental health. All participants in the treatment arm successfully completed the computer session. Students' responses on the feedback questionnaires were mainly positive, with most indicating that they found the intervention somewhat helpful and might recommend it to friends and family. Participants correctly responded to the multiple-choice questions of the intervention, suggesting that the content was understood. Moreover, themes identified in the participants' letters of advice married closely with the mindset constructs promoted in the intervention (e.g. recognition of growth alongside acceptance of difficult thoughts and feelings). Participants re-phrased content and included novel ideas, suggesting some depth of information processing (Craik & Lockhart, 1972).

The evaluation design was also feasible and acceptable. The minimum recruitment target was exceeded in a relatively short amount of time. Attrition rates for randomised trials are expected but bias may occur when rates exceed 20% (Marcellus, 2004). Attrition at the 4-week follow-up totalled 11%. Whilst this increased to 48% at 8-weeks, the majority of participants were lost due to an unexpected timetabling conflict at one educational institution. For participants who were retained, missing data was negligible. This was consistent with findings on the student feedback form, suggesting that most

understood how to complete the outcome measures. Students expressed that they were happy with randomisation. There were no reports of harm and participants expressed that they enjoyed taking part in the research.

Possible intervention effects

Significance testing was inappropriate so results are indeterminate. Nonetheless, outcome data was suggestive of positive changes in the targeted mechanisms. At post-treatment, a large superiority effect favouring the intervention group was found for personality mindset, alongside a moderate superiority effect for psychological flexibility. Moderate differences favouring the treatment arm remained for both primary outcomes at the 4-week follow-up. By 8-weeks, effect sizes were small for psychological flexibility, while moderate differences were maintained for personality mindset. Although baseline differences existed in favour of the intervention group, these were smaller in magnitude than the differences observed between groups at post-treatment and both follow-ups. Moreover, assessment of reliable and clinically-important change, which accounted for baseline scores, favoured the treatment arm.

The intervention may have also had positive impacts on the secondary outcomes. Whilst group differences for self-compassion, self-esteem, and low mood were negligible at baseline, small effects favouring the intervention group were apparent for all these variables at the 4-week follow-up. For anxiety, small baseline differences existed in favour of the intervention group; at 4-weeks, differences grew in magnitude but were still categorised as small. While assessment of reliable change at 4-weeks favoured the intervention group for self-compassion only, a higher percentage of the intervention group

compared to the control group demonstrated clinically-important differences for all secondary outcomes at this follow-up, except anxiety.

Nonetheless, effect sizes for between-group mean differences in the secondary variables had reduced to near baseline or were negligible by 8-weeks. Assessment of reliable change also yielded no substantial variation between groups. Findings for clinically-important changes were mixed, with results favouring the intervention group for self-esteem and anxiety while the control group for self-compassion, and relatively small compared to other time points (10% represented only two participants at final follow-up). Overall, this could suggest that any effects for secondary outcomes were not maintained. Nevertheless, it should be noted that almost half of participants were lost by the final follow-up, increasing the risk of bias and making it difficult to draw conclusions. A large, well-powered study could be needed to detect maintenance effects.

Implications and limitations

Overall findings suggested that the intervention and trial design were feasible. Moreover, outcome data were promising, especially for personality mindset and psychological flexibility. A full-scale randomised controlled trial is therefore warranted. There are, nevertheless, potential barriers to overcome for future trials.

Only three of the 13 host sites approached were recruited, either because educational staff could not spare the time or resources, or because it was felt that students were too busy. Moreover, some population groups were underrepresented, with most participants being females, and none from a college setting. It may therefore be beneficial to think about outreach strategies; for example, offering incentives for participation and

visiting education settings (especially colleges) in advance of recruitment to build rapport (Diaz, 2012) Alternatively, cluster-randomisation could be used to increase sample generalisability, whereby the intervention is delivered to a whole class in place of a lesson, excluding only students who opt out and do not consent to data collection.

It is also important to note that it remains unclear whether the intervention and study design are applicable across a wider child-youth age group, given that only students aged 16-18 years were recruited. The academic abilities and socioeconomic status of the participants were unknown, which may be useful additional data to collect in future trials. Given participation was voluntary, this sample may have been particularly motivated to understand and use the intervention for their benefit. Thus, estimated effects may be larger than expected in a general school population (Ng et al., 2012).

Students expressed that completing outcome measures was time-consuming. A reduction in burden would come from the removal of the self-reported change measure and feedback questionnaire, which were necessary for the feasibility assessment but not a full-scale trial. Moreover, some of the secondary outcome measures could be removed. It might be helpful to design a reliable measure of mindsets related to transient psychological factors. In the current study, evaluation for this key outcome relied on a measure of psychological flexibility as no alternative existed, but this may not be valid; the measure was designed to assess third wave, and not mindset, interventions.

Although analyses remained limited given that the current study aimed only to assess feasibility, a strength of the research design was the use of multiple methods to assess change over time, alongside estimates of between-group mean differences. Given the potential insensitivity of reliable change indexes to promotive interventions, and

because statistically-significant and clinically-meaningful change are arguably different constructs, distribution- and anchor-based methods were utilised (Hawley, 1995). Anchor-based methods are particularly upheld because they consider change as perceived by participants (Johnstone et al., 2015).

It is important to note, however, that the self-report measure used to facilitate anchor-based calculations was not pre-piloted, and almost half of participants dropped out before it was administered, meaning that estimations of minimal clinically-important difference may have been biased. Moreover, whilst differences between groups regarding the proportion of participants demonstrating reliable and clinically-important change over time were only highlighted when ≥10%, this may still reflect a small number of students, and thus not all findings were necessarily meaningful. Changes which were relatively large and consistent across methods (e.g. reliable and distribution-/anchor-based assessments) may be interpreted more confidently.

Including more interactive components and/or a break between the animation and stories from young people could improve the intervention, as 15-minutes of psychoeducation requires prolonged concentration (Bruce, Flens & Neiles, 2010). Moreover, feedback suggested that participants may have become bored. It may also help to involve teachers, equipping them to facilitate a full lesson around the 30-minute intervention, so that students have space to further discuss the content. Evidence suggests that mindsets may be shaped through day-to-day interactions over time (Mueller & Dweck, 1998), and that students internalise the mindsets of their teachers (Rattan, Good & Dweck, 2012); thus, the inclusion of educational staff could be important. Additional

intervention sessions could also be beneficial, with results suggesting that positive effects may diminish over time, especially for secondary outcomes.

However, any extension to the intervention would need careful consideration as one of its most appealing qualities was its brevity. Moreover, single-session mental health interventions have yielded significant effects for young people in previous research (Schleider & Weisz, 2017) and initial results were promising for the current study. Small effects might be expected for brief promotive interventions, but even slight changes could have wide-reaching consequences at a population-level.

Conclusion

This study explored a novel single-session mindset intervention delivered on the computer to 16-18 year old students within UK educational settings. The intervention and research design appeared feasible and acceptable to participants, though several areas for improvement were noted. Given this was a feasibility evaluation, firm conclusions cannot be drawn about intervention effects, however outcome data were promising. Analyses were indicative of positive changes for primary outcomes of personality mindset and psychological flexibility. Secondary outcomes of self-compassion, self-esteem, low mood, and anxiety also yielded some encouraging results. Whilst few improvements appeared to be maintained by the 8-week follow-up, the sample was potentially biased and/or underpowered. Given this intervention can be delivered within 30 minutes, has minimal cost, and requires limited resource, it is worth pursuing a full-scale evaluation to

determine its effectiveness for implementation as a universal, promotive mental health tool.

Disclosure statement

No potential conflicts of interest were reported by the authors.

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CHAPTER FIVE

Additional Methodology

This chapter contains information about methods that could not be included within publications due to restrictions on word counts/tables for the selected journals.

Additional Methodology

Part One: Systematic Review and Meta-Analysis

Missing data

Where the data required for meta-analyses were not wholly reported within a trial publication that met inclusion for the review, the corresponding author for the trial was contacted via email. If no response was received, additional authors were contacted where possible. In total, 14 authors were contacted regarding nine studies. Authors were given at least two weeks to provide the information required, before a study was excluded from analysis.

For these nine studies, full data were obtained for four trials; two trials were included for only some (not all) of the outcomes they assessed, due to partial provision of data; and three trials had to be totally excluded due to a complete lack of necessary data.

Correction for multiple comparisons

The Holm-Bonferroni method was applied to moderation and subgroup analyses of primary outcomes at post-treatment and follow-up, in order to correct for multiple comparisons. The procedure was as follows (Holm, 1979):

1) P-values were sorted in ascending order (per outcome variable).
2) If the first p-value was greater than or equal to alpha/n (where alpha is 0.05 and n = the number of p-values/comparisons), no p-values were deemed

significant and the procedure ended. If, however, the p-value was smaller, the procedure carried on to step three.

3) The first p-value was declared significant and the second p-value was compared to alpha/(*n*-1). If the second p-value was greater than or equal to alpha/(n-1), the procedure was stopped and no further p-values were significant. Otherwise, the procedure continued.
4) The second p-value was declared significant and the third p-value was compared to alpha/(*n*-2), and so on.

Part Two: Empirical Study

Detailed research aims

1) To explore whether a brief psychological mindset intervention that incorporated transient and trait-like factors, alongside self-compassion and other third wave constructs, was a feasible and acceptable tool to promote mental health within UK educational settings.
    - To identify whether UK sixth forms and colleges would engage with the intervention.
    - To ascertain views and experiences of the intervention (e.g. perceived helpfulness, ease of use) from students and educational staff.
    - To identify the barriers and facilitators to engagement and implementation.
    - To identify potential improvements to the intervention.
2) To investigate whether outcomes were indicative of promising intervention effects.
    - To explore mechanisms of change immediately post-intervention, and outcomes at follow-up, by identifying means and standard deviations.
    - To estimate between-group differences on measures at baseline, post-intervention, and follow-up, as well as within-group changes across time points.
    - To determine whether any differences over time were reliable and/or clinically meaningful for each treatment arm.

- To estimate effect sizes of differences between groups over time, and thus the potential sample size required for a full-scale evaluation.
- To explore students' responses to the multiple-choice questions and written task within the intervention, to further assess its potential effectiveness.

3) To determine whether the proposed evaluation design for this intervention was feasible and acceptable.
    - To explore whether the evaluation plan could be implemented as intended, as well as the barriers and facilitators to implementation.
    - To identify views and experiences of the research process (e.g. randomisation) from students and educational staff.
    - To determine recruitment and retention rates, reasons for dropout, and obstacles to recruitment or retention.
    - To explore any missing data and its randomness.
    - To identify potential improvements to the study design.

Detailed procedure

When a sixth form or college agreed to host the study, they were asked to identify a teacher or other staff member who was willing to be the key contact person for the project. This individual liaised with the researcher to coordinate delivery of the study on the host establishment's premises during normal teaching hours, and undertook the initial contact with potential participants. They were asked to approach a range of Year 12 and 13 classes/courses, either directly or by delegation to other staff, and briefly explain the

study to students. Importantly, all students were asked to confirm whether they consented to be contacted by the researchers at this point. The key contact person was also asked to put the study on the sixth form or college's virtual learning environment/intranet page, giving students the option to contact the researchers directly if they wanted more information.

Once consent to contact was established, potential participants were given an information sheet (Appendix F). Participants were also asked if they wanted a letter that explained the study to their parents/guardians (Appendix G). Students were informed that it was their choice to give this letter to their parents or guardian if they felt they wanted or needed to do so. The letter outlined that children over the age of 16 years old are presumed to have capacity to consent to research without parental consent, however, it also gave the contact details for the research team to enable enquiries from parents/guardians.

After no less than 24 hours, the key contact person asked the potential participants if they would like to be involved in the study. Students who said that they wished to participate having read the information sheet were invited to privately meet with a researcher on the host site's premises to review the information sheet again, ask any questions, and complete a consent form (Appendix H). All participants for a single site were consented on the same day; two researchers (AP and another trainee named on the empirical paper, JC, who was leading on the "sister-study" outlined below) were present to ensure this was viable. Those who did not consent were excluded from the study. Those who consented to take part were assigned a participant number and were randomised to the control or treatment arm using a block technique (Suresh, 2011).

A block randomisation approach ensures that groups have nearly equal sample sizes, without requiring the researcher to know in advance how many participants would consent. A person external to the research team generated an allocation sequence list from 1-80 using an online randomiser with block sizes of four and six (www.sealedenvelope.com). The sequence was concealed to the researchers in a password protected document. After the researchers consented a participant, they were able to access the next allocation in the sequence and inform the participant of their treatment arm.

Immediately after being told their allocation, participants were asked to complete baseline measures. After all participants at a site had done this, the intervention group completed the 30-minute, self-administered, psychological mindset intervention the same day, delivered using computer facilities provided by the host establishment. The researchers aimed to have all participants complete the intervention in a classroom at the same time. Where this was not possible (due to limited computer facilities or overlapping student commitments), multiple sessions were run within a single day. Each participant had their own computer station and they were encouraged to wear headphones, refrain from talking to peers, and engage independently. The control group participated in their usual timetabled activities, alongside those who did not wish to be involved in the study.

Once the intervention was completed, both the control and treatment arms were asked to redo measures of mindset and psychological flexibility, as these were thought to be key mechanisms targeted in the intervention. The other measures were not administered at post-treatment given that change would not be expected to occur immediately.

All participants were in a classroom together when the outcome measures were administered. The researchers emphasised that it was important for participants to complete the measures independently, without discussion amongst one another, to maintain validity of the research and confidentiality; those who did not adhere were reminded to be quiet and/or were asked to leave the classroom. To further maintain privacy the researcher liaised with the site to set up the room similar to a test environment, whereby individuals had their own private space. Moreover, individuals were asked to return completed measures to the researchers in a blank envelope provided, concealing their responses from others.

The researchers returned to each site at 4-week and 8-week follow-ups, to re-administer all outcome measures to both groups. After completing the outcome measures at the 8-week follow-up, a self-report measure of change was also administered (Appendix I). Following this, students in the control group were given the opportunity to complete the intervention. Also at the 8-week follow-up, participants and educational staff were given feedback questionnaires (Appendices J & K), seeking their views and experiences of the intervention and research process itself. Educational staff involved or impacted by the study were offered copies by the key contact person; staff also returned forms via this person to maintain anonymity. Finally, the researchers hosted a debriefing session to student participants at the 8-week time point.

Throughout the running of the study, host sites gave their students permission to take time out of their usual curriculum to partake in the research, with the intention to reduce barriers to participation and therefore increase generalisability of the sample. The

researcher worked closely with the host establishments to ensure that students did not miss essential teaching and that a minimal amount of time was taken from usual activities.

Amendment

Originally, it was planned that the study would be conducted at a single site: a local, government-funded college with over two-thousand 16-18 year olds from diverse backgrounds. However, this college struggled to recruit participants. Given this, and that it was felt important to approach a range of institutions so that the sample was representative, an amendment was made to make this a multi-site study. Ethical approval was obtained for the amendment (Appendix L) before seeking gatekeeper consent from additional host establishments.

Intervention development

The first stage of developing the intervention consisted of discussions between AP and JC, under guidance from GB and RMS (secondary supervisors), to decide content. Psychoeducation regarding neuroplasticity and the malleability of personal traits was deemed important for inclusion, given this has proved effective in previous mindset interventions for youth (Schleider & Weisz, 2016; 2018). The authors decided to also address mindsets related to transient psychological factors. This was because beliefs about thoughts, emotions, and urges were found to be highly correlated with mental health and wellbeing (e.g. Harris, 2006; Schroder, Dawood, Yalch, Donnellan & Moser, 2015; 2016).

Given that malleability mindsets for both trait-like and transient factors have also been associated with striving, self-blame, and reduced acceptance (Dweck, Chiu & Hong,

1995; Kneeland, Nolen-Hoeksema, Dovidio & Gruber, 2016), the authors incorporated constructs from third wave cognitive behavioural therapy (CBT) approaches (e.g. Harris, 2006; Neff, 2003), with the aim to promote compassionate mindsets that balanced ideas about change and acceptance.

The key messages delivered in the intervention were:

1) Thoughts, feelings, and behavioural urges have a biological basis in the brain (they result from activity between neurons).
2) There are links between thoughts, feelings, and behaviours (as well as bodily responses) that are neurologically-based.
3) Neuronal activity is transient rather than fixed, and thus so are psychological experiences.
4) Neuronal activity is also rapid, extensive, and influenced by our past experiences (personal and evolutionary), meaning it is not entirely controllable.
5) Whilst we cannot entirely control psychological experiences, we can choose how to *respond* to them.
6) We can simply watch them pass, given they are temporary surges of neuronal activity and are inherently harmless.
7) We can also be compassionate towards our psychological experiences.
8) Difficult thoughts, feelings, and urges are often our brain's way of trying to protect or do what is best for us, rooted in past experiences and learning.
9) Our brains can be overprotective, however. Sometimes, our thoughts and feelings are biased, and can urge us to behave in ways that are unhelpful.

10) Whilst remaining compassionate, we can choose not to listen to our brains or bodies, if doing so would take us further away from doing what we value in life.
11) We may notice patterns of the same thoughts, feelings, and urges arising again then again.
12) Long-standing patterns are sometimes construed as "personality".
13) Familiar psychological experiences and personality have the potential to change over time, depending on our responses to them.
14) If we change our behaviour or environment, our psychological experiences can change.
15) Our personalities and neurobiology can also alter and grow. This is because the human brain has an ability to be shaped or moulded, called neuroplasticity.
16) It means that neurons can be likened to muscles; existing connections can be made stronger or weaker, and new connections can be formed altogether.
17) Nevertheless, humans have limitations, are imperfect, and some psychological experiences or patterns are essential responses to our environment and/or evolution, so cannot be changed.
18) When this is the case, however, we can still choose to live by our values.

Several approaches were drawn on to create this content, including: 1) neurological and evolutionary science, regarding ideas about neuroplasticity, brain activity, etc. (e.g. Kays et al., 2011); 2) "first wave" and "second wave" CBT, regarding ideas about learning, links between psychological experiences/body/behaviour, the role of changing behaviour to change our psychological experiences, etc. (e.g. Beck, 2011;

Eysenck, 2013); and 3) third wave CBT, regarding ideas about self-compassion, mindfulness, acceptance, etc. (e.g. Neff, 2003; Hayes, Boyd & Sewell, 2011).

Once the content was decided, AP scripted a 10-minute psychoeducational animation and accompanying voice-over, with input from JC, GB, RMS, and a college workgroup of educational staff and students (see Patient and Public Involvement below). AP and JC then sought support from independent animators, using social media and by approaching local art/computer schools. The voice-over was recorded by AP and other volunteers. Schleider and Weisz's intervention (2016; 2018) similarly began with a psychoeducational component; whilst theirs was written text rather than an animated video, it was thought that the latter could be appealing to young people.

In line with previous mindset interventions (e.g. Schleider & Weisz, 2016; 2018), stories from fictional young people followed the animation, describing how they used its content in their everyday lives and to manage difficulties. AP wrote scripts for three actors (with input from the research team), who were sought through advertisement to the university's undergraduate courses. Videos of the actors were recorded at the TV studios on campus where possible. The three videos lasted a total of five minutes.

The next part of the intervention took the form of an interactive multiple-choice question and answer section to assess the viewer's understanding of the content. Akin to the intervention by Schleider and Weisz (2016; 2018), participants were then asked to complete a letter-writing task, giving a fictional younger student advice based on the information they received from the animation and videos.

The whole intervention (including the animation, videos, question and answer section, and letter task) was uploaded to an online platform with support from the

university's learning and technology department, so that participants could access and complete it from within the host establishment, via a weblink they were given by the researchers. It was ensured that participants' responses to the multiple-choice questions and written task were electronically saved and securely sent to the researcher for evaluation.

All those involved in creating the intervention (including the animators, and actors playing the fictional young people) were recognised in the credits section that appeared once a response to the letter-writing task had been submitted. Moreover, the credits acknowledged Schleider and Weisz, who gave permission for the intervention to follow a similar format to theirs (2016; 2018), but with adapted and additional content.

The intervention can be viewed at https://ueadldteam.typeform.com/to/T84uxV by clicking "Begin Submission" and typing in "Test" as a participant number. Alternatively, several illustrative screenshots are available in Appendix M.

Sample size

Target sample sizes for feasibility studies are typically based on recommendations for pilot studies. Whilst arguably distinct methodologies, this can be appropriate if the proposed size matches the study's objectives (National Institute for Health Research, n.d). Recommendations generally range from 20-80 participants (Cocks & Torgerson, 2013). When parameters and expected effect sizes are unknown, and where researchers want to estimate the sample size required for a full-scale trial, ≥50 participants are suggested (Cocks & Torgerson, 2013). Thus, the study aimed to recruit a minimum of 50 students in total.

Rationale for the included outcome measures

Given that the intervention integrated multiple approaches, and no single measure existed to capture all aspects, various outcome measures were used.

The Implicit Personality Theory Questionnaire (Yeager, Miu, Powers & Dweck, 2013; Schleider & Weisz, 2016; Appendix N)

This measure assessed whether individuals believed personality was malleable or fixed. It was included as a primary outcome because personality mindsets were a key mechanism targeted by the intervention.

Acceptance and Fusion Questionnaire for Youth–Short Form (AFQ-Y8; Greco, Lambert & Baer, 2008; Appendix O)

There was no existing measure that explicitly assessed the transient psychological mindsets promoted in the intervention. Whilst there was the Implicit Theories of Emotion Scale (Tamir, John, Srivastava & Gross, 2007), it was not used because it valued control of psychological experiences (e.g. “everyone can learn to control the emotions that they have”), which was discordant with the intervention. Moreover, it did not cover thoughts and urges. The mindsets promoted in this intervention were more akin to third wave constructs (e.g. encouraging acceptance of difficult psychological experiences), which this measure was designed to assess; hence its inclusion as a primary outcome. Many items (e.g. “the bad things I think about myself must be true”) are phrased like attitudes or beliefs (i.e. mindsets), and so this measure was used at all time points, including immediately post-treatment as change might be expected. Evidence suggests that the AFQ-Y8 is predictive of emotional instability, externalising, and internalising problems among youth, which further warranted its inclusion (Szemenyei et al., 2018).

Self-Compassion Scale–Short Form (SCS-SF; Raes, Pommier, Neff & Van Gucht, 2011; Appendix P)

Self-compassion involves self-kindness versus self-judgement when we suffer, fail, or think we are inadequate; 2) common humanity versus isolation, whereby we perceive difficult psychological experiences and imperfection as a shared human condition; and 3) mindfulness versus over-identification, where we are accepting and non-judgmental towards difficult thoughts and feelings, without getting caught up in them (Neff, 2003). All of these concepts were incorporated within the mindsets promoted in the intervention and thus inclusion of this measure was warranted. Moreover, the SCS-SF predicts mental health over time among adolescents (Marshall et al., 2015). The measure was not included immediately post-treatment, despite its potential importance of capturing a shift towards a compassionate mindset, because it assesses behaviour, and thus change would not be expected.

Rosenberg Self-Esteem Scale **(RSES; Rosenberg, 1965; Appendix Q)**

This measure was included because self-confidence is emphasised as a target for school-based mental health interventions (Department of Health [DoH] & NHS England, 2015a, 2015b). Moreover, scores during adolescence predict mental health in adulthood; thus, it is a useful outcome for promotive tools (Trzesniewski et al., 2006).

Revised Children's Anxiety and Depression Scale-Short Version **(RCADS-25; Ebesutani et al., 2012; Appendix R)**

This measure of low mood and anxiety was included to assess the intervention as a promotive mental health tool. Cut-off scores for clinical symptomatology were only used to understand the characteristics of participants recruited, not to evaluate the

intervention; whilst symptoms of low mood and anxiety among a general sample might be expected to reduce in response to a promotive mental health tool, the intervention was not intended to be a form of treatment for clinical populations.

Additional outcome measures

Transient psychological mindsets (Appendix S)

The researchers developed a three-item measure in attempt to assess mindsets relating to thoughts, feelings, and urges. Whilst the AFQ-Y8 was included, it was not a direct measure of mindset, nor designed for this purpose. Rather, it was made to assess third wave constructs, which were promoted in the intervention, but were integrated with other approaches. The AFQ-Y8 would have not captured a mindset balancing ideas about change with acceptance and self-compassion, nor would any of the other included measures, at least in isolation. Moreover, a few items in the AFQ-Y8 assessed behaviour, rather than attitudes/beliefs. These would have been invalid immediately post-treatment. Further, it could not be assumed that change in mindset could be measured by change in behaviour over time.

It is not uncommon to create new mindset measures when there is a lack of alternatives (e.g. Tamir et al., 2007; Schroder et al., 2015). Nonetheless, when reliability of the measure created by the researchers was calculated as part of the feasibility assessment, alpha was negative (α = -0.20). If internal consistency is negative, it is indicative of a serious problem and the measure should be revised or discarded (Tavakol & Dennick, 2011). Thus, whilst this scale was administered at all four time points (it was not pre-piloted and reliability remained unknown until analysis was fully underway), it

was subsequently deemed invalid to use the data for evaluating the intervention. Further exploration to develop a suitable measure for any ensuing trials is needed, but was beyond the scope of this thesis project.

Attendance

The researchers sought input from teachers and other educational staff with regard to the study design, who suggested that attendance data be explored as an outcome (see Patient and Public Involvement). Indeed, mental health difficulties are significantly associated with school absenteeism and maladaptive self-beliefs are thought to contribute to non-attendance (Egger, Costello & Angold, 2003; Rivers, 2010). Accordingly, the total number of days for unauthorised absences, exclusions, or sickness, 8-weeks before and after delivery of the intervention to the treatment arm were recorded. Percentage change in attendance pre-to-post intervention was calculated per group.

Attendance data were provided by the host sites at the end of the study, and thus, was only available for those who remained at 8-week follow-up (n = 42). Further, for one site, attendance data were only obtainable for 6-weeks prior to the intervention due to the timing of the study relative to the school summer holiday. Whilst the use of percentages mitigated this problem, the data were incomplete and thus was deemed non-essential for publication where word counts were restricted.

Further assessment of feasibility

Feedback from educational staff

Feasibility questionnaires were administered to educational staff to obtain their views of the intervention and trial design. Findings were not reported in the main paper

as only two responses were received. Data analysis would have not been meaningful, but responses are summarised in additional results (Ch. 6).

Researcher diaries

The researchers recorded logistical experiences in diaries, focusing on the implementation process, barriers, and facilitators. They were also present when participants completed the intervention to record observations. Key events and themes surrounding implementation and engagement were identified from the researchers' diaries and are presented in additional results (Ch, 6), with the intention to inform any future trials.

Ethical considerations

Approval

Ethical approval for the study was granted by the University of East Anglia (UEA) Faculty of Medicine and Health Sciences Research Ethics Committee (Appendix T).

Gatekeeper consent

Written gatekeeper consent was sought (e.g. from principals/head teachers) for all host sites before the researchers delivered the study at that institution.

Educational staff consent

Teachers and other educational staff were asked to participate by completing a feedback questionnaire. For self-administered questionnaires, it is not necessary to produce a separate participant information sheet or consent form (Health Research Authority [HRA], 2017a). Rather, the questionnaire itself contained a short introduction with sufficient information to enable staff to make an informed decision about completion

(Appendix K). According to the HRA, provided that this information broadly describes the nature and purpose of the research, why someone is being invited to take part, how their information will be used and stored, and how they might access study findings, then completion and return of the questionnaire was sufficient to indicate consent (HRA, 2017a).

Participant consent

Participant information sheets, created using HRA templates (2017b), detailed the purpose and nature of the study, including ethical considerations (Appendix F). In line with the General Data Protection Regulation (GDPR), they clearly stated the data to be collected and how it would be used (European Union [EU], 2017). To make a fully informed decision, participants had at least 24 hours to consider the information sheet. They were then offered an opportunity to meet with the researchers individually to ask any questions and complete consent procedures. Potential participants received open answers to any questions, as there was no reason for deception. Written consent was sought; two copies of the consent form (Appendix H; developed from an HRA template) were completed, with one retained by the participant and another by the researcher (British Psychological Society [BPS], 2014). The researchers sought consent at each stage of the intervention (e.g. by asking whether participants were happy to continue). Individuals who did not wish to continue were removed from the study.

Participant capacity

As participants were over 16 years old, capacity to consent was assumed (BPS, 2014). Prior to meeting potential participants individually to complete the consent process, the researcher asked the key contact person whether they had any reason to doubt

the capacity of those who expressed interest in taking part. The researchers were fully trained to assess individuals whose capacity to consent was queried, to see whether they could understand, retain, and weigh up the relevant information, then communicate their decision to take part; nonetheless, the key contact individuals reported no concerns. Anyone found to lack capacity to consent according to the Mental Capacity Act (2005) would have not been recruited. Ongoing capacity of those who consented was assumed throughout the study. Nonetheless, the researcher asked the key contact person to inform them if there was any new reason to doubt the capacity of those taking part; while no concerns arose, capacity would have been (re)assessed if needed.

Parental involvement

The researchers were not obliged to actively inform parents about the study or their child's participation, given that they have no role in the consent process for students over the age of 16 years old with capacity, and the duty to protect participant confidentiality. Nonetheless, participants were given the choice to take a letter informing their parents/guardians about the study (Appendix G). The letter proposed that parents/guardians approach the researchers if they had concerns or wanted more information, though none made contact throughout the running of the study.

Coercion and withdrawal

Risk of coercion into participation was reduced by seeking consent to contact and clearly stating on the information sheet that there were no consequences for an individual's rights or education should they not wish to participate or want to withdraw (BPS, 2014). The information sheet also encouraged participants not to feel pressured into being involved by their peers. Moreover, the key contact person, rather than the

researchers, approached potential participants after they read the information sheet to ascertain whether they wished to continue to consent. There were no gift/monetary incentives for participation. Participants were explicitly offered opportunities to withdraw at every time point. Data were anonymised and some analyses began from baseline, thus withdrawal of information already provided was not possible beyond this point. Nonetheless, participants were not asked to give further information. This was made clear on the participant information sheet.

Data management and confidentiality

The minimum amount of personally identifiable data needed was collected and nobody outside the research team had access to it (EU, 2017). All identifiable data were kept securely at the university, in a locked filing cabinet in a locked office. Non-identifiable data was accessed off the university premises by the researcher for the purposes of analysis, via OneDrive for Business (a secure, cloud-based file storage system, approved within the university's data management policy [UEA, 2017]). All files saved to OneDrive were password protected. Data were analysed in an anonymous format (EU, 2017). Only the consent form stated the participant's name; participant numbers were used on all other documents, to trace individuals throughout the study. All data reported for publication were not personally identifiable. In accordance with the UEA Research Data Management Policy, data will be kept securely for at least 10 years following publication, before being destroyed (UEA, 2017).

Confidentiality would have only been overridden if the researchers were significantly concerned about risk to someone (BPS, 2014). In this case, a senior member

of staff at the host site would have been informed so that they could implement their usual safeguarding procedures. Nonetheless, there were no risk issues throughout the study.

Debrief

In line with BPS guidance (2014), participants were offered a debrief. They were given clear information about the study and an opportunity to have any questions answered. Although the debrief was offered as a group session, participants were invited to speak with the researchers privately at the end if they wished. A lay-summary report of the research findings will be produced for the host sites. The key contact person will be asked to deliver this report to any participants who marked that they wanted a copy of the study findings on the consent form.

Distress

It was possible that participants would reflect on personal issues relating to the research, such as their emotional experiences, which could have been distressing. For all group components (e.g. delivery of the intervention and debriefing session), at least two members of the research team (AP and JC) and one educational staff member were available to manage any issues if they arose. The researcher asked all host sites to identify a member of staff, with existing procedures in place to manage student distress and sign-post accordingly (e.g. a pastoral support worker/safeguarding lead), who students could have been directed towards should they have presented that way to the researchers. If participants became distressed off-site, the information sheet (Appendix F) included the researcher's contact details to discuss study matters and information about making a complaint. It signposted individuals to speak to staff at their sixth form/college, their GP, the Samaritans, MAP, and Young Minds regarding personal issues (BPS, 2014). It is

important to note, however, that no participants expressed distress to the researchers throughout the running of the study.

Duty to inform

If individuals scored above clinical thresholds for total or subscale scores on the RCADS-25 (T score $\geq$ 70), they were informed of this and signposted to the key contact person and/or other appropriate staff member(s) identified by host sites (BPS, 2014). They were also encouraged by the researchers to talk to a trusted other (e.g. a parent). The participant information sheet contained contact details of local and online mental health resources, which participants may have chosen to use. One educational staff member reported that participants scoring above clinical threshold led them to have helpful conversations with these students and sign-post accordingly. The researchers were not obliged to inform participants about their scores for any of the other outcome measures, given they were not related to clinical symptomatology.

Benefits and burdens

The control group were offered the intervention to mitigate any distress from randomisation. Potential benefits were therefore not withheld from any participant. Burdens comprised the time taken from educational activities to participate. The researchers attempted to minimise this burden (e.g. by selecting short measures) and benefits were predicted to outweigh costs.

There were also burdens for host sites (e.g. time taken for educational staff to coordinate delivery of the study), though the researchers attempted to be as supportive and flexible as possible.

Analysis

Analysis of feedback questionnaires

The student and teacher feedback questionnaires included a space to provide open-ended comments. Nonetheless, responses were very limited, so they were subject to a simple, quantitative content analysis (Morgan, 1993). Findings did not add to the conclusions drawn in the main paper, so were reported in additional results only (Ch. 6).

Content analysis of participant's letters

Participants' responses to the written task in the intervention were more complex and lengthy, and thus were analysed with a more in-depth form of content analysis, as described by Erlingsson and Brysiewicz (2017). The researcher first familiarised themselves with the data, by reading through responses several times and answering four reflective questions:

1) What is the text talking about?
2) What stands out?
3) How did I react reading the text?
4) What message was I left with?

Following this, they condensed responses into meaning units, which were close to the text and reflected manifest content. Meaning units were then assigned codes (one or two word descriptive labels of the units), which were subsequently organised into categories (groups of related codes). Where codes seemed to fit into two categories, the researcher returned to the data to see if it could be more accurately reflected by a different meaning unit or code, and/or tried to narrow the categories. Finally, themes were identified that connected categories together; the researcher returned to the data when

creating the themes to ensure that they captured the latent or more abstract meanings apparent in the content.

The whole process was circular and repetitive, whereby the researcher repeatedly returned to the data, their initial impressions, and analysis over time (Erlingsson & Brysiewicz, 2017).

Reliable difference

The following formula was used to calculate reliable change indexes (Jacobson & Truax, 1991):

$$RC = \frac{x_2 - x_1}{S_{diff}}$$

Where the calculated score was greater than 1.96 ($p < .05$), change was deemed reliable (Hawley, 1995).

When computing reliable change indexes, the data required to calculate *Sdiff* (i.e. standard deviation [SD] and reliability of the measure) was initially sought from a normative sample of UK adolescents, given this would be representative. Alternatively, reliability was obtained from validation studies with young people, and SD was calculated for the current sample at baseline. It is common practice to obtain reliability scores from normative samples or validation studies (Ferguson, Robinson & Splaine, 2002). It is acceptable to estimate SD from baseline scores of the study sample (Evans, Margison & Barkham, 1998).

Anchor-based assessment of change

Anchor-based methods involve using an external indicator to the study's outcome measures, referred to as an anchor, to calculate clinically minimal important difference (MID) (Revicki, Hays, Cella & Sloan, 2008). It is common to use a self-report scale as an anchor, asking participants to classify themselves as unchanged since entering the study, or experiencing small, moderate, or large improvement or deterioration (Johnstone et al., 2015). As such, a self-report measure of change (Appendix I) was administered at the 8-week follow-up, not as an outcome measure itself, but to facilitate analysis; it contained nine items, or anchors, assessed on a 7-point scale to capture the change categories listed above (Johnstone et al., 2015). Each item corresponded to one of the primary or secondary outcome measures (Table 5.1).

Table 5.1

Items on the self-report measure of change and the primary/secondary outcome for which they were an anchor

Item(s)	Outcome measure
Item 1 + 8*	Transient Psychological Mindset
Item 2	Implicit Personality Theory Questionnaire
Item 3	Self-Compassion Scale–Short Form
Item 4	Rosenberg Self-Esteem Scale
Item 5 + 6*	Revised Children's Anxiety and Depression Scale–Short Version (item 5 was the anchor for the anxiety subscale and item 6 was the anchor for the depression subscale specifically)
Item 7 + 9*	Acceptance and Fusion Questionnaire for Youth–Short Form

*Where there were multiple anchors per outcome measure, individuals were categorised as experiencing no/small/moderate/large change by taking an average from the anchors. Note: Item one was not utilised as it was related to the transient psychological mindset measure, which was deemed too unreliable to explore possible intervention effects.

For each of the primary and secondary variables, MID was calculated as the mean difference in scores for these outcomes between baseline and the 8-week follow-up (as

this is when the anchor scale was administered), derived from the group of participants categorised as experiencing "small change" on the related anchor(s) (Revicki et al., 2008). Participants experiencing small positive or small negative change were pooled together, accounting for differences in the direction of scores (Revicki et al., 2008). The percentage of participants scoring above or equal to the estimations of MID for each outcome measure were then identified per treatment arm.

Patient and public involvement

During the research planning stage, a college that later agreed to host the study organised a workgroup of staff and students, to input into the intervention and study design, at request from the researcher. The workgroup were shown a draft of 1) the voice-over script for the animation, and 2) the stories of fictional young people. They were asked for particular feedback regarding age-appropriateness and comprehensibility of the content. Appendix U details brief meeting minutes taken by the researcher during this workgroup.

The research design and intervention were adjusted accordingly. For example, attendance was included as an outcome measure because educational staff emphasised its importance in evaluating school-based mental health interventions. Moreover, students thought the language was too complicated in the animation and so it was simplified. Students also emphasised the potential benefits of including coping strategies for difficult psychological experiences within the stories from fictional young people, which the researchers acknowledged (e.g. a defusion technique from third wave approaches was subsequently incorporated into one of the actor's scripts).

Sister-study

A sister-study by another UEA trainee (JC) was run concurrently, recruiting a separate pool of participants to assess the feasibility of the intervention with 9-11 year olds. Each trainee led on adapting the intervention content and language to suit their participant age group, with support from one another and the project supervisors, though differences were kept minimal.

The two age groups encompass unique developmental periods, characterised by extensive change (e.g. entering puberty/adulthood, changing roles/relationships), which increase stress and can impact mental health in the long-term (Christie & Viner, 2005; Schulenberg, Sameroff & Cicchetti, 2004). There is evidence suggesting that fixed mindsets may be particularly detrimental when children face challenges (Romero et al., 2014) and that it benefits to offer interventions during transitions (Durlak & Wells, 1997).

It is unclear at which age it is most beneficial to offer mindset interventions promoting mental health. A clearer sense of personal identity develops in late adolescence alongside complex affective and cognitive skills, including an increased meta-cognitive capacity, which may be relevant for mindset work (Christie & Viner, 2005; Steinberg, 2005). Therefore, the intervention may be most accessible and appropriate for this age group. Nonetheless, if 50% of all mental health conditions are established by age 14 years (Kessler et al., 2005), earlier delivery may be more beneficial.

The trainees created the intervention and research design (e.g. selection of outcome measures/analysis plan) in collaboration. They were separately responsible for recruitment of (and liaison with) host sites, creating research documentation (e.g. consent forms), managing data, conducting analyses, and reporting of their own studies. The

trainees supported one another to deliver the intervention within educational settings, administer outcome measures, and host debriefing sessions.

While a comparison of the results from the two studies is beyond the scope of the individual theses, the concurrent nature of these sister studies has highlighted an interesting future research topic investigating the impact of developmental stage. Such research will help assess mindset tools as universal interventions.

CHAPTER SIX

Additional Results

This chapter contains results that could not be included within publications due to restrictions on word counts/tables for the selected journals.

Additional Results

Part One: Systematic Review and Meta-Analysis

Risk of bias and quality ratings

Figure 6.1 presents a breakdown of the risk of bias and quality ratings agreed in discussion between the primary author and a secondary assessor named on the review paper (AGP) for controlled trials randomising individuals. Ratings for cluster-randomised controlled trials are presented in Figure 6.2.

Figure 6.1: Ratings for risk of bias/quality for studies using individual randomisation

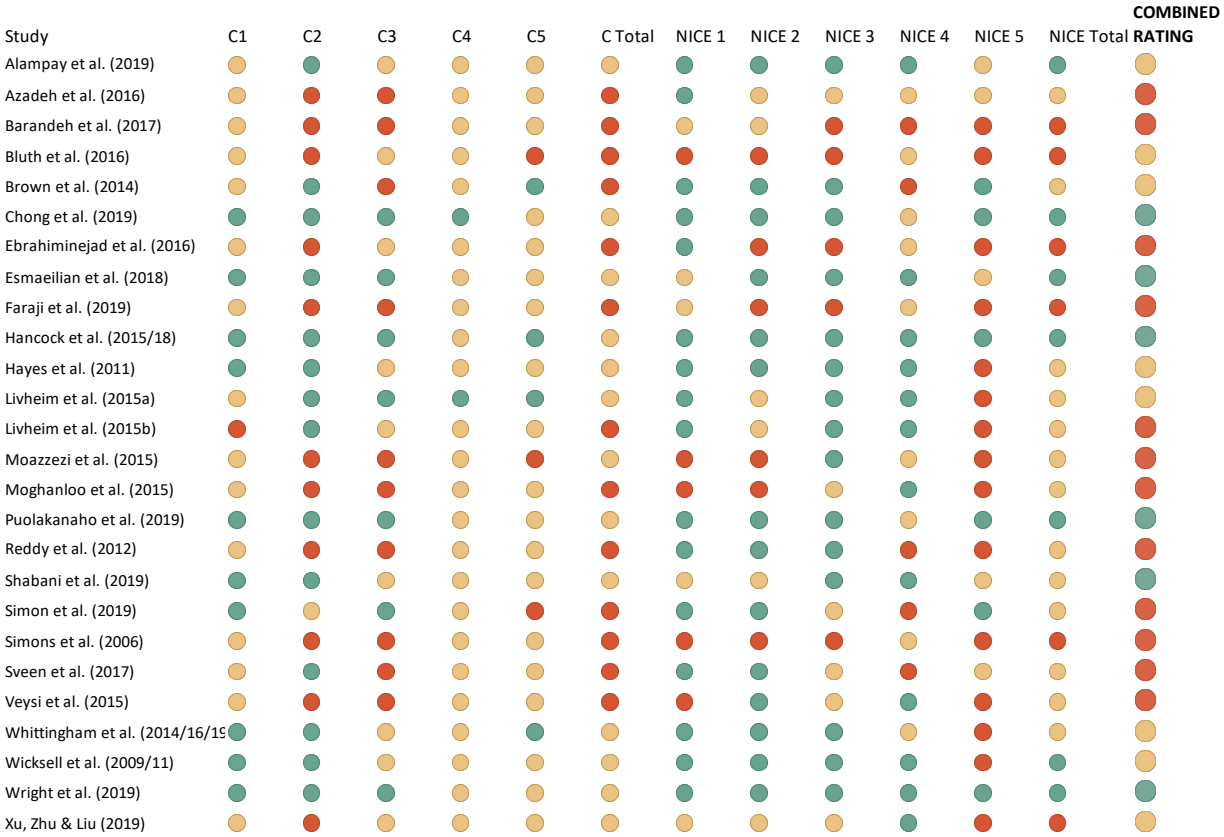


Figure 6.1. Ratings assigned using the Cochrane-risk-of-bias tool for randomised trials (Sterne et al., 2019, Ver. 2) and the NICE quality appraisal checklist for quantitative interventions studies (NICE, 2012)
Note: "C1-5" = domains 1-5 on the Cochrane tool; "C Total" = Cochrane overall risk of bias; "NICE 1-5" = selected items from the NICE checklist (see supplementary material in Ch. 2); "NICE total" = overall rating of quality based on these five NICE items; "combined rating" = categorisation of high (green), moderate (yellow), or low (red) quality, based on ratings from both tools
Code for individual items: red circles = high risk of bias on Cochrane/"-" and "Not Reported" on NICE; yellow circles = some concerns on Cochrane/"+" on NICE; green circles = low risk of bias on Cochrane/"++" on NICE

Figure 6.2: Rating for risk of bias/quality for studies using cluster randomisation

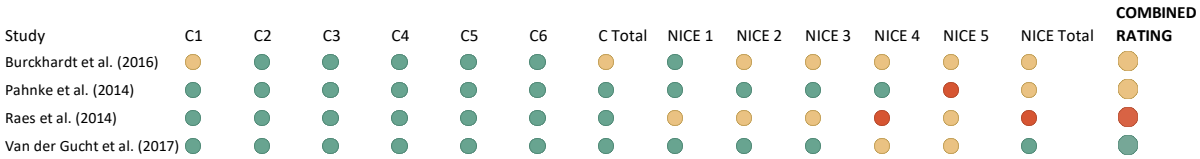


Figure 6.2. Ratings assigned using the Cochrane-risk-of-bias tool for cluster-randomised designs (Eldridge et al., 2016) and the NICE quality appraisal checklist for quantitative interventions studies (NICE, 2012).
Note: "C1-6" = domains 1-6 on the Cochrane tool; "C Total" = Cochrane overall risk of bias; "NICE 1-5" = selected items from the NICE checklist (see supplementary material in Ch. 2); "NICE total" = overall rating of quality based on these five NICE items; "combined rating" = categorisation of high (green), moderate (yellow), or low (red) quality, based on ratings from both tools
Code for individual items: red circles = high risk of bias on Cochrane/"-" and "Not Reported" on NICE; yellow circles = some concerns on Cochrane/"+" on NICE; green circles = low risk of bias on Cochrane/"++" on NICE

Additional assessment of publication bias

Table 6.1 summarises rank correlation tests of funnel plot asymmetry for the primary outcomes at follow-up. Funnel plots for the primary outcomes at follow-up are presented in Figure 6.3. Funnel plots for the secondary outcomes at post-treatment are shown in Figure 6.4.

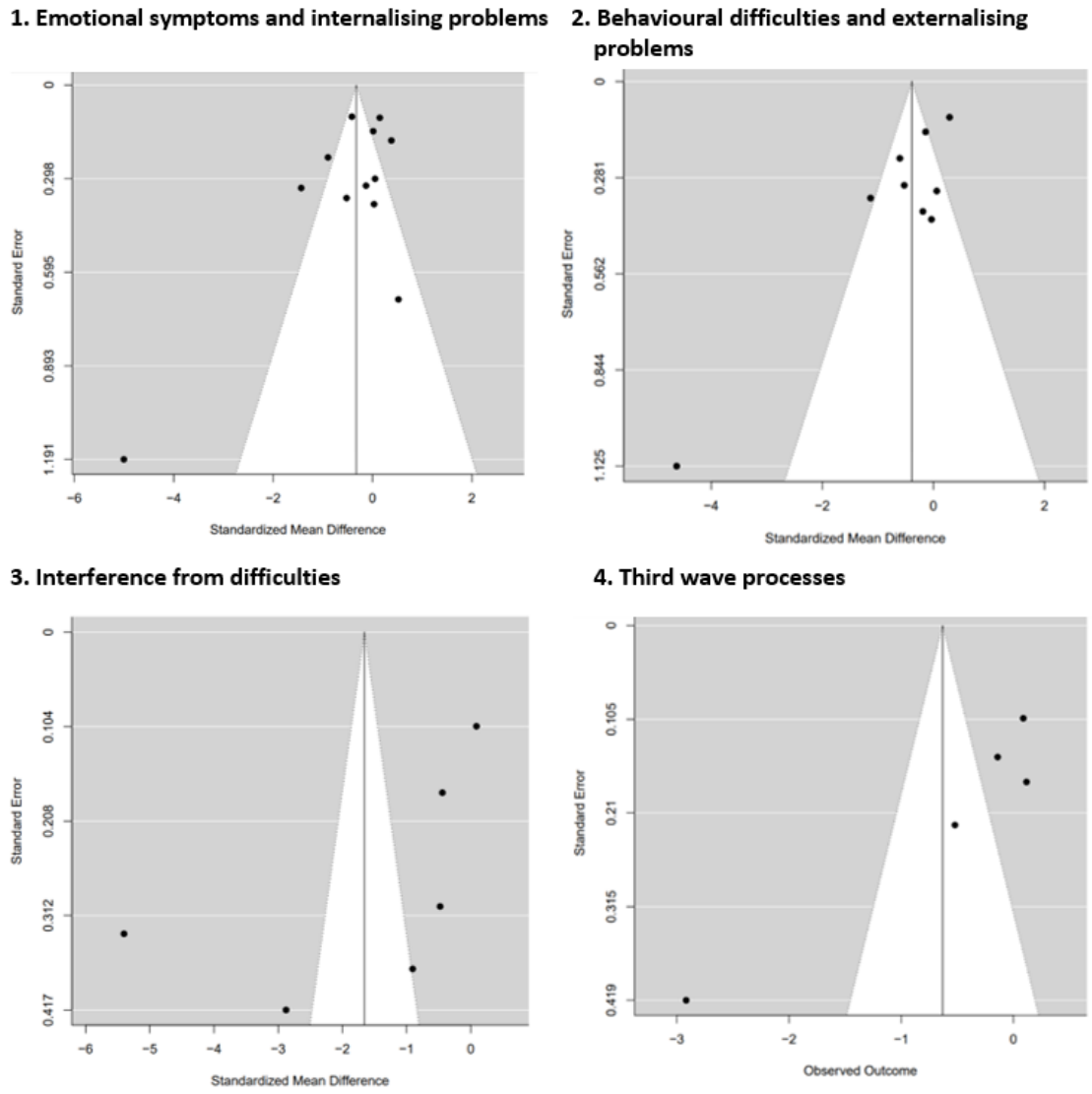
Table 6.1

Rank correlation tests of funnel plot asymmetry for primary outcomes at follow-up

Outcome	τ	Significance
Emotional symptoms and internalising problems	-.30	.197
Behavioural difficulties and externalising problems	-.28	.359
Interference from difficulties	-.73	.057
Third wave processes	-.80	.083
Wellbeing and flourishing	.40	.483
Quality of life	.00	1.00
Physical health and pain	.60	.233

Note: τ = Kendall's tau

Figure 6.3: Funnel plots for the primary outcomes at follow-up



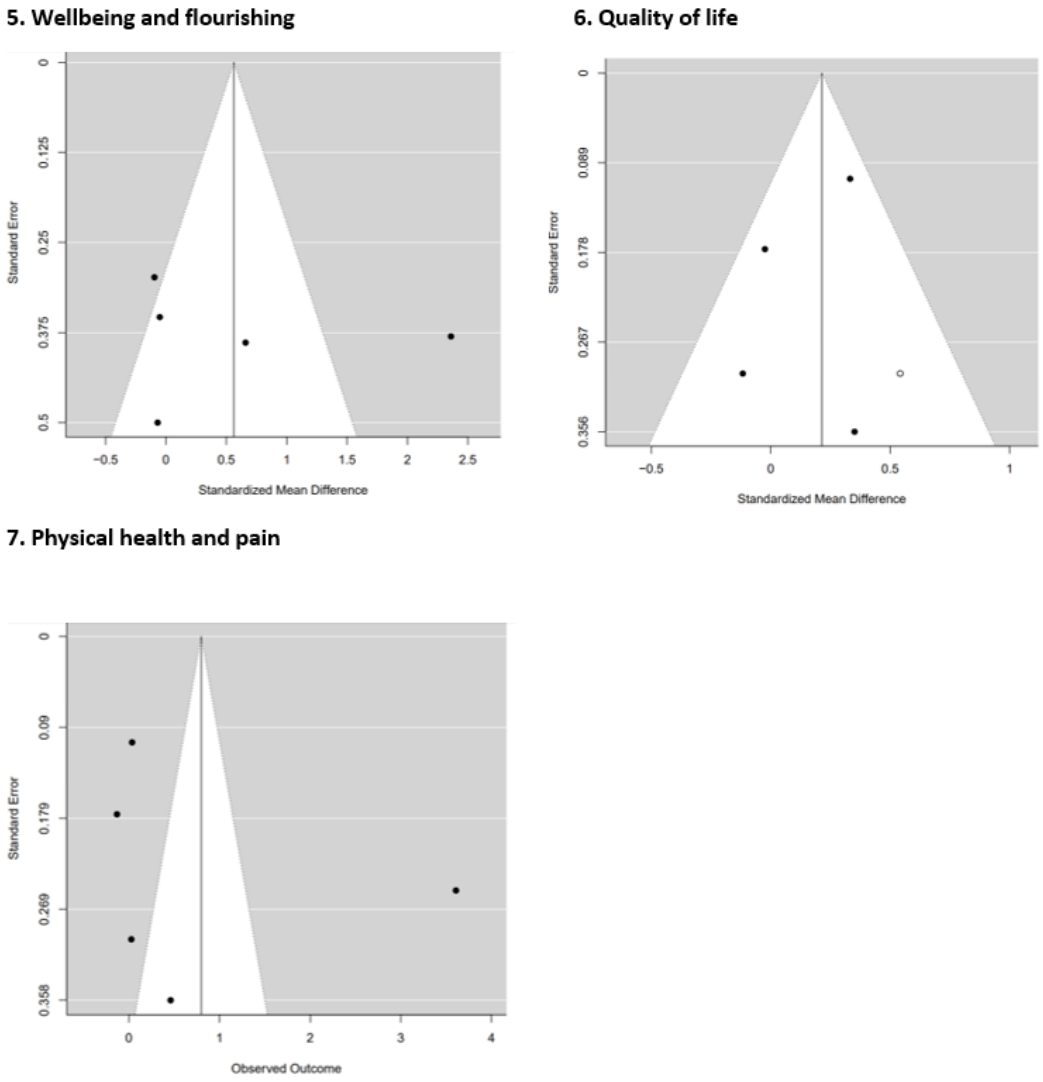


Figure 6.3. Funnel plots (random effects models) for the primary outcome variables at follow-up. Open circles (if any) show missing null studies estimated with the trim-and-fill method

Figure 6.4: Funnel plots for the secondary outcomes

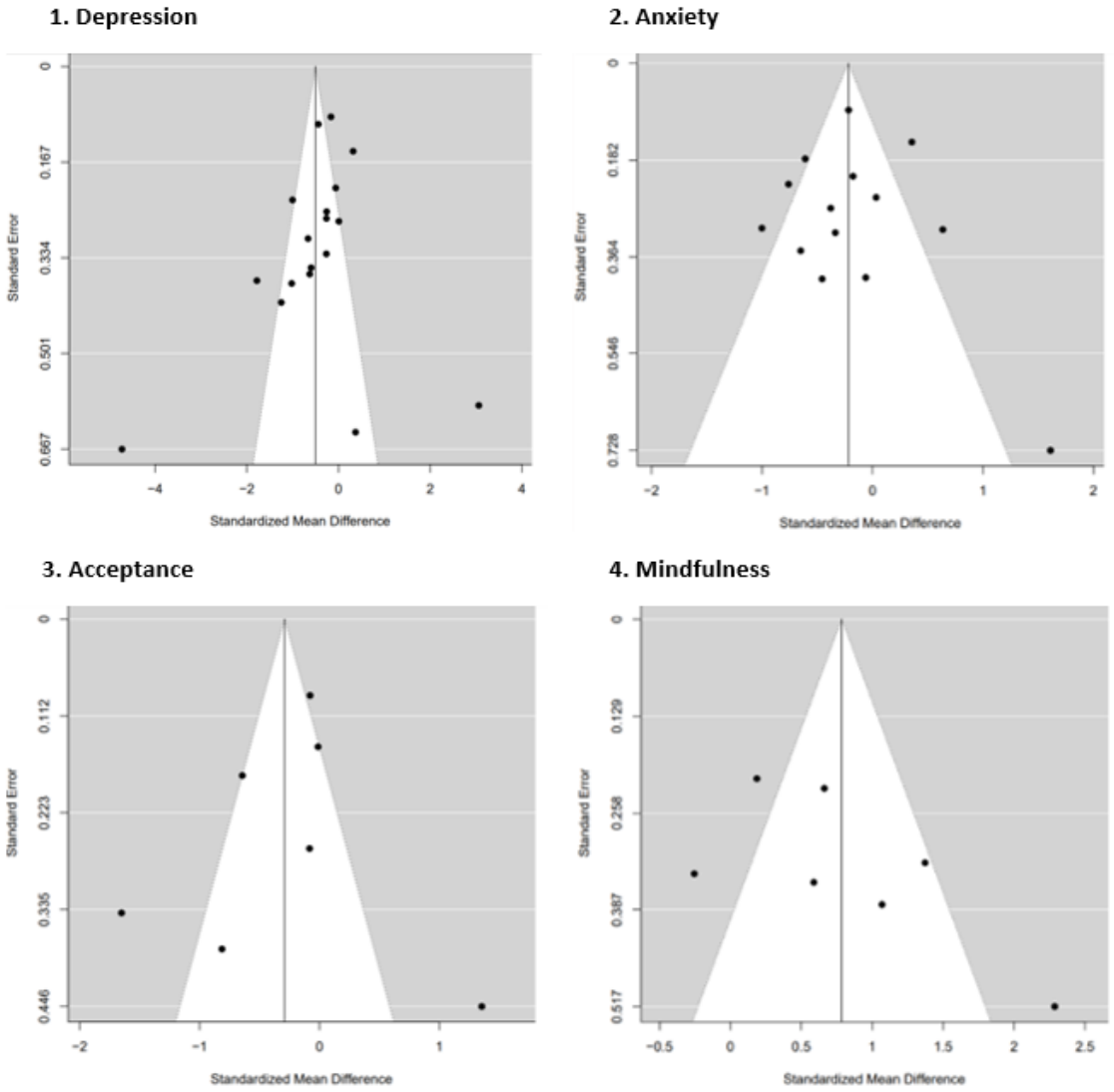


Figure 6.4. Funnel plots (random effects models) for the secondary outcome variables at post-treatment. Open circles (if any) show missing null studies estimated with the trim-and-fill method

Corrections for multiple comparisons

Tables 6.2 and 6.3 display adjusted alpha values for each outcome variable at post-treatment and follow-up. The correction was not applicable for physical health/pain, given no moderator or subgroup analyses were possible at either time point. Similarly, the correction was not applicable at follow-up, except for emotional symptoms/internalising problems and behavioural difficulties/externalising problems, given that moderators and subgroups were impossible to explore for the other variables.

Table 6.2

Holm-Bonferroni correction for moderation analyses at post-treatment and follow-up

Variable	Number of comparisons	First p-value non-significant	Final adjusted alpha-level[1]
Post-treatment			
Emotional symptoms and internalising problems	5	Second	.0125
Behavioural difficulties and externalising problems	4	First	.0125
Interference from difficulties	2	None	.0500
Third wave processes	2	First	.0250
Wellbeing and flourishing	3	First	.0167
Quality of life	3	Second	.0250
Follow-up			
Emotional symptoms and internalising problems	4	First	.0125
Behavioural difficulties and externalising problems	2	First	.0250

[1]Any p-values equal to or greater than this adjusted alpha level for an outcome variable were declared non-significant

Table 6.3

Holm-Bonferroni correction for subgroup analyses at post-treatment and follow-up

Variable	Number of comparisons	First p-value non-significant	Final adjusted alpha-level[1]
Post-treatment			
Emotional symptoms and internalising problems	12	Second	.0045
Behavioural difficulties and externalising problems	8	Third	.0083
Interference from difficulties	4	Third	.0250
Third wave processes	4	Fourth	.0500
Wellbeing and flourishing	6	Second	.0100
Quality of life	6	Fourth	.0167
Follow-up			
Emotional symptoms and internalising problems	8	First	.0063
Behavioural difficulties and externalising problems	4	First	.0125

[1]Any p-values equal to or greater than this adjusted alpha level for an outcome variable were declared non-significant

Part Two: Empirical Study

Open-ended responses from the participant feedback questionnaires

Responses to the open-ended questions were very limited. Five participants commented that the intervention was helpful or informative. One participant said it was not useful, another said they had forgotten what was included, and one said they were skeptical about the content. Three participants reported having technical problems on the computer. One participant thought the session was too lengthy and another said the session would be better if it were more interactive. With regard to the research process, three participants reported that the study was well explained and it was easy to take part, and three participants said it was time-consuming.

This feedback was generally consistent with responses on the questionnaire's Likert scale items. It provided information that could inform future trials (e.g. the potential benefit of including more interactive components), although it is important to note that comments reflect a small number of participants, and thus, may not be representative.

Educational staff feedback questionnaire responses

Only two educational staff completed the feedback questionnaires so means and standard deviations were not calculated. Responses are presented in Table 6.4.

Table 6.4

Feasibility questionnaire responses from educational staff

	R1	R2
Intervention-related items		
I feel like the students struggled to engage with the intervention	7	2
I think the intervention has benefited (or will benefit) the students	4	NR
It would be useful to have this intervention in schools	4	NR
It was difficult to get the resources to run the intervention	10	2
This intervention could fit within the school timetable	8	9
Research-related items		
It was easy to get students involved in the research	3	8
There was adequate support for staff and students	10	10
The measures used seemed appropriate	7	8
The research study consumed too much time	9	7
I did not like that students were randomly allocated to different groups	1	1

Note: R = respondent. NR = not reported
Responses were reported on a 10 point scale from 1 (definitely do not agree) to 10 (definitely agree). Scores of 5-6 indicated "maybe agree"

With regard to open-ended responses, one respondent commented that they did not see the intervention so could not make a judgment about its usefulness or potential benefit on the Likert scale. They commented that they would have like to have done the intervention themselves, in order to engage in further discussion with students about the content when the researchers left. Another said that a full course on this topic was needed in schools, not just a single session. One respondent said that the research consumed a considerable amount of time but was a smooth process, well supported, and well explained by the researchers. One respondent reported that the research was engaging and interesting to students.

Overall, it was difficult to draw conclusions with regard to feedback from educational staff, as the sample was very limited and some results were mixed. However, some findings were consistent with those reported by students (e.g. suggesting that

randomisation was acceptable/the study was time-consuming). It may be helpful to seek further feedback from educational staff in any ensuing trials, as it could inform future implementation.

School attendance

Attendance data was only available for those who remained in the study at 8-week follow-up (n = 42). Average attendance 6-8 weeks prior to the intervention was 97% (SD = 4.94) for the treatment arm and 95% (SD = 5.30) for the control arm. For the eight weeks following the intervention, average attendance was 94% (SD = 5.16) for the treatment arm and 94% (SD = 6.47) for the control arm. Mean percentage difference in attendance from before and after the intervention was -2.69 (SD = 7.02) for the treatment arm and -0.53 (SD = 4.31) for the control arm. Whilst results appear to slightly favour the control group, it is hard to draw conclusions due to potential bias from sample attrition.

Exploration of within-group mean differences

Change scores over time were calculated for each participant and an average is presented for each treatment arm per time point (Table 6.5). Estimates of effect size for the difference between mean score at baseline and post-treatment, 4-week follow-up, and 8-week follow-up were also calculated for each treatment arm (Table 6.5). These analyses were deemed as exploratory and not necessary for reporting in the main empirical paper, as estimates of between-group differences alongside assessments of clinically-important and reliable change had already been included.

Table 6.5

Average change scores and estimates of within-group differences over time for the intervention and control conditions

Measure	Time	Intervention		Control	
		Mean difference (SD) to baseline	ES (95% CI)	Mean difference (SD) to baseline	ES (95% CI)
IPTQ	Post-Trt	-3.72 (2.62)	**-1.68** (-2.20 to -1.17)	-0.24 (1.15)	0.02 (-0.42 to 0.46)
	4-wk FU	-1.63 (2.68)	**-0.69** (-1.15 to -0.24)	-0.28 (2.26)	-0.06 (-0.52 to 0.40)
	8-wk FU	-1.62 (2.52)	**-0.65** (-1.19 to -0.11)	-0.25 (2.61)	**-0.20** (-0.73 to 0.33)
AFQ-Y8	Post-Trt	-3.28 (3.09)	**-0.63** (-1.08 to -0.17)	-0.79 (2.07)	-0.13 (-0.57 to 0.31)
	4-wk FU	-1.32 (4.83)	**-0.26** (-0.71 to 0.18)	+0.21 (4.95)	0.08 (-0.38 to 0.54)
	8-wk FU	-1.52 (5.06)	-0.18 (-0.71 to 0.35)	+0.29 (5.04)	0.00 (-0.52 to 0.53)
SCS-SF*	4-wk FU	+2.74 (5.67)	**0.43** (-0.02 to 0.88)	+0.18 (5.07)	0.03 (-0.43 to 0.49)
	8-wk FU	+0.90 (5.74)	0.00 (-0.53 to 0.52)	+0.62 (4.75)	0.04 (-0.48 to 0.57)
RSES*	4-wk FU	+0.54 (2.68)	0.12 (-0.32 to 0.57)	-0.12 (2.37)	-0.07 (-0.53 to 0.39)
	8-wk FU	+1.40 (3.66)	0.07 (-0.46 to 0.60)	+0.52 (2.87)	0.01 (-0.52 to 0.54)
RCADS-25	4-wk FU	-2.03 (6.82)	-0.18 (-0.62 to 0.27)	-0.50 (5.62)	-0.03 (-0.49 to 0.43)
	8-wk FU	-2.19 (8.23)	0.07 (-0.45 to 0.60)	+0.14 (7.22)	0.04 (-0.49 to 0.57)
Anxiety	4-wk FU	-1.55 (4.06)	**-0.25** (-0.70 to 0.19)	-0.91 (2.92)	-0.11 (-0.58 to 0.35)
	8-wk FU	-1.86 (4.75)	-0.02 (-0.54 to 0.51)	+0.10 (3.55)	0.00 (-0.52 to 0.53)
Depression	4-wk FU	-0.47 (3.94)	-0.07 (-0.52 to 0.37)	+0.41 (3.77)	0.07 (-0.39 to 0.54)
	8-wk FU	-0.33 (4.23)	0.19 (-0.34 to 0.72)	+0.05 (4.24)	0.07 (-0.46 to 0.60)

Note: Post-Trt = post-treatment; wk = week; FU = follow-up; ES = effect size (Hedge's g); CI = confidence interval. Small-large effect sizes are denoted in bold. For measures marked with an asterisk, a positive ES is favourable. For all other measures, a negative ES is favourable.

Measures: IPTQ = Implicit Personality Theory Questionnaire; AFQ-Y8 = Acceptance and Fusion Questionnaire for Youth–Short Form; SCS-SF = Self-Compassion Scale–Short Form; RSES = Rosenberg Self-Esteem Scale; RCADS-25 = Revised Children's Anxiety and Depression Scale–Short Version; Anxiety = RCADS-25 Anxiety-Subscale; Depression = RCADS-25 Depression-Subscale

Overall, results were consistent with the analyses reported in the main empirical paper and further suggested that the intervention may have had positive effects. For the control group, differences to baseline were negligible on all measures over time, except for the IPTQ, which indicated a small improvement at the 8-week follow-up. Conversely, large improvements from baseline were estimated for the IPTQ in the intervention condition, alongside moderate improvements for the AFQ-Y8, at post-intervention. At 4-week follow-up, moderate improvements were estimated among the intervention group for the IPTQ, together with small improvements for the AFQ-Y8, SCS-SF, and RCADS-25 anxiety-subscale. At 8-weeks, moderate improvements were estimated among the intervention group for the IPTQ only.

Key events and themes from the researcher's diaries

Recruitment and consent

No students contacted the researcher directly in response to advertisement of the study on the educational institutions' online learning portals/intranet pages. Rather, all students were recruited via teachers/educational staff. All participants reported that they understood the information sheets and consent forms.

Outcome measures

The outcome measures took participants 5-15 minutes to complete per time point. All participants completed the measures without support, although one participant asked the researchers about the meaning of a word.

Technological issues

It was important to ensure that access to the intervention weblink was permitted by computer technicians at each institution. At one sixth form, volume was limited on the students' computer accounts, making it difficult for some participants

to hear the animation and videos. The intervention webpage crashed on three occasions and participant responses were not saved. Such technological issues should be considered in any future trials.

Logistics and burden

Researchers noted that having participants enter and leave their classes multiple times per day may have been potentially disruptive for teachers and peers. They highlighted that the research design could be changed so that participants only need to leave once per time point. For example, by consenting participants and administering baseline measures in advance, rather than on the day, of the intervention (otherwise participants need to leave class once to consent then return to complete the computer session when everybody else has consented).

Content analysis

Initial reflections on the participants' letters

In response to the four questions outlined by Erlingsson and Brysiewicz (2017), the researcher made the following reflections:

1) What is the text talking about?
    - Students have written letters of encouragement to younger pupils.
    - These letters are very reflective of the intervention content (e.g. ideas about self-compassion, the fluidity of thoughts and feelings, acceptance, the possibility of change, doing what is important to you).
2) What stands out?
    - Normalisation and acceptance of difficult emotions.
    - Emphasis that feelings are the brain's way of protecting us.
    - Hope for the future, growth, and change.

- Expressions of compassion.
- Emphasis on doing what you value in life.
- We are not defined by thoughts and feelings.
- Seeking support from others.

3) How did I react reading the text?
    - It was moving and exciting to read these letters, as they suggested that the students grasped the intervention content.
    - Before reading these, I was skeptical about single-session interventions and thought that maybe the content was too difficult to understand in 15 minutes.
4) What message was I left with?
    - Ideas from the intervention seemed to be understood and/or accepted by the participants. Indeed, many messages from the animation and videos were repeated or reflected in the letters.
    - Perhaps these messages were internalised on a deeper level too - participants re-phrased content and brought up related ideas, which were not explicitly stated in the intervention (e.g. several mentioned that we are not defined by our thoughts and feelings, but this notion was not explicit in the intervention content).

Codes and categories

Figure 6.5 depicts example codes and categories that formed themes, to illustrate the process of analysis.

Figure 6.5: Codes and categories comprising themes from participants' intervention responses to the written task

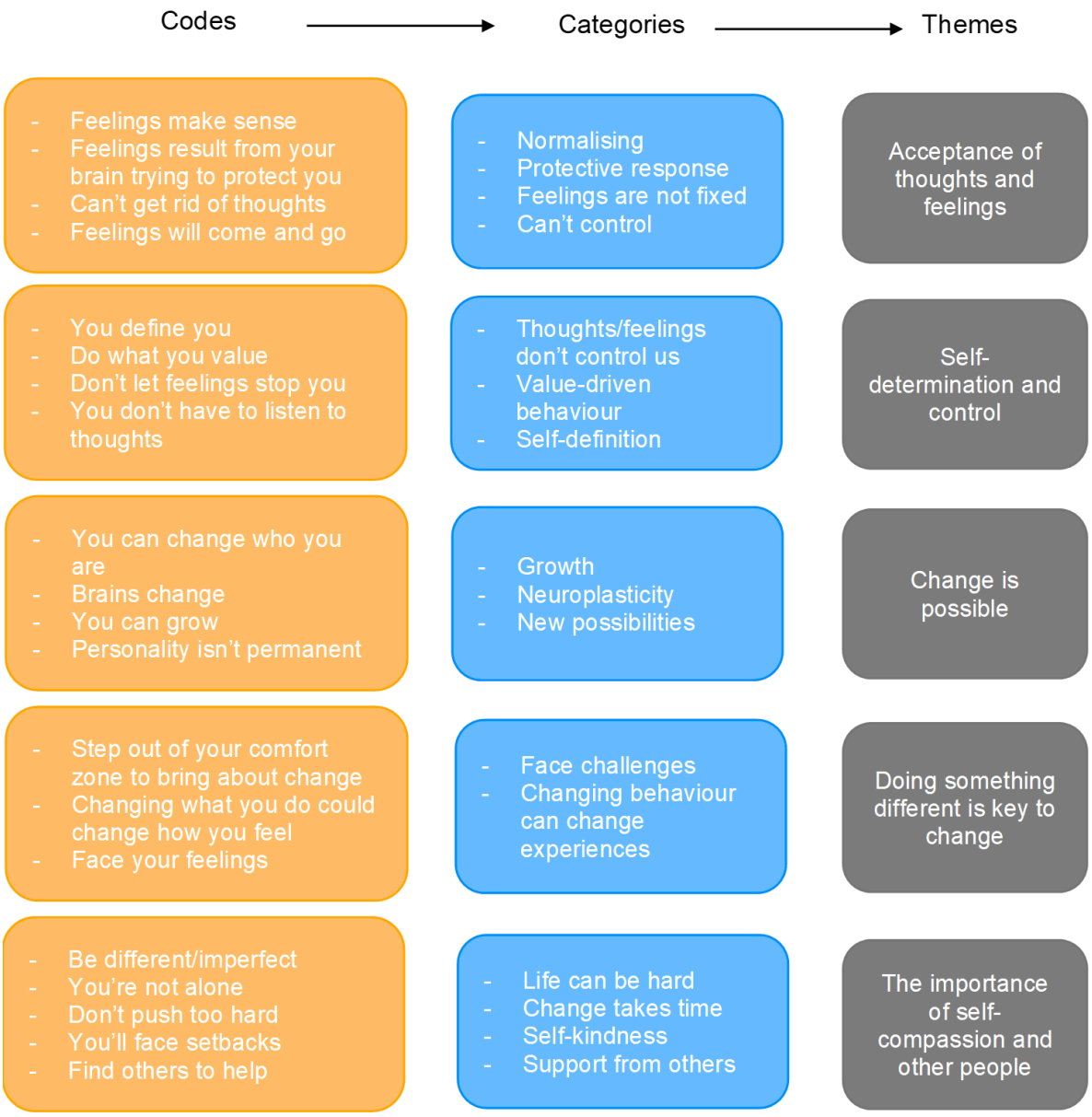


Figure 6.5. Depiction of the content analysis process.

CHAPTER SEVEN

Discussion and Critical Evaluation

Discussion and Critical Evaluation

The overarching aim of this thesis was to explore the use of third wave CBT and psychologically-based mindset interventions for young people. There is a growing agenda to ensure high-quality treatment within child and adolescent mental health services, and to employ universal methods more widely to engender positive emotional wellbeing amongst future generations (Department of Health [DoH] & Department for Education [DfE], 2017; Public Health England [PHE], 2019). Both third wave CBT and mindset interventions are universal approaches with potential relevance to treatment and public health, yet the evidence-base for use of these methods with children and adolescents was lacking. This research aimed to contribute to aforementioned gaps in the literature; namely to determine the effectiveness of third wave CBT across clinical and non-clinical settings by synthesising data from existing randomised controlled trials (RCTs), and to explore the feasibility of an integrative mindset intervention as a promotive public health tool for young people.

To knowledge, this thesis presents the first meta-analysis of third wave CBT for children and adolescents. The data from thirty RCTs were extracted and analysed using random-effects models. Many studies were of low quality and the impact of bias was assessed. Moderation and subgroup analyses were conducted to explore heterogeneity. Overall, results suggested that third wave CBT may be a promising intervention for young people across a wide range of, but not all, outcomes. There was some indication that interventions may be more effective in clinical as opposed to non-clinical settings, though moderation and subgroup analyses had significant limitations. Moreover, considerable effects were found for third wave processes, wellbeing, and flourishing, suggesting that third wave CBT could be relevant to the general

population. Further high-quality trials were warranted, especially to investigate third wave CBT as a public health tool to promote thriving among young people.

An empirical study followed from the meta-analysis, aiming to assess the feasibility of a psychologically-based mindset intervention, which integrated methods from third wave approaches, as a promotive tool in a general school sample of 16-18 year olds in the UK. An RCT design was used; 80 participants were recruited from sixth forms and colleges and allocated to either the intervention or usual school activities. The intervention was delivered in a single, 30-minute session via the computer. Overall, the intervention and study design was feasible and acceptable. Minimum recruitment targets were exceeded and attrition accumulated to 11% at 4-weeks and 48% at final follow-up. Student feedback was largely positive, and participants appeared to understand and engage with the intervention content. The intervention appeared to induce promising effects for primary outcomes of personality mindset and psychological flexibility. Moreover, it may have induced change for secondary outcomes of self-compassion, self-esteem, low mood, and anxiety. However, it was unclear whether intervention effects were maintained at the 8-week follow-up. Given that the intervention was so cost- and time-efficient, it was concluded that a full-scale evaluation was warranted. Potential areas of improvement to the intervention and research design were nonetheless noted.

Strengths and Limitations

The systematic review and meta-analysis offered a highly comprehensive synthesis of existing research. It assessed the effectiveness of third wave CBT for a

variety of outcomes at post-treatment and included a range of settings and populations (e.g. young people with physical and mental health conditions, as well as general school samples). This enabled assessment of third wave CBT as a universal intervention applicable across diagnostic categories and along the spectrum from ill-health to thriving. Several forms of third wave CBT were evaluated, as well as different modes of delivery (e.g. group versus individual therapy). Moderator and subgroup analyses were conducted. The review therefore allowed exploration of variation in effectiveness amongst outcomes, settings, and intervention characteristics, which is important for informing clinical practice. The review also explored follow-up data and offered a comparison of third wave CBT to other psychological therapies specifically.

Several measures were taken to ensure feasibility and validity in the face of inevitable heterogeneity from conducting such a broad review. For example, the research team carefully deliberated which forms of third wave CBT were to be reviewed, excluding interventions where it was thought they had a unique methodology (e.g. interpersonal/psychodynamic stance) that may not be reliably pooled with cognitive-behavioural techniques. Where there was controversy in the literature regarding which interventions constituted third wave CBT, and which aligned more comfortably as "integrative" approaches, a decision regarding inclusion was made in collaboration among the researchers, based on clinical knowledge and experience.

Moreover, separate meta-analyses were conducted for distinct outcomes to increase homogeneity. The chosen outcome categories (and the measures that encompassed them), moderators, and subgroups were also carefully deliberated and clinically-informed. A strength of conducting the review as a scientist-practitioner was

that the research design was simultaneously relevant to clinical practice whilst methodologically rigorous (Shapiro, 2002). For example, to ensure that several moderators and subgroups could be investigated without the risk of data dredging – that is, the misuse of numerous analyses in search of a statistically-significant result – the analysis plan was decided *a priori* and the review was pre-registered (Marshall, 1990). Corrections for multiple comparisons were also employed (Holm, 1979).

The framework used to assess the quality of trials included within the review was also clinically- and scientifically-informed. Assessment of bias and quality is an integral step of the review process, which has vital implications for the conclusions drawn (Cuijpers, 2016). Despite this, many psychological reviews have given limited attention to quality (e.g. Weisz, McCarty, & Valeri, 2006; Eckshtain et al., 2020). Moreover, multiple bias assessment tools exist, with no clear direction about which to choose for a meta-analysis. The Cochrane risk-of-bias assessments are widely upheld (Sterne et al., 2012; Eldridge et al., 2016), but they focus on research processes (e.g. blinding, randomisation), whilst clinically-important information relevant to quality is lacking (e.g. generalisability of the sample, quality/replicability of the intervention). Nonetheless, such information could be essential to inform further research and clinical practice, especially with regard to novel interventions. The applicability of the Cochrane tools for psychological treatments has been questioned in particular (Martins Scalabrin, Mello, Swardfager & Cogo-Moreira, 2018).

In the study protocol, it was planned that the Cochrane risk-of-bias tool would therefore be supplemented with a narrative appraisal of quality, addressing clinical issues relevant to psychology. Nonetheless, a NICE appraisal checklist, covering matters such as generalisability and intervention replicability, was subsequently identified (NICE, 2012). A decision was made prior to data extraction and analysis to

use items from this tool, rather than a narrative approach, given it was standardised and thus reduced chance of bias. While the validity and reliability of integrated methods to quality assessment remain unknown, bridging scientific and clinical practice in this way is a unique and important role of psychologists for progressing research. Moreover, only three studies were re-categorised (i.e. from low to moderate-high quality, or vice versa) when using the supplemented tool as opposed to the Cochrane assessment alone, meaning the study's general conclusions are unlikely to have been vastly altered.

Overall, the methodological and analytical approach for this review was strong, enabling substantial conclusions to be made about the effectiveness of third wave CBT for young people based on existing research. One notable limitation, however, was that moderator and subgroup variables were not explored in conjunction, as the complexity required for such an analysis was beyond the scope of a clinical psychology thesis project that also encompassed a substantial empirical study. Limitations of the review largely resulted from the scarcity and quality of existing RCTs available for synthesis. For example, there were limited follow-up data and few studies were conducted with general samples exploring third wave CBT as a preventative and promotive tool. Moreover, there were often not enough data to conduct planned moderation and subgroup analyses, which would have been helpful to explore widespread heterogeneity. The systematic review and meta-analysis reported should be repeated after further high quality trials have emerged.

The feasibility design for the empirical study was also clinically-informed and methodologically rigorous. For example, multiple educational institutions were approached to increase sample size and aid generalisability. Outcome measures were carefully chosen, with regard for what might be sensitive and appropriate to promotive

mental health interventions. These were administered at baseline, post-treatment, and follow-up, considering that maintenance effects are important to evaluate intervention tools. Moreover, educational staff and young people inputted into the research and intervention design through a workgroup. The intervention content was carefully deliberated through discussions between multiple psychologists, with consideration of existing psychological mindset interventions and their possible limitations, such as overlooking mindsets beyond personality that are relevant to mental health, or promoting views of malleability without considering the increased risk of self-blame and striving.

A feasibility design was appropriate for the empirical study because psychological mindset interventions have not been previously explored in the UK education system. Moreover, this was the first mindset intervention known to incorporate third wave constructs with the aim to balance ideas about growth and change with self-compassion and acceptance. The intervention also focused more on mindsets related to transient psychological experiences, such as thoughts and feelings, compared to preceding research. Thus, a large scale trial would have not been suitable as an initial line of enquiry (Orsmond & Cohn, 2015). Whilst null-hypothesis significance testing was not conducted, which would have allowed for an investigation into effectiveness, this was appropriate given that feasibility designs are underpowered. A strength of this study, nonetheless, was the use of multiple methods to assess change alongside between-group differences.

It is important to note, however, that the empirical study had several limitations. For example, no measure existed to explicitly capture mindsets relating to transient psychological factors, making it difficult to evaluate this key outcome. Whereas a measure of psychological flexibility was included (Greco, Lambert & Baer,

2008), which was arguably reflective of the intervention content, it was not designed as a direct assessment of mindset.

Although the researchers attempted to create a transient psychological mindset measure, its reliability was extremely poor, so it could not be used to evaluate the intervention. This was a feasibility study so pre-piloting the measure was not essential, nor possible within the scope of a trainee research project. Nonetheless, it is recommended that a reliable and valid measure of transient psychological mindsets is developed and included alongside a personality mindset measure within any future trials. Given the integrative nature of the intervention, both measures should aim to capture notions of change balanced with acceptance and self-compassion. To evaluate universal promotive interventions, it may also be important to measure school-/community-level effects, in addition to individual-level outcomes such as emotional difficulties or wellbeing (PHE, 2019).

There was high sample attrition by the final follow-up due to an unexpected timetabling conflict. Future trials should have sufficient power to investigate whether the intervention induces long-standing effects, given this has important implications for practice. Moreover, intention-to-treat methods should be employed, whereby all participants randomised are included within estimations of intervention effects, regardless of noncompliance, protocol deviations, and drop-out. This helps ensure that results are reflective of a real-world scenario (Gupta, 2011). Intention-to-treat analyses were not utilised in the empirical study given the feasibility design was not intended to determine effectiveness.

Clinical and Theoretical Implications

Third wave CBT has been increasingly used as a treatment strategy in child and adolescent services, and more widely across school and community settings. Nonetheless, evidence regarding its effectiveness has been lacking. Following the systematic review and meta-analysis, clinicians can feel more confident to use third wave CBT with young people to target a variety of outcomes, including: emotional symptoms/internalising problems, interference from difficulties, third wave processes, wellbeing/flourishing, and quality of life. It is, nonetheless, important for clinicians to note that some effects were non-significant (including for behavioural difficulties/externalising problems and physical health/pain), alongside limitations of the review. For example, there was considerable heterogeneity and it is possible that third wave CBT is more effective in some circumstances or populations than others. Further, many of the RCTs available for review were of poor quality, and conclusions may be subject to change as more rigorous trials emerge.

Third wave CBT is founded on universal biopsychosocial-spiritual models, such as relational frame theory and evolutionary approaches (e.g. Gilbert, 2010; Hayes, 2016). As a result, it is argued to target common human processes that are relevant along the spectrum from ill-health to flourishing and across diagnostic categories (Hayes & Hoffman, 2017). This can be contrasted to other prevalent approaches, including those that are focused on psychopathology alone, viewing difficulties as a disease or biological illness, as well as those that categorise human experiences and apply disorder-specific models and interventions (Hayes & Hoffman, 2017). The review upholds the theoretical and clinical stance that interventions are effective where they target universally-relevant processes, such as acceptance, mindfulness, self-compassion, and values-accordant behaviour.

Interventions targeting mindsets about psychological factors (i.e. people's fundamental beliefs about personality, thoughts, feelings, and behaviour) are another promising universal approach. The empirical study suggested that a single-session psychological mindset intervention was a feasible and acceptable promotive mental health strategy for young people. Whilst analyses were limited given this was a feasibility assessment, outcome data were promising. More research is needed before such interventions are implemented in the UK education system, and there are potential areas for improvements. For example, given that mindsets are beliefs and attitudes, thus are likely shaped by repeated interactions and experiences over time, interventions might be most effective when embedded within the day-to-day ethos of educational institutions (e.g. Mueller & Dweck, 1998).

It was difficult to ascertain whether third wave CBT and mindset approaches targeted different universal processes, as the intervention studied incorporated both methods. Nonetheless, mindsets have been found to correlate with emotional health (e.g. Schroder et al., 2015) and previous interventions focused on mindsets alone have proven effective (e.g. Schleider & Weisz, 2016; 2018). Whilst there are undoubtedly overlaps between third wave CBT and mindset approaches, with regard to the notions promoted (e.g. values-accordant behaviour) and techniques employed (e.g. meta-cognitive), there are also distinctions; with mindset interventions emphasising constructs or methods associated with malleability and change, and third wave approaches emphasising constructs or methods associated with acceptance, mindfulness, and self-compassion. It is therefore plausible that each target distinct processes fundamental to mental health and wellbeing.

It has been recently advised for clinicians to move away from being constrained within specific intervention models, to assimilate processes from different

approaches, in order to most effectively meet need (Hayes & Hoffman, 2019). To this end, an integrative stance was taken, balancing change-based methods from preceding mindset interventions (as well as from first and second wave CBT), with acceptance-based methods from a variety of third wave approaches. Whilst this meant that the empirical study did not address aforementioned gaps in the literature with regard to exploring third wave CBT or mindset interventions as isolated, promotive strategies within educational settings, the research offered an important and unique contribution to the literature with regard to an exploration of an assimilative approach.

It is thought that, currently, many clinicians feel apprehensive to use integrative approaches (Hayes & Hofmann, 2017), perhaps because of a lack of research or published examples. Moreover, different therapies have been pitched against one another historically, causing polarisation, even amongst generations of CBT. Nonetheless, highly-acclaimed clinicians have emphasised that the metaphor of a "wave" to describe new therapeutic approaches was never intended to "wash away" and displace earlier work; rather, that "waves hitting a shore assimilate and include previous waves" (Hayes & Hofmann, 2017, p. 245). The empirical study contributed to clinical and theoretical knowledge, by illustrating the possibility of balancing models, even when they may at first seem discordant (i.e. change and acceptance). Integrative approaches may be particularly appropriate for children and adolescents to support well-rounded growth and development (Hayes & Ciarrochi, 2015).

Overall Conclusions

Improving the mental health and wellbeing of children and adolescents is a global priority. Universal approaches, applicable across diagnostic categories and

along the continuum from ill-health to flourishing, have been of increasing interest to clinical services and public health. This thesis portfolio contributed to current research by pooling data from randomised trials to determine the effectiveness of third wave CBT for young people across clinical and non-clinical settings. It also investigated the feasibility of an integrative mindset intervention as a promotive mental health tool within the UK education system. Findings suggested that health, wellbeing, and thriving may be promoted among young people by using third wave CBT approaches and shaping psychological mindsets. Limitations and areas for future research were discussed.

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Appendix A: Clinical Psychology Review Author Guidelines

CLINICAL PSYCHOLOGY REVIEW

AUTHOR INFORMATION PACK

TABLE OF CONTENTS

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DESCRIPTION

Clinical Psychology Review publishes substantive reviews of topics germane to **clinical psychology**. Papers cover diverse issues including: psychopathology, psychotherapy, behavior therapy, cognition and cognitive therapies, behavioral medicine, community mental health, assessment, and child development. Papers should be cutting edge and advance the science and/or practice of clinical psychology.

Reviews on other topics, such as psychophysiology, learning therapy, experimental psychopathology, and social psychology often appear if they have a clear relationship to research or practice in **clinical psychology**. Integrative literature reviews and summary reports of innovative ongoing clinical research programs are also sometimes published. Reports on individual research studies and theoretical treatises or clinical guides without an empirical base are not appropriate.

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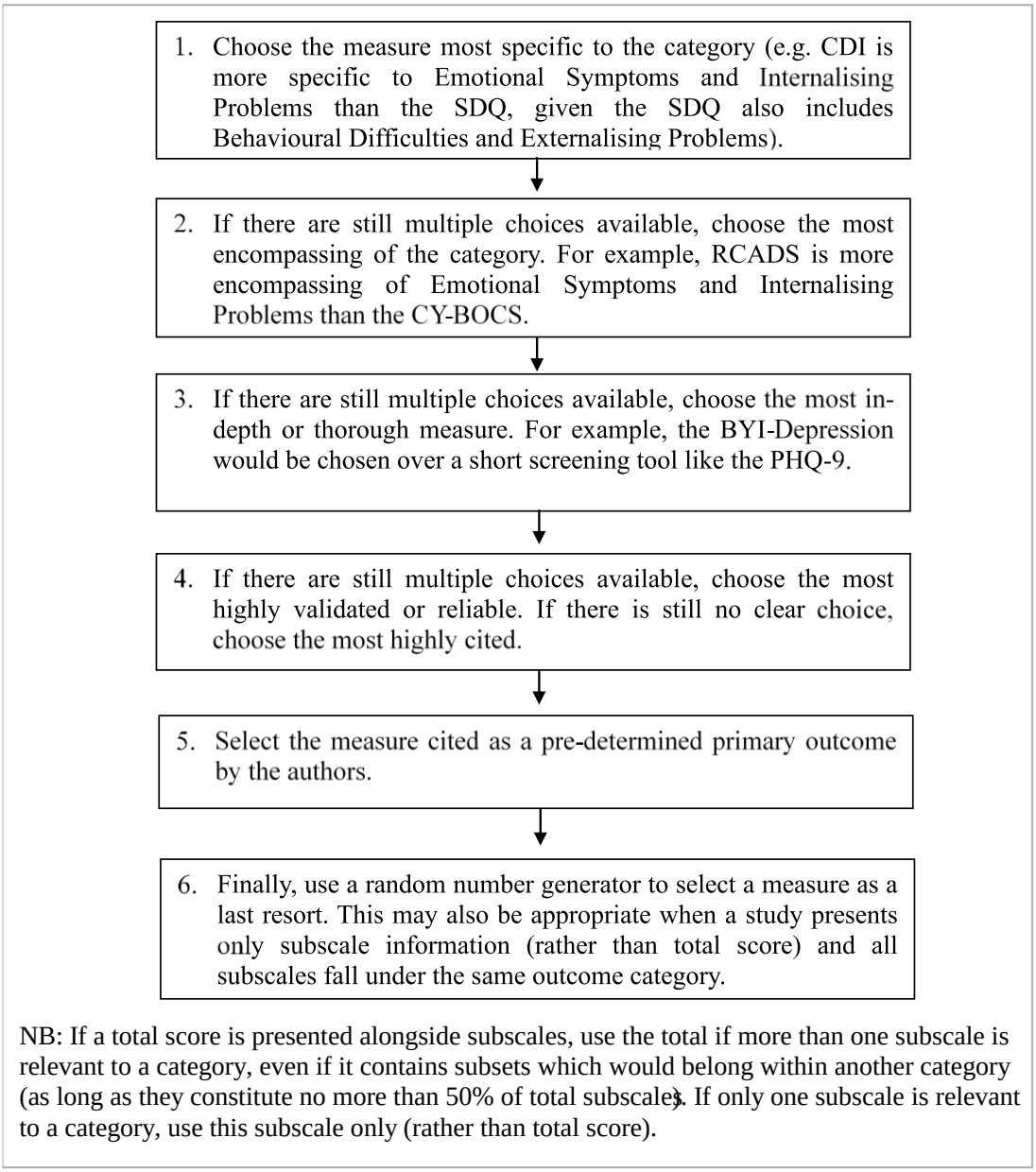
Appendix B: Outcome Measure Selection Procedure for the Systematic Review



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Flow chart to select outcome measures when there were multiple possibilities within a single study. This procedure was decided collaboratively by the research team prior to extraction.

Appendix C: Measures by Outcome Category for the Systematic Review

Category	Example Measures (list not exhaustive)
Emotional Symptoms and Internalising Problems	Perceived Stress Scale; Child Stress Reaction Checklist – Short Form; Childhood Trauma Questionnaire; Children's Yale-Brown Obsessive Compulsive Scale; Social Anxiety Scale; State-Trait Anxiety Inventory; Children's Depression Inventory; Reynold's Adolescent Depression Scale-2; Affective Control Scale; Difficulties with Emotion Regulation Scale; Strengths and Difficulties Questionnaire; Revised Child Anxiety and Depression Scale
Behavioural Difficulties and Externalising Problems	Child Behaviour Checklist; Eyberg Child Behaviour Inventory; Attention Control Scale; Inventory of Interpersonal Problems; State-Trait Anger Expression Inventory-2; Inventory of Callous and Unemotional Traits for Youth; Difficulties with Emotion Regulation Scale; Strengths and Difficulties Questionnaire
Interference from Difficulties	Pain Interference Scale; Pain and Impairment Relationship Scale; Children's Anxiety Life Interference Scale; Child and Youth Resilience Measure; Avoidance and Fusion Questionnaire Youth
Third Wave Processes	Meta-Cognitions Questionnaire; Child and Adolescent Mindfulness Measure; Mindful Attention Awareness Scale; Acceptance and Action Questionnaire; Avoidance and Fusion Questionnaire Youth
Wellbeing and Flourishing	Valued Living Questionnaire; Flourishing Scale; Children's Hope Scale; Meaning in Life Questionnaire; Social Connectedness Scale; Rosenberg Self-Esteem Scale; Self-Compassion Scale, Meaning in Life Questionnaire
Quality of Life	Satisfaction with Life Scale; Student's Life Satisfaction Scale; Affect Balance Scale; Pediatric Quality of Life, Enjoyment and Satisfaction Questionnaire; Child Health Questionnaire
Physical Health and Pain	Functional Disability Inventory; Pain Intensity Rating; Pain Coping Questionnaire; Short Form (36) Health Survey; Special Health Self-Efficacy Scale; Hospital Visits; BMI; Number of Days with Symptoms

NB: Some measures were relevant across multiple categories (e.g. Strengths and Difficulties Questionnaire). Sometimes subscales were used in analyses rather than the total measure (see Appendix B).

Appendix D: Additional References for the Systematic Review

(Those included in the meta-analysis/supplementary material but not cited in the main text)

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Appendix E: Journal of Clinical Child and Adolescent Psychology Author Guidelines

Journal of Clinical Child & Adolescent Psychology ›

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- Formatting and Templates
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- Using Third-Party Material
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- Data Sharing Policy
- Publication Charges
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Appendix F: Participant Information Sheet for the Empirical Study

Version 2.0 12/03/2019
Ethics Reference Number: 201819 - 045

University of East Anglia

PARTICIPANT INFORMATION SHEET

Study Title: A Brief Psychological Mindset Intervention to Promote Mental Health in UK College Students: A Feasibility Study

Why have I been given this information sheet?

We would like to invite you to take part in our research study. Please read this information carefully and talk to your parent or guardian about the study if you wish. Feel free to ask us anything that is not clear or if you want to know more. Take time to decide if you want to take part.

Why are we doing this research?

This study is exploring a new animated learning resource that could be used in schools to promote mental health. It is computer-based and delivered in a single session, lasting 30 minutes. It teaches young people about thoughts, feelings, personality and the brain. It includes ideas about "mindsets" – or beliefs about the brain – which may be helpful in day-to-day life, both in and out of sixth form or college.

Why have I been asked to take part?

When new resources to promote mental health are made it is good to run a small trial of them first, called a feasibility study. This will help us to see whether the resource is suitable, practical and liked by students and teachers. It will also help us to check the best way to measure whether the resource helps or not.

We are looking for 50-120 students age 16-18 years to take part in our study. We would like a wide mix of different people to get involved. The animated learning resource aims to promote good mental health not treat mental illness, so everyone is suitable to take part no matter how they would currently rate their emotional health. We cannot include people who are involved in other mental health research studies at their sixth form or college.

Who is organising and funding the research?

Our research team is made up of four people:

Amorette Perkins	Trainee Clinical Psychologist at the UEA, employed by the NHS
Joseph Cassidy	Trainee Clinical Psychologist at the UEA, employed by the NHS
Dr Gemma Bowers	Clinical Lecturer at the UEA
Dr Richard Meiser-Stedman	Clinical Reader in Clinical Psychology at the UEA

Amorette Perkins will be leading this research as part of her training to be a clinical psychologist. The project is paid for by the training programme.

Who has checked the study?

This study has been checked by the UEA Faculty of Medicine and Health Sciences Research Ethics Committee and approved for meeting ethical and legal rules. The study has also been given the go ahead by the Principal or Head Teacher at your sixth form or college.

Do I have to take part?

No – it is entirely up to you! Taking part is completely voluntary and we do not want you to feel any pressure to get involved. None of your teachers nor the researchers will mind if you don't. We urge

you not to feel pressured by your peers into taking part or not. If you don't wish to get involved, you don't need to give a reason and there will be no consequences for your education or rights.

What will happen if I decide to take part?

A person from the research team will meet with you on your own to answer any questions. You will be asked to sign a consent form saying that you understand what the study involves. If you do consent to take part, you will then be asked to complete some short measures related to mental health (e.g. about your self-confidence).

Next, you will be randomly put in one of two groups – either a group who receive the computer session first or a group who receive the session later. The group going first will get a chance to do the session that day. Once this group has finished on the computers, both groups will be asked to fill out some short measures.

The researchers will return to your sixth form or college twice after this and ask for all the measures to be completed again – once between 4-6 weeks and again at 8-10 weeks. Doing the measures shouldn't take more than around 15 minutes each time. On our final visit, you will also be asked to fill out a feedback questionnaire, telling us your views and experiences. Once these have been completed, the second group who have still not received the computer session will get a chance to do it. Your part in the research will then be finished.

Where and when will the study be done?

All the things you will do during the study (e.g. the computer task, filling out measures) will be at your sixth form or college during school hours. We are aiming to start the study in [MONTH, YEAR] and finish by [MONTH, YEAR].

How much of my time will it take?

We expect that doing the consent forms, measures, one-off computer session and feedback questionnaires will take a maximum of three hours.

What information will be collected and how will it be used?

We will be collecting different bits of information to help us evaluate the animated learning resource and the research study in general.

- Your age, gender and ethnicity will be recorded so we know who took part.
- If you withdraw part-way through the study, we will ask you the reason why as this could help us reduce dropout in the future (though you don't need to tell us if you don't want to!).
- During the computer task, you will be asked to respond to questions and tasks. Your responses will be recorded and studied by the researchers to evaluate the session.
- We will record your responses on the mental health measures.
- We will also collect information about your school attendance from 10 weeks before the study begins until when the study ends, including how much you were off and type of absence (e.g. unauthorised, sickness or other).
- We will ask you and some teachers to complete a feedback questionnaire. You will be asked to rate your emotional health over the study period.
- Throughout, the researchers will be writing down what it was like to do the study.

You have the right to access any personal data we collect – you can ask us in person or in writing.

What if I agree to take part then do not want to do the research anymore?

You are free to stop taking part at any time during the research without giving a reason. If you decide to stop, this will not affect your education or rights. Once you have started redoing measures and have handed them in, everyone's data will be put together for analysis, meaning it would not be possible to withdraw any information you have given before that point. Though, you will not be asked to give any more information.

Will anyone else know I'm doing this?

When you were given this information sheet, you would have been offered a copy addressed to parents/guardians telling them about the study. It is your choice whether or not to give your parents/carers this letter and tell them if you decide to take part - we will not be informing them. The people in our research team and some of your teachers will know that you are taking part. Your teachers will know so that they can organise for you to complete the study at your sixth form or college and so we can access your attendance records. Your peers may see that you are taking time out of usual sixth form or college activities and guess that you are taking part. When you do the computer task, it will be in a room with the other participants, so you will know one another is taking part.

Who will see the information collected about me?

All information collected during the study (e.g. your answers on the measures) will be treated as strictly confidential and only members of the research team will be able to look at it. Your teachers and others will not have access. Though, a quality check may be done by another team at the UEA to ensure that the study keeps to high standards, in which case they would also access all study information.

The researchers will follow EU General Data Protection Regulations 2018. All paper information will be kept in a locked cabinet in a locked office at the UEA and anything uploaded to the computer will be password protected. To further protect anonymity, you will be given a number to write on forms and measures rather than your name.

The only time that we may break confidentiality is if you tell us something that suggests you or somebody else is at significant risk of harm in any way. If that happens, we may need to report it to a senior member of staff at your sixth form or college for action by them. In that case, we would always try to discuss this with you first.

Is there anything I should be worried about if I take part?

We do not expect the study to have any risks to your wellbeing, but it is always possible that you might find something in the computer session or measures sensitive or upsetting. If you feel this way, please let us know and a researcher will ask if you wish to continue and remind you that you can withdraw. If you need further support, we will guide you to contact the appropriate staff members at your sixth form or College. If you score highly on our measures of anxiety or depression, we will let you know and recommend that you speak to [name of the key contact teacher] or another appropriate person at your sixth form or college.

You may need to take time out during college hours to take part in the study. We have tried to keep this time to a minimum. We hope that the benefits of taking part will outweigh this cost.

Will taking part help me?

The animated learning resource aims to promote and protect mental health. We predict it might have some benefits for your emotional wellbeing, though we do not know for certain. You might learn

something new or find it rewarding to know you have been part of research which could be used to help promote mental health in schools.

It is important to know that this is a research study, not a form of treatment for mental health problems. Therefore, if you are worried about your mental health or wellbeing, please speak to [NAME OF KEY CONTACT TEACHER FOR THE STUDY], another appropriate person at your sixth form or college and/or your GP. Or you can contact either:

MAP
Tel: 01603 766994 or www.map.uk/net

Samaritans
Tel: 116 123

Young Minds Crisis Messenger Text YM to 85258

What happens when the study finishes?

At the end when all measures are finished and both groups have done the computer task, all those who took part at your sixth form or college will be brought together for the chance to discuss their experiences and have any questions answered by the researchers.

What happens to the results of the research?

We plan to share the results of this study in presentations, publications and using media. On the consent form you will be asked if you would like us to share a copy of the findings with you. If you mark "yes", we will send this to you as soon as it is done. We aim for this to be within a year after you have finished taking part. Other researchers working on similar topics might ask to look at the results of our study as it could help them with their own research. However we share results, it would always be anonymous and unidentifiable so no one would know you took part.

Following UEA guidance, information collected during the study will be kept safely for at least 10 years following any publications before being destroyed.

What if there is a problem or something goes wrong?

If you are worried about anything relating to the research, please speak to someone from the research team and we will try our best to help you.

If you have a complaint about the research or researchers, please contact the Head of the Clinical Psychology Doctoral Programme at clinpsyd@uea.ac.uk. This person is separate from this research study so you can speak to them confidently.

How can I find out more?

You can contact Amorette Perkins: Email: amorette.perkins@uea.ac.uk Tel: 07935 160558

What happens next?

After giving you this information sheet, you will hear nothing from us for at least 24 hours. This is to make sure that you have time to read the information and fully consider if you would like to take part. You will then be asked if you want to be involved or speak more with the researchers. If you decide that you would rather not take part in this study, you do not need to give a reason and no further contact will be made.

Thank you very much for considering this research,
The Research Team

Appendix G: Letter to Parents/Guardians for the Empirical Study

University of East Anglia

Department of Clinical Psychology
Norwich Medical School
Faculty of Medicine and Health Sciences
University of East Anglia
Norwich
NR4 7TJ

Dear Parent/Guardian,

We are researchers from the University of East Anglia (UEA). Your child's sixth form or college are hosting one of our research studies and your child has been invited to take part. We would like you to understand why the research is being done and what it will involve for your child.

This letter details further information about the study. You are welcome to contact us if you would like to know more or have questions.

Study title:

A brief psychological mindset intervention to promote mental health in UK college students: A feasibility study.

What is the purpose of the study?

The Government want to link mental health services and schools to build young people's resilience and improve emotional wellbeing. This study is exploring a new intervention that could be delivered in schools on the computer to promote mental health. It teaches young people about thoughts, feelings, personality and the brain. It is called a "mindset" intervention, because it looks at people's "mindsets" or beliefs about the brain.

Who is organising and funding the research?

Our research team is made up of four people:

Amorette Perkins	Trainee Clinical Psychologist at the UEA, employed by the NHS
Joseph Cassidy	Trainee Clinical Psychologist at the UEA, employed by the NHS
Dr Gemma Bowers	Clinical Lecturer at the UEA
Dr Richard Meiser-Stedman	Clinical Reader in Clinical Psychology at the UEA

Amorette Perkins is leading this research study as part of her doctoral thesis. The project is paid for as part of her clinical psychology training programme.

Who has reviewed the study?

All research organised by the UEA is looked at by an independent group of people, called a Research Ethics Committee. This study has been reviewed and approved for meeting ethical and legal requirements by The Faculty of Medicine and Health Sciences Research Ethics Committee at the UEA. It has also been approved by the Principal or Head Teacher at your child's sixth form or college.

Why has my child's sixth form or college been chosen?

When new interventions are made, it is important to run a small trial of them first, called a feasibility study. This will help us to see whether the intervention is suitable, practical and liked by students and teachers. It will also help us to check the best way to measure whether the intervention works or not. We can use the information to change the intervention or the way we measure it ready for a larger-scale study that could help us determine whether the intervention works across a wider range of schools.

Several local sixth forms and colleges have been asked to get involved with this research project. Your child's sixth form or college is one of them that agreed to host the study. They have a number of young people from a mix of backgrounds, so we can get different views on the intervention. We are looking for 50-120 students age 16-18 years who can consent to take part. The aim of the intervention is to promote good mental health, not treat mental illness, so everyone is suitable to take part regardless of how they would currently rate their emotional health.

Does my child have to take part?

No, taking part is completely voluntary and there will be no impacts on your child's education and rights if they do not wish to get involved. Your child has been given an information sheet similar to this one and asked if they want to take part. We have taken special measures to make sure that your child does not feel pressured into getting involved. If your child would rather not take part, they do not need to give a reason and no further contact will be made with them by the researchers.

What will happen if my child agrees to take part?

If your child reads the information sheet and says they would like to take part, a researcher would meet with them individually to answer any questions they have. They will then be asked to sign a consent form that says they understand what the study involves. Following this, they will complete a few short measures related to mental health (e.g. about self-confidence).

Next, your child will be put randomly in one of two groups – either a group who receive the intervention first or a group who receive the intervention later. The group going first will get a chance to do the intervention that day, which is a 30-minute computer task. Both groups will be asked to redo some measures once this group has finished.

The researchers will return to your child's sixth form or college twice after this and ask for the measures to be completed again – once between 4-6 weeks and again at 8-10 weeks. Doing the measures shouldn't take more than 10-15 minutes each time. At our final visit, they will also be asked to fill out a feedback questionnaire. The second group who have still not received the intervention will then get a chance to do the computer task. Your child's part in the research will then be finished.

What will happen if my child wants to withdraw from the study?

Your child is free to withdraw without any consequences for their education or rights. If your child wishes to withdraw from the study after starting to redo the measures, everyone's data will have been put together for analysis, meaning it would not be possible to withdraw any information they have given before that point. Nonetheless, participants will not be asked to give further information. This will be made clear on the information sheet given to your child.

What information will be collected and how will it be used?

We will be collecting lots of different bits of information to help us understand the intervention and the research study in general. This could help us make the intervention and the way we measure it better.

- Your child's age, gender and ethnicity will be recorded so we know who took part.
- If your child withdraws part-way through the study, we will ask the reason why as this could help us reduce dropout in the future. Though, they don't need to tell us if they don't wish to.
- During the computer task, your child will be asked to respond to questions and complete a written task. Their responses will be recorded and studied by the researchers as this could help us test the intervention.
- We will record your child's responses on the mental health measures.
- We will also collect information about your child's school attendance from 10 weeks before the study begins until when the study ends.
- We will ask your child and some teachers to complete a feedback questionnaire. The children will also be asked to rate any change in their emotional health over the study period.
- Throughout, the researchers will be recording what it was like to do the study to explore how to use the intervention and run further studies in schools.

Will my child's taking part in the research study be kept confidential?

All information gathered during the study will be treated as strictly confidential. The researchers will follow EU General Data Protection Regulations 2018. All paper information will be kept in a locked cabinet in a locked office at the UEA and anything uploaded to the computer will be password protected. Only members of the research team will be able to look at the personal information collected from this study. A quality audit may be done by another team at the UEA to ensure that the study keeps to high standards and follows protocol, in which case they would also access all study information. When we write up the findings to share with others or publish, the data used will be anonymous and unidentifiable.

The only time that we may break confidentiality is if your child tells us something that suggests they or somebody else is at significant risk of harm in any way. If that happens, we may need to report it to a senior member of staff at their sixth form or college for action by them. In that case, we would always try to discuss this with your child first.

Where will the study take place?

All parts of the study that your child will be involved in (e.g. the computer task, filling out measures) will take place at your child's sixth form or college during school hours.

What are the possible disadvantages or risks of taking part?

We do not expect the study to have any risks to your child's wellbeing and safety. It may be possible that the study causes your child to think about upsetting personal matters, like difficult thoughts or feelings. If your child gets upset at any point during the study, a researcher will ask if they wish to continue and remind them that they can withdraw. If they ask for advice regarding personal matters, they will be directed to appropriate staff at their sixth form or college. If your child scores highly on our measures of anxiety or depression, we will let them know and signpost them to speak to [NAME OF IDENTIFIED STAFF MEMBER] or another appropriate staff member at their sixth form or college.

Your child will take time out of their usual school activities to take part in the study. We expect that the maximum time to complete all parts is three hours. We have tried to keep this time to a minimum. We hope that the benefits of taking part will outweigh this cost.

What are the possible benefits of taking part?

The intervention might have a positive effect on their mental wellbeing. Your child might also learn something new about thoughts, feelings, personality and the brain. Some participants could find it rewarding to know they have been part of research which could be used to help promote mental health in schools.

What happens when the research study stops and what will happen to the results?

Once both groups have done the computer task, all those who took part at your child's sixth form or college will be brought together to have the chance to discuss their experiences and have any questions answered by the researchers.

We plan to share the results of this study in presentations, publications and using social or national media. We will make sure that no participants can be identified. When your child completes the consent form, they will be asked if they would like to receive a copy of the findings. If they mark "yes", we will send this to them as soon as it is finished.

Following UEA regulations, data will be kept securely for at least 10 years following any publications or presentations before being destroyed.

What if there is a problem?

This is a research study, not a form of clinical treatment. Therefore, if you are concerned about your child's mental health or wellbeing, please speak to your child's sixth form or college, your GP or call 999 in an emergency.

If you have a complaint about the research or researchers, please contact the Head of the Clinical Psychology Doctoral Programme at clinpsyd@uea.ac.uk. This person is independent from this research study.

What if I do not wish my child to take part in this research study?

By law, young people over 16 years old are presumed able to consent to take part in research by themselves. If you are concerned about your child taking part in this research, you can contact the researchers using the details below. A member of our research team would be happy to discuss any of the study details with you.

How can I find out more?

You can contact the research team:
Email: amorette.perkins@uea.ac.uk Tel: 07935 180558

Thank you for taking the time to read this information sheet. Please feel free to get in touch if you have questions, would like further information or if you have concerns about your child taking part in this research.

Yours sincerely,

Amorette Perkins
Doctoral Student in Clinical Psychology, University of East Anglia

Appendix H: Consent Form for the Empirical Study

Version 2.0, 12/03/2019
Ethics Reference Number: 201819 - 045
Study Identifier: A Brief Psychological Mindset Intervention
Participant Identification Number for this trial: ……...

University of East Anglia

CONSENT FORM

A Brief Psychological Mindset Intervention to Promote Mental Health in UK College Students: A Feasibility Study

Name of Lead Researcher: Amorette Perkins, University of East Anglia

Contact Information: amorette.perkins@uea.ac.uk

Please initial box

1. I confirm that I have read the information sheet dated 12/03/19 (version 2.0) for the above study. I have had the opportunity to consider the information, ask questions and have had these answered satisfactorily. ☐
2. I understand that my participation is voluntary and that I am free to withdraw at any time, without giving any reason, and without my education or rights being affected. ☐
3. I understand that if I withdraw from the study after starting to redo measures, I will not be able to withdraw the contribution I made before that point, but I will not be asked to give any more information. ☐
4. I understand that relevant sections of my school records (i.e. attendance) and data collected during the study, may be looked at by individuals from the University of East Anglia, where it is relevant to my taking part in this research. I give permission for these individuals to have access to my records. ☐
5. I understand that the information collected about me may be used to support other research in the future, and may be shared anonymously with other researchers. ☐
6. I consent to the storage and processing of personal information and data for the purposes of this study. ☐
7. I understand that the information gathered during the study will be treated as strictly confidential and handled in accordance with the EU General Data Protection Regulation 2018. I understand that confidentiality may be breached and senior teachers informed if the researchers are significantly concerned about risk to myself or others. ☐
8. I would like to receive a copy of the study's findings. Yes/No ☐
9. I agree to take part in the above study. ☐

Name of Participant	Date	Signature
Name of Person taking consent	Date	Signature

Appendix I: Self-Reported Change Measure for the Empirical Study

The following questions ask about your experiences of change since agreeing to join the research study. Please rate your change in the following areas:

1) How sure I am that my thoughts and feelings come and go.

1	2	3	4	5	6	7
Much less sure			Unchanged			Much more sure

2) How sure I am that the way my brain works can change over time.

1	2	3	4	5	6	7
Much less sure			Unchanged			Much more sure

3) How kind I am to myself, including when I have difficult thoughts and feelings or notice things I don't like about myself.

1	2	3	4	5	6	7
Much less kind			Unchanged			Much more kind

4) How good I feel about myself and my confidence.

1	2	3	4	5	6	7
Much worse			Unchanged			Much better

5) How worried and nervous I feel.

1	2	3	4	5	6	7
Much worse			Unchanged			Much better

6) How sad and low I feel.

1	2	3	4	5	6	7
Much worse			Unchanged			Much better

7) How much my thoughts, feelings and urges get in the way of doing what's important to me.

1	2	3	4	5	6	7
Much less			Unchanged			Much more

8) How much my thoughts, feelings and urges make sense to me.

1	2	3	4	5	6	7
Much less			Unchanged			Much more

9) How worried or upset I get about thoughts, feelings and urges when they come up.

1	2	3	4	5	6	7
Much less			Unchanged			Much more

Appendix J: Participant Feedback Questionnaire for the Empirical Study

Please answer all questions honestly – there are no right or wrong answers

SECTION 1

The following questions are focussed on your experience of the computer-based mindset session.

1) The mindset session made sense to me.

1	2	3	4	5	6	7	8	9	10
Definitely do *not* agree				Maybe agree					Definitely agree

2) The mindset session was hard to complete on the computer.

1	2	3	4	5	6	7	8	9	10
Definitely do *not* agree				Maybe agree					Definitely agree

3) I think the mindset session has been (or will be) helpful for me.

1	2	3	4	5	6	7	8	9	10
Definitely do *not* agree				Maybe agree					Definitely agree

4) I would recommend the mindset session to a friend or family member.

1	2	3	4	5	6	7	8	9	10
Definitely do *not* agree				Maybe agree					Definitely agree

5) I found the mindset session boring.

1	2	3	4	5	6	7	8	9	10
Definitely do *not* agree				Maybe agree					Definitely agree

Please provide any comments about the mindset session:

SECTION 2

The following questions are focussed on your experience of the research process itself:

1) I understood what the questionnaires were asking me.

1	2	3	4	5	6	7	8	9	10
Definitely do *not* agree				Maybe agree					Definitely agree

2) The questionnaires took too long to complete.

1	2	3	4	5	6	7	8	9	10
Definitely do *not* agree				Maybe agree					Definitely agree

3) I did not like being put in different groups at random.

1	2	3	4	5	6	7	8	9	10
Definitely do *not* agree				Maybe agree					Definitely agree

4) I enjoyed taking part in this research study.

1	2	3	4	5	6	7	8	9	10
Definitely do *not* agree				Maybe agree					Definitely agree

Please provide any comments about the research process:

Appendix K: Educational Staff Feedback Questionnaire for the Empirical Study

STUDY TITLE: A Brief Psychological Mindset Intervention to Promote Mental Health in UK College Students: A Feasibility Study

INFORMATION ABOUT COMPLETING THIS QUESTIONNAIRE

The Government want to link mental health services and schools to build young people's resilience and confidence. It is thought that by doing this, mental health problems could be prevented early on and young people will have better emotional wellbeing. Researchers at the University of East Anglia (UEA) have been exploring a new intervention that could be carried out in schools on the computer to promote mental health. It teaches young people about thoughts, feelings, personality and the brain. It is called a "mindset" intervention because it looks at people's "mindset" or beliefs about the brain. When new interventions are made it is good to run a small trial of them first, called a feasibility study. This helps us to see whether the intervention is suitable, practical and liked by students and teachers. It also helps us to check the best way to measure whether the intervention works or not.

Several students in your sixth form or college have taken part in a feasibility trial of this new intervention. We would like to invite you as teachers to complete this questionnaire to feed back about the intervention and the research process itself. We may use the information collected to change the intervention or the way we measure it ready for a larger study that could help us more carefully decide whether the intervention should be used to promote mental health in UK schools. This is an anonymous questionnaire and you are free to respond openly and honestly. It is entirely your choice to complete the questionnaire and there will be no impacts on your treatment or rights whether you decide to or not. All responses will be stored securely in accordance with the university's research policies and will not be accessible by anyone outside the UEA. When the feasibility trial is complete, we plan to share the results in presentations, publications and using social or national media. Any feedback shared will be anonymous and unidentifiable. A report detailing findings will be available to the school, should you wish to read this. By completing and returning this questionnaire you are consenting to the storage and use of the information you provide in the way proposed.

Depending on your level of involvement in this trial, you may feel unable to answer some of the questions. **Please feel free to leave these blank, answering only the questions which feel appropriate to you.** We welcome any additional comments or feedback in the open-response sections.

Please hand your completed questionnaires to [NAME OF CONTACT TEACHER], who will pass this on to the research team.

If you have any concerns, you can contact the research team on:
Tel: .....
Email: .....

Please answer all questions as honestly and accurately as possible

SECTION 1

The following questions are focussed on the intervention itself.

1) I feel like the students struggled to engage with the intervention.

1	2	3	4	5	6	7	8	9	10
Definitely do *not* agree				Maybe agree					Definitely agree

2) I think the intervention has benefitted (or will benefit) the students who completed it.

1	2	3	4	5	6	7	8	9	10
Definitely do *not* agree				Maybe agree					Definitely agree

3) It would be useful to have this intervention in schools.

1	2	3	4	5	6	7	8	9	10
Definitely do *not* agree				Maybe agree					Definitely agree

4) It was difficult to get the resources to run the computerised intervention in school.

1	2	3	4	5	6	7	8	9	10
Definitely do *not* agree				Maybe agree					Definitely agree

5) This intervention could fit within the school timetable.

1	2	3	4	5	6	7	8	9	10
Definitely do *not* agree				Maybe agree					Definitely agree

Please provide any comments about the intervention:

SECTION 2

The following questions are focussed on the research process itself.

1) It was easy to get students involved in the research.

1	2	3	4	5	6	7	8	9	10
Definitely do *not* agree				Maybe agree					Definitely agree

2) There was adequate support for students and staff during the research process.

1	2	3	4	5	6	7	8	9	10
Definitely do *not* agree				Maybe agree					Definitely agree

3) The measures used seemed appropriate.

1	2	3	4	5	6	7	8	9	10
Definitely do *not* agree				Maybe agree					Definitely agree

4) The research study consumed too much time.

1	2	3	4	5	6	7	8	9	10
Definitely do *not* agree				Maybe agree					Definitely agree

5) I did not like that students were randomly allocated to different groups.

1	2	3	4	5	6	7	8	9	10
Definitely do *not* agree				Maybe agree					Definitely agree

Please provide any comments about the research process:

Appendix L: Letter of Ethical Approval for Amendment to the Empirical Study

Faculty of Medicine and Health Sciences Research Ethics Committee

Amorette Perkins
MED

Research & Innovation Services
Floor 1, The Registry
University of East Anglia
Norwich Research Park
Norwich, NR4 7TJ

Email: fmh.ethics@uea.ac.uk

Web: www.uea.ac.uk/researchandenterprise

18 March 2019

Dear Amorette

Project title: A Brief Psychological Mindset Intervention to Promote Mental Health in UK College Students: A Feasibility Study

Reference: 201819 - 045

Thank you for your e-mail of 15 March notifying us of the amendments you would like to make to your above proposal. These have been considered and we can now confirm that your amendments have been approved.

Please can you ensure that any further amendments to either the protocol or documents submitted are notified to us in advance, and also that any adverse events which occur during your project are reported to the Committee.

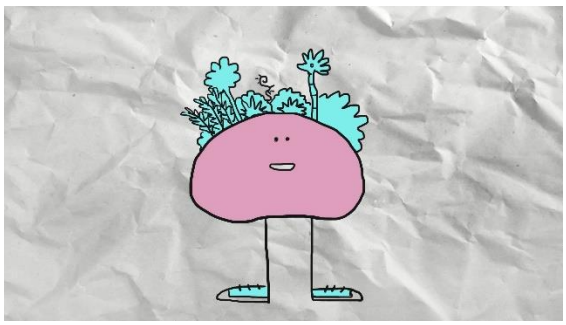
Approval by the FMH Research Committee should not be taken as evidence that your study is compliant with GDPR and the Data Protection Act 2018. If you need guidance on how to make your study GDPR compliant, please contact your institution's Data Protection Officer.

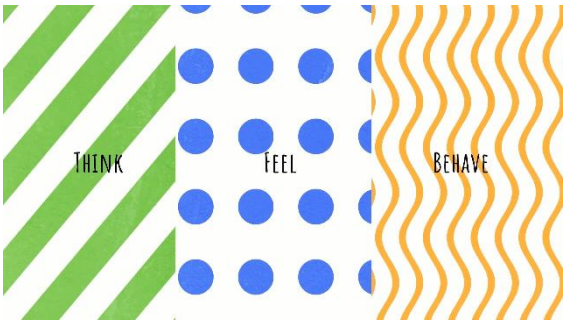
Please can you also arrange to send us a report once your project is completed.

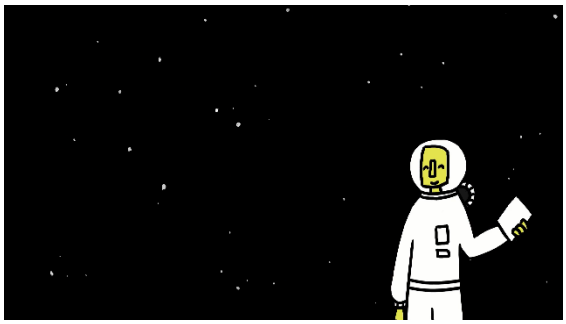
Yours sincerely,

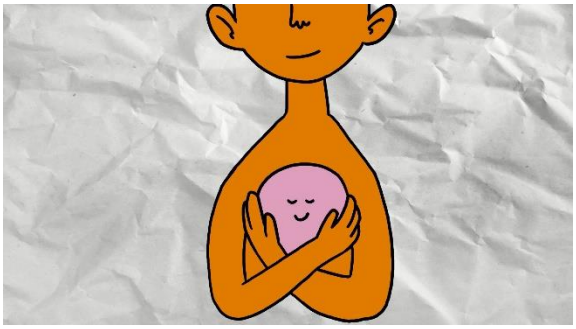
Professor M J Wilkinson
Chair
FMH Research Ethics Committee

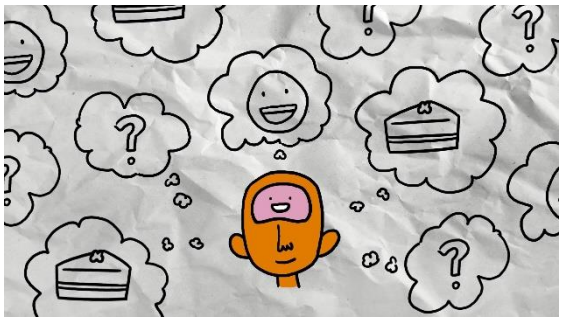
Appendix M: Screenshots of the Intervention developed for the Empirical Study

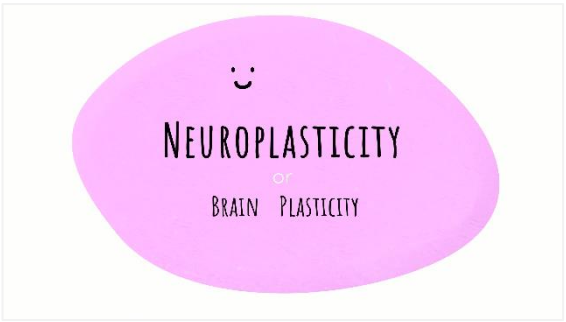


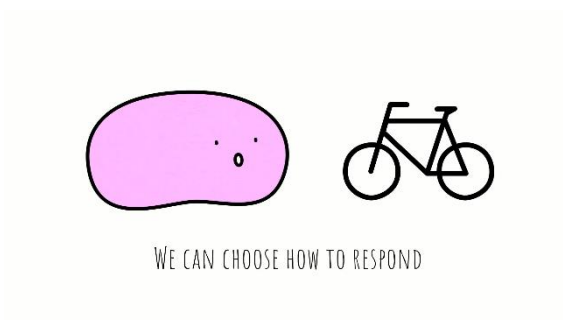


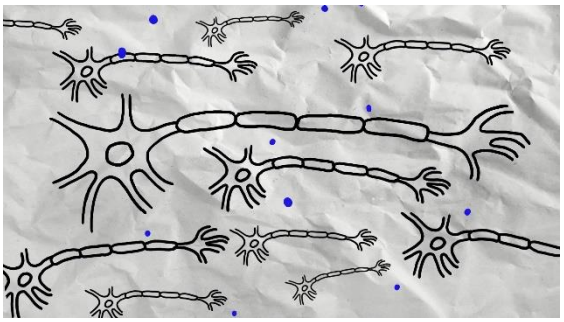












Full intervention can be viewed at: https://ueadldteam.typeform.com/to/T84uxV

Appendix N: The Implicit Personality Theory Questionnaire

1) You have a certain personality, and it is something that you can't do much about.

1	2	3	4	5	6
Really disagree					**Really agree**

2) Your personality is something about you that you can't change very much.

1	2	3	4	5	6
Really disagree					**Really agree**

3) Either you have a good personality or you don't, and there is really very little you can do about it.

1	2	3	4	5	6
Really disagree					**Really agree**

Appendix O: Acceptance and Fusion Questionnaire for Youth–Short Form

We want to know more about what you think, how you feel, and what you do. Read each sentence. Then, circle a number between 0-4 that tells how true each sentence is for you.

	Not at all True	A little True	Pretty True	True	Very True
1. My life won't be good until I feel happy.	0	1	2	3	4
2. My thoughts and feelings mess up my life.	0	1	2	3	4
3. The bad things I think about myself must be true.	0	1	2	3	4
4. If my heart beats fast, there must be something wrong with me.	0	1	2	3	4
5. I stop doing things that are important to me whenever I feel bad.	0	1	2	3	4
6. I do worse in school when I have thoughts that make me feel sad.	0	1	2	3	4
7. I am afraid of my feelings.	0	1	2	3	4
8. I can't be a good friend when I feel upset.	0	1	2	3	4

Appendix P: Self-Compassion Scale–Short Form

Please read each statement carefully before answering. To the left of each item, indicate how often you behave in the stated manner, using the following scale:

Almost never				Almost always
1	2	3	4	5

_____1. When I fail at something important to me I become consumed by feelings of inadequacy.

_____2. I try to be understanding and patient towards those aspects of my personality I don't like.

_____3. When something painful happens I try to take a balanced view of the situation.

_____4. When I'm feeling down, I tend to feel like most other people are probably happier than I am.

_____5. I try to see my failings as part of the human condition.

_____6. When I'm going through a very hard time, I give myself the caring and tenderness I need.

_____7. When something upsets me I try to keep my emotions in balance.

_____8. When I fail at something that's important to me, I tend to feel alone in my failure

_____9. When I'm feeling down I tend to obsess and fixate on everything that's wrong.

_____10. When I feel inadequate in some way, I try to remind myself that feelings of inadequacy are shared by most people.

_____11. I'm disapproving and judgmental about my own flaws and inadequacies.

_____12. I'm intolerant and impatient towards those aspects of my personality I don't like.

Appendix Q: Rosenberg Self-Esteem Scale

Below is a list of statements dealing with your general feelings about yourself. Please indicate how strongly you agree or disagree with each statement.

1. On the whole, I am satisfied with myself.

   Strongly Agree Agree Disagree Strongly Disagree

2. At times I think I am no good at all.

   Strongly Agree Agree Disagree Strongly Disagree

3. I feel that I have a number of good qualities.

   Strongly Agree Agree Disagree Strongly Disagree

4. I am able to do things as well as most other people.

   Strongly Agree Agree Disagree Strongly Disagree

5. I feel I do not have much to be proud of.

   Strongly Agree Agree Disagree Strongly Disagree

6. I certainly feel useless at times.

   Strongly Agree Agree Disagree Strongly Disagree

7. I feel that I'm a person of worth, at least on an equal plane with others.

   Strongly Agree Agree Disagree Strongly Disagree

8. I wish I could have more respect for myself.

   Strongly Agree Agree Disagree Strongly Disagree

9. All in all, I am inclined to feel that I am a failure.

   Strongly Agree Agree Disagree Strongly Disagree

10. I take a positive attitude toward myself.

    Strongly Agree Agree Disagree Strongly Disagree

Appendix R: Revised Children's Anxiety and Depression Scale–Short Version

Please put a circle around the word that shows how often each of these things happen to you. There are no right or wrong answers.

1. I feel sad or empty	Never	Sometimes	Often	Always
2. I worry when I think I have done poorly at something	Never	Sometimes	Often	Always
3. I would feel afraid of being on my own at home	Never	Sometimes	Often	Always
4. Nothing is much fun anymore	Never	Sometimes	Often	Always
5. I worry that something awful will happen to someone in my family	Never	Sometimes	Often	Always
6. I am afraid of being in crowded places (like shopping centers, the movies, buses, busy playgrounds)	Never	Sometimes	Often	Always
7. I worry what other people think of me	Never	Sometimes	Often	Always
8. I have trouble sleeping	Never	Sometimes	Often	Always
9. I feel scared if I have to sleep on my own	Never	Sometimes	Often	Always
10. I have problems with my appetite	Never	Sometimes	Often	Always
11. I suddenly become dizzy or faint when there is no reason for this	Never	Sometimes	Often	Always
12. I have to do some things over and over again (like washing my hands, cleaning or putting things in a certain order)	Never	Sometimes	Often	Always
13. I have no energy for things	Never	Sometimes	Often	Always
14. I suddenly start to tremble or shake when there is no reason for this	Never	Sometimes	Often	Always
15. I cannot think clearly	Never	Sometimes	Often	Always
16. I feel worthless	Never	Sometimes	Often	Always
17. I have to think of special thoughts (like numbers or words) to stop bad things from happening	Never	Sometimes	Often	Always
18. I think about death	Never	Sometimes	Often	Always
19. I feel like I don't want to move	Never	Sometimes	Often	Always
20. I worry that I will suddenly get a scared feeling when there is nothing to be afraid of	Never	Sometimes	Often	Always
21. I am tired a lot	Never	Sometimes	Often	Always
22. I feel afraid that I will make a fool of myself in front of people	Never	Sometimes	Often	Always
23. I have to do some things in just the right way to stop bad things from happening	Never	Sometimes	Often	Always
24. I feel restless	Never	Sometimes	Often	Always
25. I worry that something bad will happen to me	Never	Sometimes	Often	Always

Appendix S: Transient Psychological Mindset Measure (created for the Empirical Study)

1) The things our brains do make sense.

1	2	3	4	5	6
Really disagree					**Really agree**

2) You can completely control your thoughts, feelings and urges.

1	2	3	4	5	6
Really disagree					**Really agree**

3) Your thoughts, feelings and urges come and go.

1	2	3	4	5	6
Really disagree					**Really agree**

Scoring:

- Reverse items 1 and 3.
- Lower total scores are indicative of more adaptive mindsets.

Appendix T: Letter of Ethical Approval for the Empirical Study

Faculty of Medicine and Health Sciences Research Ethics Committee

Amorette Perkins
MED

Research & Innovation Services
Floor 1, The Registry
University of East Anglia
Norwich Research Park
Norwich, NR4 7TJ

Email: fmh.ethics@uea.ac.uk

Web: www.uea.ac.uk/researchandenterprise

07 January 2019

Dear Amorette

Project title: A Brief Psychological Mindset Intervention to Promote Mental Health in UK College Students: A Feasibility Study

Reference: 201819 - 045

Thank you for your response to the recommendations from the FMH Ethics Committee to your proposal. I have considered your amendments and can now confirm that your proposal has been approved.

Please can you ensure that any further amendments to either the protocol or documents submitted are notified to us in advance, and also that any adverse events which occur during your project are reported to the Committee.

Approval by the FMH Research Committee should not be taken as evidence that your study is compliant with GDPR and the Data Protection Act 2018. If you need guidance on how to make your study GDPR compliant, please contact your institution's Data Protection Officer.

Please can you also arrange to send us a report once your project is completed.

Yours sincerely,

Professor M J Wilkinson
Chair
FMH Research Ethics Committee

Appendix U: Brief Minutes from the Educational Staff/Student Workgroup

Research design:

- Attendance is an important outcome for schools.
- There is anecdotal evidence that mindsets contribute to truancy.

Intervention feedback:

- Some words are too big. You need to explain neuroplasticity more.
- There is too much science.
- Evolution fits with the curriculum (it doesn't need explaining in detail).
- Add more visuals and less words.
- Make sure there are lots of visuals to illustrate the points.
- Put in tips for coping with the brain once we understand how it works (in the stories by young people).
- Use examples that people can easily relate to.
- Emphasise that we can't change the past but we can change the future.
- The house fire and toast/garden examples are good.
- This intervention helps understand repetitive thoughts and feelings.
- School children aren't usually taught how to understand the brain. This intervention should be given to children of all ages.
- The intervention could help reduce stigma about mental health.