Information Processing and Family-Based Interventions in Eating Disorders

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Overview

The following thesis portfolio aims to review both the theoretical underpinnings of eating disorders, and the clinical impact of the treatment for the conditions. Specifically, it seeks to examine information processing in binge eating disorder (BED) in relation to the cognitive model of eating disorders, and the development in treatments for children and adolescents with anorexia nervosa and bulimia nervosa. The thesis portfolio comprises five chapters. Chapter one is a meta-analysis of literature that has examined attentional biases in BED.

Chapter two is a bridging chapter, which discusses the theoretical underpinnings from chapter one and how this relates to chapter three. Chapter three is a systematic review and meta-analysis which reviews the existing literature on the current first-line treatment of eating disorders for children and adolescents and compares this with the developing evidence-base for multifamily therapy.

Chapter four provides a detailed explanation of the methodologies chosen for both meta-analyses, and the results of additional analyses that were not included in the papers. Finally, Chapter five comprises an overall discussion and evaluation of the thesis, including a reflective statement.

Acknowledgements

I would like to thank my research supervisors, Professor Siân Coker and Professor Kenneth Laidlaw, for their guidance and support with the development of this thesis. I would also like to thank Dr Penny Hazell for her encouragement and supervision, particularly when I was on placement with her, as this helped me to understand the clinical relevance of the thesis topic.

I am grateful to my parents and sister, who have always encouraged me to aim high, and who have always believed that I could achieve anything I wanted to. I am also grateful to my wider family and friends, who have cheered me on.

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This thesis is dedicated to Tubbs, who kept me company and napped by my side as I wrote the thesis.

Thesis Portfolio Abstract

Aims: Two meta-analyses of under-researched areas within the eating disorder field are presented. The first explored whether people with binge eating disorder (BED) exhibit greater deficits in attentional bias (AB) when attending to food stimuli compared to controls. The second explored whether eating disorder-focused family therapy (ED-FT) for young people with anorexia nervosa and bulimia nervosa is effective when compared to other forms of family therapy (FT).

Methods: The first meta-analysis produced 13 studies, which were analysed using a Hedges's *g* random effects model. The second meta-analysis identified 15 papers for ED-FT and seven for multifamily therapy (MFT). The analysis of ED-FT compared with other forms of FT was conducted using an Odds ratio random effects model; the analysis of changes in weight for MFT was conducted using a Hedges's *g* random effects model.

Results: The first meta-analysis yielded a pooled Hedges's *g* effect size of -.12 (95% CI: -.36, .12), indicating no statistically significant difference between groups (p = .339). The second meta-analysis found a non-significant pooled Odds ratio effect of .64 (95% CI: .36, 1.12) indicating that ED-FT was not as efficacious when compared with other forms of FT. The effect of MFT on changes in weight yielded a pooled medium Hedges's *g* effect of .64 (95% CI: .23, 1.05), indicating that MFT may be a promising alternative treatment to ED-FT.

Conclusions: The results of the first meta-analysis do not support the theory that ABs are greater in BED. Future research is needed to produce higher quality research which tests other components of ABs in BED. The second meta-analysis highlights the need for research into the efficacy of other forms of FT or modifications to ED-FT. Patients receiving MFT appear to gain weight; however, the

lack of comparison groups limits the ability to draw firm conclusions.

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Chapter One. Meta-Analysis of Attentional Biases in Binge Eating Disorder

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What is the Impact of Binge Eating Disorders on Information Processing, in the Attentional Domain? An Exploratory Meta-Analysis

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Abstract

Objective: The aim was to test the applicability of the cognitive model of EDs to binge eating disorder (BED) by reviewing the literature that has investigated whether BED has greater attentional biases (ABs) towards food cues compared to controls, using experimental paradigms.

Methods: Literature searches were conducted on PubMed, PsycINFO, EMBASE, CINAHL, and Medline. Search terms related to binge eating, attention, and paradigms such as the Stroop task, dot-probe, spatial cueing task and visual search task were used. Inclusion criteria included experimental designs that had recruited participants across the lifespan, either with BED or subthreshold BED, and a control group. Hedges's *g* effect sizes were calculated for studies that provided means and standard deviations of reaction times. An attentional bias score was also calculated from the difference between means. This was used to compute the Hedges's *g* effects. A quality assessment was conducted on all papers that met inclusion criteria.

Results: 13 papers were identified that met inclusion criteria, totalling 656 participants. When comparing the difference between AB scores for the BED and control group, the random effects pooled Hedges's *g* yielded an effect size of -.12 (95% CI: -.36, .12), indicating no effect. This demonstrates that there is no difference between how people with BED respond to cues compared with controls.

Discussion: There are inconsistencies in methodological approaches taken by the current literature when testing ABs in BED. The non-effect also tentatively questions the applicability of either the cognitive model to BED, or whether the use of the current paradigms are an appropriate test of ABs in BED.

Key words: Binge eating disorder, attentional bias, experimental paradigms, cognitive theory of eating disorder

Introduction

Binge eating (BE) is characterised by consuming a large quantity of food within a discrete period of time (American Psychiatric Association, 2013). This act is accompanied by feeling out of control and distressed (American Psychiatric Association, 2013). BE is the primary symptom of bulimia nervosa (BN) and is also present in binge-purge subtypes of anorexia nervosa (AN).

Binge eating disorder (BED) is the most common eating disorder (ED) diagnosis (Hutson, Balodis & Potenza, 2018) and has been recognised as a distinct type of ED since the release of the Diagnostic and Statistical Manual of Mental Disorders (DSM-5) in 2013 (American Psychiatric Association, 2013). In order to meet diagnostic criteria, the above symptoms must be present at least once a week for three months, without the presence of compensatory behaviours, such as vomiting or laxative use. Other forms of subthreshold BED are prevalent, such as loss of control (LOC) eating (Stojek et al., 2018), but much less is known about such disorders. BED and subthreshold BED have a lifetime prevalence of roughly 1.9% (Kessler et al., 2013), with studies indicating a lower age of onset than previously thought, at 12.6 years (Kittel, Schmidt, & Hilbert, 2017).

Cognitive Theories of ED

While a number of psychological models exist for AN and BN, there is no widely accepted model of BED (Burton & Abbott, 2017). Instead, a number of theories of AN and BN have been applied to BED. However, this is not satisfactory, as none seems to adequately explain the mechanisms maintaining the disorder. For example, the dual pathway model (Stice & Agras, 1998) does not account for the cognitive component of EDs, while the cognitive model (Fairburn, 1981) does not

place emphasis on the impact of negative mood states on BE. Furthermore, theories such as the escape theory (Heatherton & Baumeister, 1991) and the transdiagnostic theory of EDs (Fairburn, Cooper & Shafran, 2003) have not been empirically tested using a BED population.

The cognitive model of EDs is perhaps the most widely applied theory to BED (Fairburn, 1981). This model proposes that individuals with EDs exhibit impaired information processing due to habituations towards maladaptive food-related schemas. This is thought to influence confirmatory and attentional biases (AB) towards food- and body-related cues. For example, for those with a diagnosis of BN, ABs are thought to develop towards food cues, disrupting cognitive processes, as attention towards other cues is less likely to be maintained when food stimuli are present in the environment (Brooks, Prince, Stahl, Campbell, & Treasure, 2011). This is thought to be driven by an over-evaluation of body weight and shape, leading to extreme dieting behaviour to improve self-image (Fairburn, 1981). Any deviance from the dietary rules is perceived by the individual as a 'failure', leading them to BE and compensate by vomiting or laxative. The role of dietary restraint is poorly understood in BED, and as such, warrants further investigation.

Components of Attention

AB refers to the way in which attention to a particular stimulus is automatically allocated. AB can occur via 'facilitated attentional engagement', which refers to how fast attention is drawn to salient stimuli (such as food) compared to non-salient stimuli (Stojek et al., 2018). AB can also occur via 'attentional disengagement', which refers to the degree to which salient stimuli capture attention, making it difficult to attend to other cues. Attentional disengagement is thought to be harder to achieve than attentional engagement (Stojek et al., 2018).

A wealth of research, including meta-analyses, has examined AB towards ED salient stimuli, in AN and BN (Brooks, et al., 2011; Dobson & Dozois, 2004; Johansson, Ghaderi, & Andersson, 2005; Werthmann, Jansen, & Roefs, 2015). These studies have consistently shown that people with a diagnosis of AN or BN exhibit greater ABs compared with controls. For example, Brooks et al. (2011) meta-analysed 16 Stroop studies, which yielded a pooled small effect of .34 (95% CI: .20, .48). However, this study did not include a BED group, and did not review the quality of the research; it is therefore unclear how the results relate to a BED population, and how much significance the conclusions can be given.

Previous meta-analyses have not included research investigating BED. The literature has included the diagnosis of 'eating disorder not otherwise specified'(EDNOS), which is a subtype of ED that previously encompassed BED (American Psychiatric Association, 2013). It is highly likely that these studies inadvertently included participants who would now meet criteria for BED in the EDNOS group; however, the results are not reliable as this diagnosis would have also included participants who displayed subthreshold AN and BN. It is therefore currently unclear whether the cognitive model of ED applies to BED.

Experimental Paradigms Examining Attention

The existing research has used a number of experimental paradigms to investigate ABs in EDs, including BED. The most commonly used paradigm is the Stroop task (Stroop, 1935). This measures AB by asking participants to name the colour of written words categorised as either salient or neutral. With this task, an AB towards salient stimuli is inferred from slower RTs, as this is thought to signify cognitive interference. This paradigm has a number of limitations and is considered a

poor test for AB (Starzomska, 2017; Cisler, Bacon, & Williams, 2009). Specifically, the mechanisms that underlie how people attend to the congruent and incongruent words are still poorly understood, making it difficult to ascertain whether the Stroop interference is indicative of AB. This is a limitation of the existing literature on ABs in AN and BN, as these studies have predominantly drawn conclusions using this paradigm (Brooks et al., 2011; Stojeck et al., 2018).

The visual probe task (MacLeod, Mathews, & Tata, 1986) was developed as a more accurate measure of AB (Starzomska, 2017). This task asks participants to respond to a probe that is presented on either the left- or right-hand side of the computer screen. Faster RTs to valid probes that have replaced a salient stimulus indicate facilitated attentional engagement to salient stimuli. (Stokek et al., 2018).

The spatial cueing task (Posner, 1980; Cisler et al., 2009) measures facilitated attentional engagement by recording the time it takes participants to press a key which corresponds to the location of a probe, following presentation of a salient or neutral cue. Facilitated attentional engagement is inferred from faster RTs on valid salient cues.

Finally, visual search tasks (Cisler et al., 2009) ask participants to detect a word that is the 'odd one out' from a matrix of either neutral or salient words. Facilitated attentional engagement is inferred from faster RTs to detect salient target words.

While these paradigms are frequently used within the literature to measure ABs in EDs, it is unclear whether they are the most appropriate experimental tool. The literature is clear that the Stroop is a poor test, due to it not being clear exactly which component of AB it measures (Starzomska, 2017; Eysenck, 1992). Furthermore, there appear to be significant inconsistencies in the methodological

approaches taken when using these experimental paradigms (Starzomska, 2017; Stojek et al., 2018). It is also unclear how easily the outcomes from these experimental studies can be applied to the clinical application of EDs beyond AN and BN samples, as this has not been statistically analysed to date (Stojek et al., 2018).

Current Literature

Previous research has examined the relevance of the cognitive model of EDs in BED, by using the aforementioned paradigms to investigate possible ABs towards food and body cues in BED; however, a meta-analytic review has not yet been completed.

A recent systematic review (Stojek et al., 2018) summarised the studies that have investigated AB in EDs with a BE component, including BED. The review found that individuals who BE demonstrate increased AB towards food cues compared with controls. However, the review was limited as two databases (PubMed, PsychInfo) were used to conduct the search, which may have reduced the number of articles found. The review lacked clarity on what data were used to calculate the effect sizes for the studies, making it difficult to replicate. Lastly, the review failed to conduct a quality assessment, reducing confidence with which conclusions can be drawn.

Rationale for Current Meta-Analysis

While BED is often conceptualised within a cognitive framework, to date, a meta-analysis on AB in BED has not been completed. This review offers an investigation into whether ABs are present in people across the lifespan who exhibit BE without compensatory behaviours. Due to the variation in paradigms used to

measure AB in BED (Stojek et al., 2018), the focus of the current MA will be on identifying more broadly whether ABs are present in BED across paradigms. The paucity in the literature means that this paper will review BED across the lifespan and consider a possible relationship between ABs towards food cues and BED or subthreshold BED, such as LOC. To the author's knowledge, this will be the first meta-analytic review of AB in BED.

Research Questions

The study population, intervention, comparison, outcome and study type (PICOS) tool was used to guide the research questions (Centre for Reviews and Dissemination Guidelines, 2009). In this case, the population is anyone who has a diagnosis of BED or subthreshold BED without compensatory behaviours; the intervention is the paradigm used to measure ABs; the comparison group is a non-BED population who do not have an ED diagnosis; the outcome is the mean and standard deviation (SD) reaction time (RT); and the study type is an experimental case-control design. In line with these criteria, the questions this meta-analysis hopes to answer are:

1. When completing an experimental paradigm, do people with a diagnosis of BED or subthreshold BED exhibit greater AB towards food stimuli compared with controls?
2. Is there a difference between BED and control groups for AB scores?
3. When conducting subgroup analysis, which paradigm(s) contribute most towards the overall effect?
4. Is there a difference between how clinical BED and subthreshold BE samples respond to salient stimuli?

Methods

Search

The Cochrane Library was searched for existing similar reviews. The review protocol (CRD42018089216) was registered on the international prospective register of systematic reviews (PROSPERO). The methodology of the meta-analysis was guided by the preferred reporting items for systematic reviews and meta-analyses (PRISMA; Liberati et al, 2009). The healthcare database advanced search (HDAS) was used to systematically search the following electronic databases for literature: PsycINFO, EMBASE, Medline, CINAHL and Pubmed. Hand-searching of journals was conducted by using search engines such as Google Scholar and hand-searching reference lists. The grey literature was also searched by searching papers that were in press from journals that have published articles in the field, and by searching the electronic thesis online service database.

The following search terms were chosen to identify relevant papers: "(binge eating disorder OR binge eating)" AND "(information processing OR cognitive bias OR attention OR attentional bias OR Stroop task OR Stroop colour word interference OR colour-word interference OR verbal interference OR spatial cueing task OR visual probe task OR dot probe task OR dot-probe OR visual search task)".

Study Selection

Due to the limited research in this field, a date restriction was not placed on the search. An age limit was also not placed, to maximise the number of studies that could be included. Studies were included if they used an experimental case-control design investigating AB towards food stimuli in participants with BED, or subthreshold BED, such as LOC eating.

Studies using the following AB paradigms were included: the Stroop task; dot-probe task; spatial cueing task; visual search task. Variations of these tasks, such as the colour-word interference task, were also included. These paradigms were chosen as they are most commonly used to investigate AB in EDs (Aspen, Darcy & Lock, 2013).

Studies were excluded if they were not in English or published in peer-reviewed journals. They were excluded if they recruited solely participants with AN or BN, or they only included an obese population who did not have BED or subthreshold BED.

Study Identification

The primary author (RJ) independently conducted the literature search and study identification in January 2018. Figure 1 highlights the process of study identification, in line with PRISMA guidelines (Liberati et al, 2009). The search produced 1433 papers, of which 807 remained following the removal of duplicates. The study title and abstracts from the search results were screened, with studies not meeting the inclusion criteria excluded. The full articles of the remaining 35 papers were read, resulting in the identification of 13 papers for entry into the MA review. Three full papers were unavailable as they were conference abstracts and have not been published as journals. Therefore, these papers were not included in the final inclusion list as they did not provide the appropriate data.

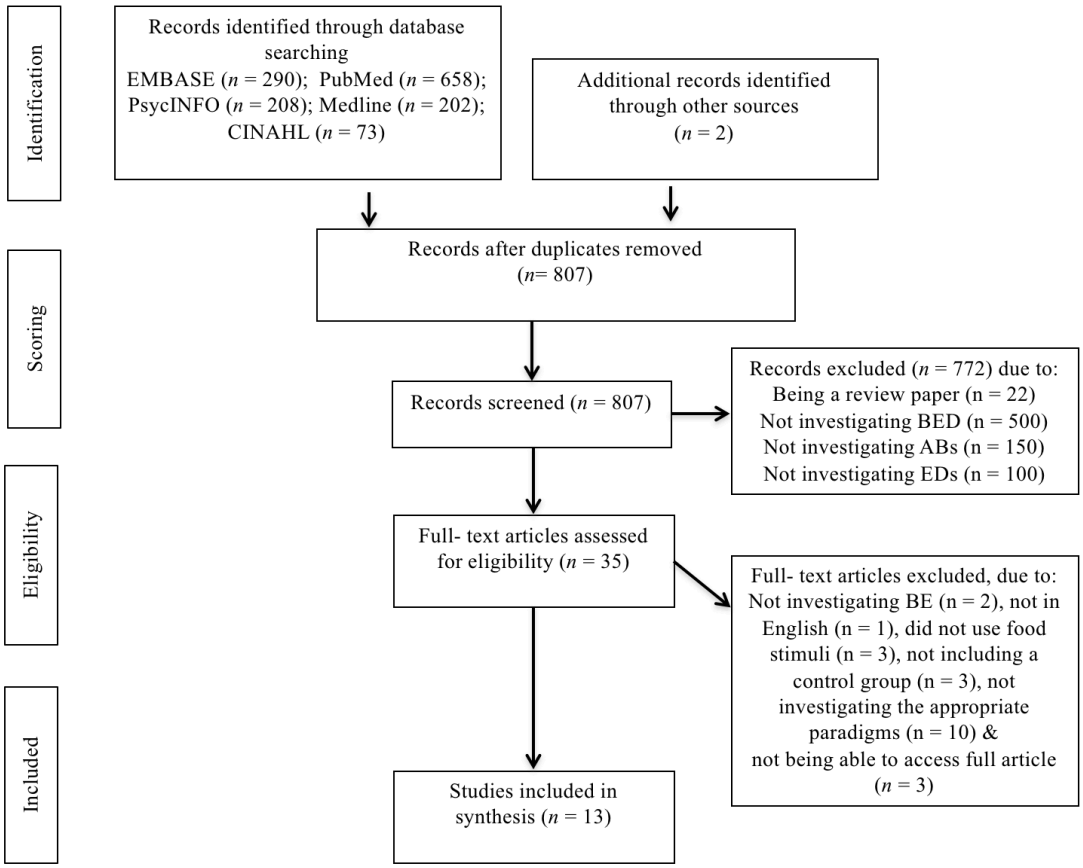

Figure 1. PRISMA diagram detailing the study selection process

Data Coding and Extraction

The experimental procedure, measure of AB, stimulus cue, and participant population were coded. Only one effect size was extracted from each paper. Therefore, if a paper provided information pertaining to more than one effect size, criteria for each code were developed, based on the inclusion criteria and previous literature (Pool, Brosch, Delplanque & Sander, 2016; Cuijpers, 2016).

The first moderator was the paradigms used to test AB in BEDs. This included: the Stroop task; dot-probe task; spatial cueing task; visual search task. Each paper utilised one of these paradigms.

The type of measure used to assess AB was the second moderator. This included RT, response accuracy, number of errors, and completion time. These are

considered 'indirect' measures, as they rely on a behavioural response, rather than 'direct' measures, such as eye movements (Pool et al., 2016). Where more than one measure of AB was provided, the RT was documented above the other measures. This is because it is the most widely used behavioural measure of AB when using these paradigms (Aspen et al., 2013).

The stimuli-type used were coded. The focus of this review was on the use of disorder-salient cues, such as food cues. The raw scores were documented for response to salient, and neutral stimuli. In order to be as consistent as possible in comparing measures of ABs across paradigms, data from stimuli that were designed to trigger facilitated attentional engagement were used. Therefore, data from salient (i.e. food) or salient-valid cues were used (Stojek et al., 2018).

Lastly, the clinical group was coded to include BE, BED, LOC for the experimental groups. If a BED group was not used, preference was given to a subthreshold BED group, followed by LOC. From here on in, the participants in the clinical group will be referred to as the 'BED group'. For the control group, the type of group was coded as healthy control, overweight control, weight-matched control or control group. Where more than two control groups were used, preference was given to weight-matched or overweight controls, followed by healthy controls.

The aforementioned information was extracted from the studies and added to an electronic database that was developed by the primary author. Where the data were unclear or unavailable, the papers' authors were contacted with requests for this information. Although a number of researchers responded (n = 4), a large proportion did not or were unable to provide the required information. This led to an inability to gather enough data to meta-analyse each paradigm separately.

Quality Assessment

Conducting a quality assessment or using a risk of bias tool is an important aspect of conducting meta-analyses, as it highlights studies that have utilised poor methodological designs when reporting their research (Cuijpers, 2016). The majority of tools available are designed to assess randomised controlled trials (RCTs) of treatment trials, such as the Cochrane risk of bias tool (Higgins & Green, 2011) and the RCT of psychotherapy quality rating scale (RCT-PQRS; Kocsis et al., 2010). A limited number of tools have been designed for non-treatment cohort studies using an experimental design.

As the RCT-PQRS has been shown to have good internal reliability (Gerber et al., 2011) and external validity (Kocsis et al., 2010), it was adapted for this study (see Appendix B). It also integrated key questions from the quality assessment tool for observational cohort and cross-section studies (National Heart Lung and Blood Institute, 2014) and Cochrane risk of bias tool (Higgins & Green, 2011). The inclusion and exclusion criteria were used to guide the development of the tool (Cuijpers, 2016). The adapted tool consisted of 18 questions, each requiring a rating of zero to two. The sum of the ratings referred to one of seven quality ratings, ranging from ‘exceptionally poor’ to ‘exceptionally good’.

The primary author (RJ) developed the adapted tool with the secondary and last authors. The tool was refined following supervision. RJ then conducted a pilot of the adapted quality tool with a member of the research team. They randomly selected and rated two papers and found very little discrepancy between their ratings for each item. The tool was then refined, particularly the wording of two items. RJ then independently scored each article using the refined tool. Finally, the secondary author independently scored 20% of the randomly selected studies. Any

disagreements were discussed until an agreement was reached. Cohen's kappa coefficient (Landis & Koch, 1977) from the three papers produced a moderate agreement result of 0.52, with a percentage agreement of 68.8% (Landis & Koch, 1977).

Analysis

Comprehensive meta-analysis (CMA) software (version 3; Borenstein, Hedges, Higgins & Rothstein, 2005) was used to run the analysis. The RT to food and non-food stimuli and SD data that were extracted from the papers were used to calculate the effect size and 95% confidence intervals for each study, to standardise the findings (Brooks et al., 2011).

Two effect sizes were computed from these figures: the first effect size was of the standard mean difference between the BED and control group for RT scores towards salient stimuli only. It is important to standardise the RT between salient and neutral cues, in order to increase the validity of the results (Brooks et al., 2011). Therefore, a second effect size was computed by calculating an 'AB score'. This was calculated by subtracting the mean RT from the neutral stimuli scores from the salient stimuli scores (Brooks et al., 2011). The pooled standard deviation was calculated (Cohen, 1988).

This was used to compute the individual and pooled Hedges's *g* effect sizes and 95% confidence intervals, using a random-effects model. A negative effect signifies a greater AB score towards the BED group, and a positive effect signifies a greater AB score towards the control group. See Figure 2 for an illustration of the effect sizes that can be calculated from the data. The orange arrows represent the effect size that can be calculated from the RT scores between the BED and control

group for either food or non-food stimuli only. The purple arrows depict the within-groups effect size, such as the standard mean difference between RT scores for food compared to non- food stimuli for the BED group only. Finally, the green arrow represents the difference between the BED and control group for the AB scores, taking in to account the how each group respond to both food and non-food stimuli. Table 1-4 also report the Hedges's g effect size for the AB scores.

Hedges's *g* was chosen instead of Cohen's *d* as the former takes account of aspects of sample heterogeneity and adjusts for possible biases, such as small sample sizes (Cuijpers, 2016). The Hedges's *g* effect size were based on Cohen (1988), who advises considering $d \leq .2$ as small; $d = .50$ as medium; $d \geq .80$ as large. For the analysis of BED group and control group for food stimuli, a negative effect size indicates that the BED group experienced greater AB towards food stimuli.

A random effects model was used to calculate the pooled effect size and 95% confidence intervals. This model was used as opposed to a fixed effects model, as a random effects model assumes that the observed effects are distributed randomly across populations (Borenstein, Hedges, Higgins & Rothstein, 2009).

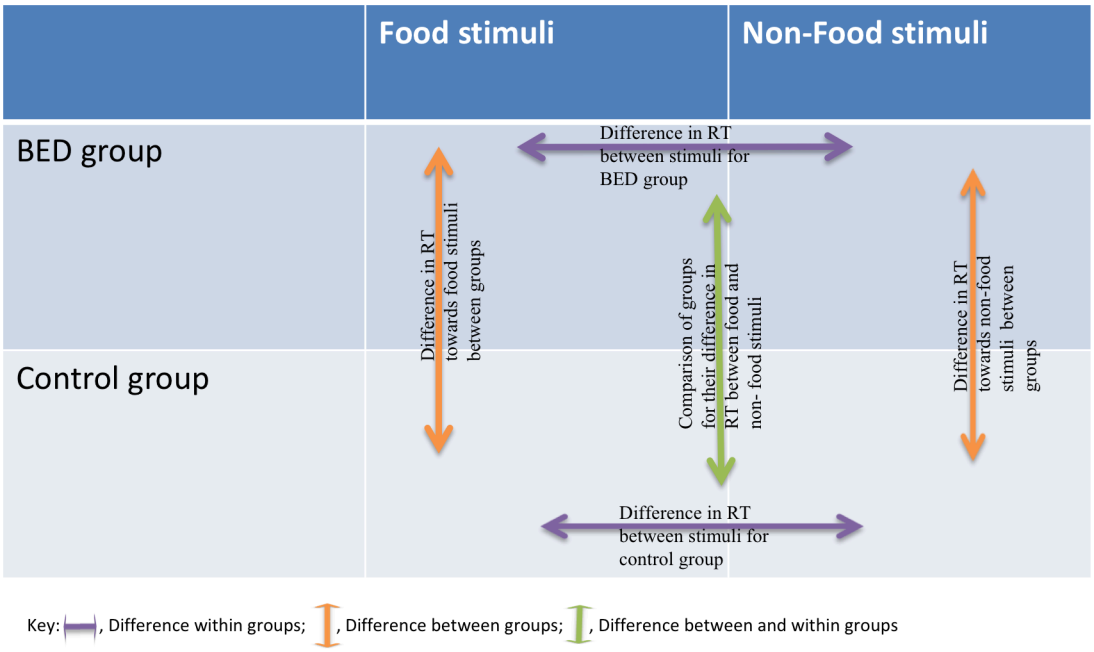

Figure 2. Illustration of different measures used to calculate Hedges's *g* effect size.

A test for heterogeneity was conducted by calculating Cochran's Q-statistic, which is the weighted squared deviation, which provides an estimation of variation (Borenstein et al., 2009). The I-squared statistic (I^2) was also utilised as this provides a percentage that indicates the extent of the variability in the distribution of effect sizes that may be due to heterogeneity, rather than chance (Borenstein et al., 2009). The I^2 ranges from 0 to 100%, with an I^2 of 25% denoting small heterogeneity, 50% medium, and 75% large (Borenstein et al., 2009).

Publication bias across studies was analysed by constructing funnel plots (Begg & Mazumdar, 1994). The 'trim and fill' method (Duval & Tweedie, 2000) was used to calculate the number of potential missing studies from the analysis. Rosenthal's Fail-Safe N (Rosenthal, 1979) was also calculated as this indicates the number of studies that would be needed in order for the *p*-value to become non-significant.

Additional analyses were conducted, including conducting a sensitivity analysis and subgroup analysis. The sensitivity analysis was conducted in order to assess how robust the method of analysis was (Borenstein et al., 2009). Subgroup analyses were also conducted in order to determine whether the effect sizes differed based on particular subgroups (based on age, diagnosis and paradigm type).

Results

Study Characteristics

Tables 1- 4 summarise the shortlisted studies (n = 13) that met inclusion criteria. Of these studies, only five provided the appropriate data to meta-analyse.

The decision was made to include all 13 studies in the initial synthesis, to highlight the high degree of heterogeneity between studies that have attempted to investigate AB in BED. For example, there are large discrepancies in the reporting of results between all of the studies, and variation in the paradigms used to measure AB.

The 13 studies consisted of cross-sectional experimental designs, using a range of paradigms to test ABs. There was an even use of clinical and subthreshold participants, as studies recruited participants with either BED (n = 172), or subthreshold BE (n = 83), and LOC (n = 47). All experimental conditions were compared with a control group, which consisted of healthy controls (n = 14), weight-matched controls (n = 124) and an overweight group (n = 216).

In order to assess heterogeneity across studies, the variations in gender, body mass index (BMI) and age were extracted. With the exception of Duchesne et al. (2010), all of the studies recruited predominantly females. Deluchi, Costa, Friedman, Goncalves, and Bizarro (2017) did not report the gender ratio for each experiment group but stated that 90.9% of the total participants (n = 44) were female. Three studies (Kittel et al., 2017; Shank et al., 2015; Schmidt, Luthold, Kittel, Tetzlaff, & Hilbert, 2016) recruited adolescents with the remainder recruiting adult participants. Of the studies that provided mean and SD BMI, every study apart from Lyu, Zheng, and Jackson (2016) recruited a BE or BED sample that would be categorised within the obese range (BMI > 30).

The Stroop task. Variations in the type of Stroop tasks utilised by the studies are evident: of the seven studies, five used computerised tasks (Lavender et al., 2014; Galioto et al., 2012; Manasse et al., 2015; Lee, Namkoong, & Jung, 2017; Balodis et al., 2013), while two used a paper-and-pen version (Kittel et al., 2017; Duchesne et al., 2010). Six studies used word-based cues (Galioto et al., 2012;

Duchesne et al., 2010; Lavender et al., 2014; Manasse et al., 2015; Balodis et al., 2013; Kittel et al., 2017), and one used image-based tasks (Lee et al., 2017). Large variation can be seen in the type of measures used to record AB (see Table 1).

The dot-probe task. Two studies examined AB in BEDs using the visual probe task (Deluchi et al., 2017; Shank et al., 2015). Both of these papers recruited a subthreshold sample, and they both used food and neutral images. These studies also differed in their methodology as Shank et al. (2015) presented the stimuli for 2000ms, whereas Deluchi et al. (2017) presented the stimuli for 100, 500 and 2000ms. Both studies used RT as their measure of AB; however, Deluchi et al. (2017) did not report the raw scores.

The spatial cueing task. Two studies used the spatial cueing task to assess AB in BED (Schmitz, Naumann, Trentowska, & Svaldi, 2014; Lyu et al., 2016). They both utilised food-based cues, such as high calorie food images, and household items for the neutral cues. The studies differed in the stimulus duration used, as Lyu et al. (2016) showed the images for 300ms, while Schmitz et al. (2014) showed the images for 100ms. They also differed in the sample-type as Schmitz et al. (2014) recruited a clinical group of participants with a diagnosis of BED, while Lyu et al. (2016) recruited a non-clinical population, with subthreshold BE behaviours. Both studies used and reported RT as their measure of AB; however, Lyu et al. (2016) reported the overall mean RT for each group, as opposed to each group's responses to salient and neutral cues (see Table 3).

The visual search task. Finally, two studies reported their findings using the visual search task (Schmidt et al., 2016; Sperling, Baldofski, Luthold, & Hilbert, 2017). Both of these studies used food images for the salient cues and non-food images for the neutral cues. While both studies recruited participants with BED,

Schmidt et al. (2016) recruited adolescents, and Sperling et al. (2017) recruited adults. Both of these studies used and reported the mean RT for salient and neutral stimuli for each group (see Table 4).

Study Quality

The adapted tool highlighted a range in quality ratings, from moderately good to moderately poor. However, the majority of the studies were rated as moderately good - average. All of the studies provided a sufficient description of the paradigm used, however the studies varied in the reporting of raw scores. Furthermore, the studies all clearly stated how they recruited their control samples.

Table 1. Demographic and effect size comparison of Stroop paradigms measuring AB to food

Study	Sample	Mean age (SD)	Mean BMI (SD)	Paradigm	Measure of AB	Mean total errors (SD)	Mean overall completion time (SD)	Mean RT (SD) food	Mean RT (SD) non-food	Mean number of correct answers (SD)	Diff (SD)	Hedges's *g* (95% CI)	Study quality rating (raw score)
Duchesne et al, 2010	38 BED, 38.2% female	33.29 (5.01)	35.89 (2.91)	Stroop task	Completion time (ms)	-	122.95 (2.74)	-	-	-		-	Moderately good (22)
	38 CG, 44.7% female	35.42 (7.88)	36.60 (3.75)			-	120.63 (7.94)	-	-	-			
Lee et al, 2017	13 BED, 100% female	23.6 (2.6)	35.6 (3.8)	Stroop match-to-sample task	RT (ms)	-	-	559.35 (55.5)	565.99 (76.3)	-	-6.64 (66.59)	-.09 (-.83, .64)	Moderately good (18)
	14 CG*, 100% female	23.3 (2.2)	20.4 (2.6)			-	-	549.84 (73.1)	549.57 (81.0)	-	0.27 (77.15)		
Manasse et al, 2015	31 BE, 100% female	45.06 (14.86)	36.84 (7.97)	Colour-word interference	Total errors	3.36 (4.38)	-	-	-	-		-	Average (15)
	43 CG** 100% female	51.09 (8.26)	37.85 (6.27)			3.34 (3.46)	-	-	-	-			
Kittel, et al, 2017	22 BED, 81.8% female	14.91 (2.22)	N. R	Colour-word interference	Completion time (ms)	-	92.55 (17.58)	-	-	-		-	Moderately good (21)

	22 CG** 81.8% female	14.82 (2.63)	N. R			-	91.17 (25.09)	-	-	-		
Lavender et al, 2014	20 BED, 85.4% female	43.04 (11.01)	47.53 (5.52)	Verbal interference	Correct answers	-	-	-	-	52.72 (9.73)	-	Moderately good (17)
	48 CG** 100% female	42.65 (10.34)	45.65 (4.84)			-	-	-	-	51.49 (10.76)		
Balodis et al, 2013	11 BED, 81.8% female	47.6 (12.7)	37.1 (3.9)	Colour-word interference	RT (ms)	-	-	-	-	-	-	Average (14)
	13 CG** 38.5% female	35.4 (9.3)	34.6 (4.1)			-	-	-	-	-		
Galioto et al, 2012	41 BED, 96.3% female	43.58 (11.45)	45.4 (6.12)	Verbal interference	Correct answers	-	-	-	-	-	-	Average (17)
	90 CG** 83.1% female	41.18 (10.4)	44.87 (6.58)			-	-	-	-	-		

BED, binge eating disorder; BE, binge eating; CG, control group; AB, attentional bias; RT, reaction time; ms, milliseconds, SD, standard deviation; CI, confidence interval

*Healthy controls **Overweight group. *Note*. - signifies that the study did not provide this information

Table 2. Demographic and effect size comparison of dot-probe paradigms measuring AB to food

Study	Sample	Mean age (SD)	Mean BMI (SD)	Paradigm	Measure of AB	Overall mean RT (SD)	Mean RT (SD) food	Mean RT (SD) non-food	Diff (SD)	Hedges's *g* (95% CI)	Study quality rating (raw score)
Shank et al, 2015	47 LOC, 85.1% female	13.8 (2.4)	N. R	Visual probe task	RT (ms)	-	577.51 (75.93)	584.65 (84.89)	-7.14 (80.53)	-.06 (-.52, .40)	Average (15)
	29 CG, 89.7% female	15.6 (1.6)	N. R			-	562.81 (64.33)	565.61 (65.69)	-2.8 (65.01)		
Deluchi et al, 2017	19 BE, %female N.R	45 (9.62)	47.85 (N. R)	Visual probe task	RT (ms)	-	-	-		-	Moderately poor (12)
	23 CG %female N.R	49.5 (12.92)	48.24 (N. R)			-	-	-			

LOC, loss of control eating; BE, binge eating; CG, control group; AB, attentional bias; RT, reaction time; ms, milliseconds, SD, standard deviation; CI, confidence interval

Note. - signifies that the study did not provide this information

Table 3. Demographic and effect size comparison of spatial cueing paradigms measuring AB to food

Study	Sample	Mean age (SD)	Mean BMI (SD)	Paradigm	Measure of AB	Overall mean RT (SD)	Mean RT (SD) food	Mean RT (SD) non-food	Diff (SD)	Hedges's *g* (95% CI)	Study quality rating (raw score)
Schmitz et al, 2014	27 BED, 100% female	46.04 (14.54)	34.71 (5.14)	Spatial cueing task	RT (ms)	-	560.74 (77.38)	571.07 (82.31)	-10.33 (79.88)	-.12 (-.62, .38)	Average (17)
	33 CG, 100% female	42.42 (13.47)	32.41 (6.38)			-	521.18 (99.77)	522.27 (100.29)	-1.09 (70.73)		
Lyu et al, 2016	33 BE, 100% female	21.52 (1.37)	20.44 (2.33)	Spatial cueing task	RT (ms)	391.70 (12.94)	-	-	-	-	Average (16)
	31 CG, 100% female	21.42 (1.54)	19.64 (2.05)			437.05 (13.36)	-	-	-		

BED, binge eating disorder; BE, binge eating; CG, control group; AB, attentional bias; RT, reaction time; ms, milliseconds, SD, standard deviation; CI, confidence interval

Note. - signifies that the study did not provide this information

Table 4. Demographic and effect size comparison of visual search task paradigms measuring AB to food

Study	Sample	Mean age (SD)	Mean BMI (SD)	Paradigm	Measure of AB	Mean RT (SD) food	Mean RT (SD) non-food	Diff (SD)	Hedges's *g* (95% CI)	Study quality rating (raw score)
Schmidt et al, 2016	25 BED, 88.0% female	14.68 (2.85)	N. R	Visual search task	RT (ms)	948.48 (178.01)	967.56 (193.59)	-19.08 (185.96)	.03 (-.51, .58)	Moderately good (18)
	25 CG, 88.0% female	15.28 (2.39)	N. R			876.36 (203.76)	850.64 (197.58)	25.72 (200.69)		
Sperling et al, 2017	23 BED, 65.2% female	35.3 (11.39)	32.4 (9.24)	Visual search task	RT (ms)	765.23 (94.66)	773.05 (95.5)	-7.82 (95.08)	-.11 (-.67, .46)	Moderately good (19)
	23 CG, 65.2% female	35.96 (12.2)	32.79 (9.01)			798.00 (138.14)	793.41 (131.36)	4.59 (134.79)		

BED, binge eating disorder; CG, control group; AB, attentional bias; RT, reaction time; ms, milliseconds, SD, standard deviation; CI, confidence interval

Meta-Analytic Results

Due to large variations in the reporting of results, a meta-analysis on each paradigm was not possible. Therefore, a preliminary meta-analysis across paradigms was conducted, with a focus on the difference between groups on mean RTs to food and non-food stimuli. Specifically, the meta-analysis sought to highlight whether the BED group exhibited greater attentional engagement towards food stimuli when compared with controls across paradigms. While this form of meta-analysis is not as methodologically robust as conducting separate analyses for each paradigm, it has been conducted in other fields (Dudeney, Sharpe & Hunt, 2015; Field, Munafo & Franken, 2009). For example, Dudeney et al. (2015) calculated between-groups and within-groups differences and calculated a pooled Cohen's *d* effect size for the whole dataset, as well as for each paradigm where possible. Field et al. (2009) used the calculated AB scores to calculate the pooled effect size across paradigms. These methods were replicated for this review.

Difference between groups. Firstly, an analysis was conducted, comparing the BED group to the control group for RT towards food stimuli. Five studies were included in this analysis (Schmitz et al., 2014; Shank et al., 2015; Lee et al., 2017; Schmidt et al., 2016; Sperling et al., 2017). Five studies were excluded, as RT data for food stimuli was unavailable (Lyu et al, 2016; Duchesne et al, 2010; Manasse et al, 2015; Kittel, et al, 2017; Lavender et al, 2014). The analysis revealed a pooled Hedges's *g* effect of 0.2 (95% CI: -.05, .44), which was not significant ($p = .116$). One study (Sperling et al., 2017) produced a small effect size of -.3 (95% CI: -.84, -.30), which was in the opposite direction, potentially skewing the overall effect. While the small effect size demonstrates that the control group responded faster than the BED group

towards food stimuli, the group difference is not statistically significant, indicating that the effect is not large enough to infer firm conclusions (see Figure 3).

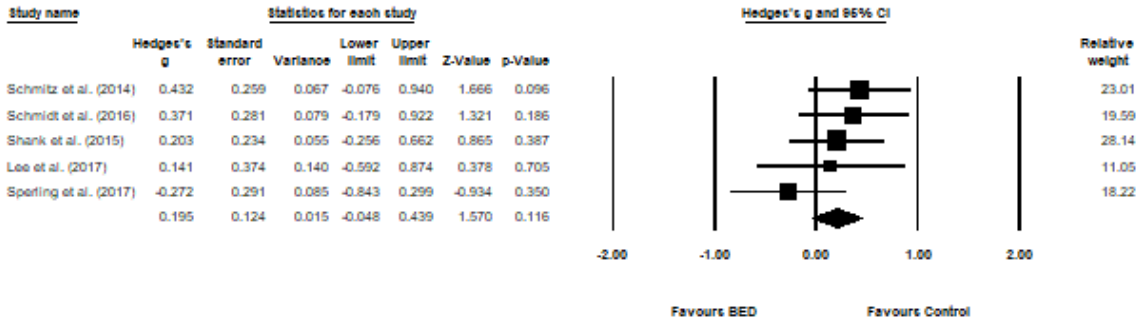

Figure 3. Difference in mean RT and standard deviation between the BED group and control group for food stimuli.

Risk of bias. When testing for heterogeneity, the Q-value was 3.82 (*df* = 4), with the I^2 being 0.00. These figures were not statistically significant ($p = .431$), indicating that the between-groups meta-analysis contained minimal heterogeneity (Cuijpers, 2016). When testing for possible publication bias, a visual inspection of the funnel plot indicated no publication bias, as the effect sizes appeared symmetrically distributed around the mean (Cuijper, 2016). The values remained unchanged after the 'trim and fill' analysis (Duval & Tweedie, 2000). Egger's test for interception (Egger, Smith, Schneider, & Minder, 1997) and Begg and Mazumdar (1994) Rank Correlation Test were non-significant ($p = .655$, $p = .462$ respectively). This means that there was little risk of bias across the studies (see Figure 4).

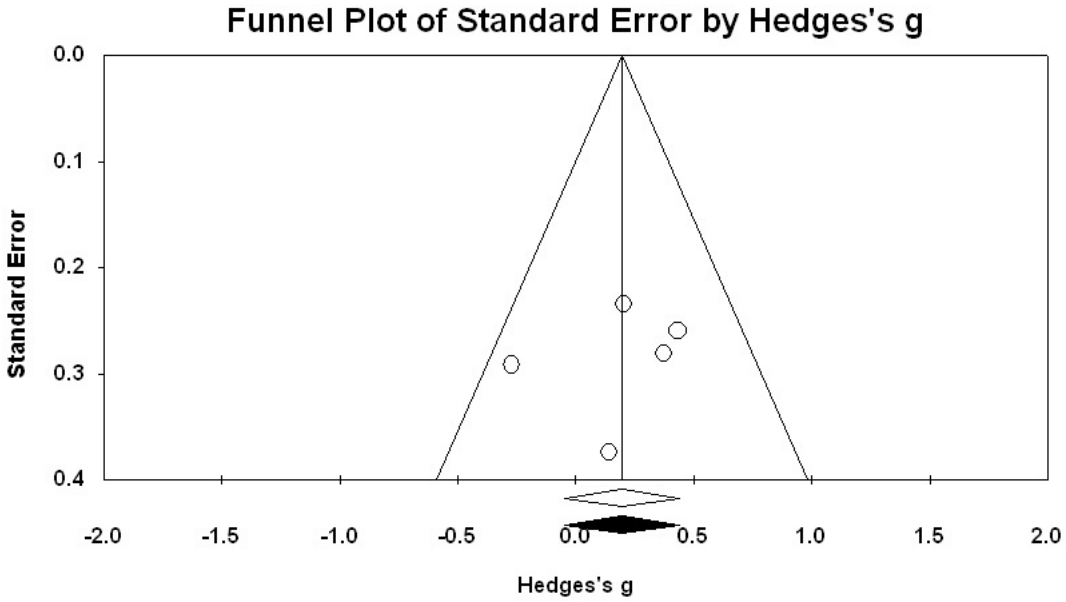

Figure 4. Funnel plot of standard errors of the difference in RT toward food stimuli between the BED group and control group.

Meta-analysis of AB scores. A meta-analysis was also conducted from the calculated AB scores (see Figure 5). The pooled Hedges's *g* was -.12 (95% CI: -.36, .12), which was not statistically significant (p = .339). The effect size is close to zero, indicating that there is no overall effect. This result highlights that, overall, the difference between RT towards disorder-salient and neutral cues was very similar between the two groups.

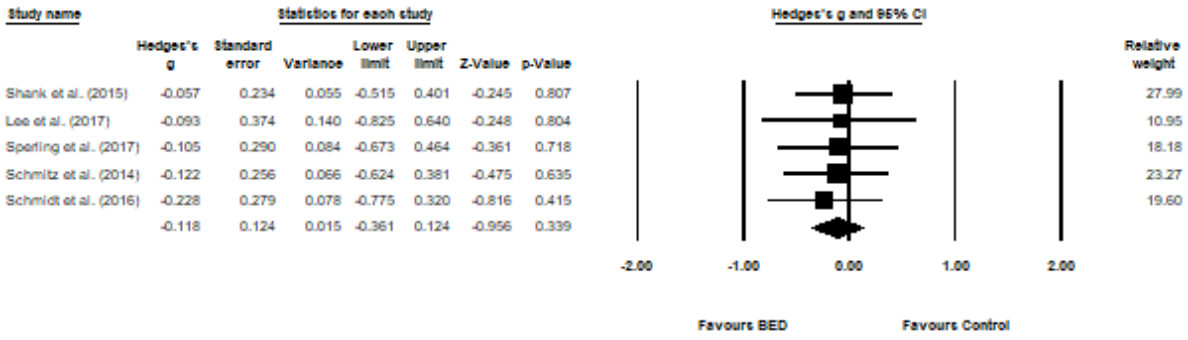

Figure 5. Forest plot of the difference between groups for AB scores

Additional analyses. A number of random-effects subgroup analyses were conducted in order to investigate the impact of different groups on the overall effect. A

subgroup analysis of BED and subthreshold BE and LOC experimental groups highlighted that only one study included in the analysis used a subthreshold sample (Shank et al., 2015). The BED group produced a small effect size of 0.19 (95% CI: -.14, .51), which was not significant (p = .264). This indicates that the two groups did not significantly differ in their scores. This means that these subgroups did not contribute heavily towards the overall effect, and supports the decision taken to include participants across clinical and subthreshold BED.

A subgroup analysis was also conducted in order to investigate the impact of specific paradigms on the overall effect. Of the four types of paradigms included in this review, the visual search task was the only paradigm to have more than one study included in the analysis. These two studies produced an overall effect of 0.05 (95% CI: -.58, .68). However, Schmitz et al. (2014) produced the greatest effect size of 0.43 (95% CI: -.08, .94), indicating that this study has contributed the most towards the overall effect size.

Lastly, a sensitivity analysis was conducted, in order to determine whether the results would have differed if the Stroop task had been omitted from the analysis (Lee et al., 2017). The reason for this was because the Stroop paradigm is designed to measure AB differently compared with the other paradigms. The results did not change when the study by Lee et al. (2017) was taken out, indicating support for analysing across paradigms.

Discussion

This review was an exploratory examination of a possible AB towards food cues, in individuals with clinical and subthreshold BED. This remains an under-researched population. The majority of studies found that the BED group responded more quickly to food stimuli than the neutral cues, indicating that they exhibited ABs

when completing the paradigm measures. However, the meta-analysis indicates that there was no statistically significant difference between groups.

This finding is in contrast to the systematic review conducted by Stojek et al. (2018), who indicated that people who BE have greater ABs compared to controls. This review found a number of studies that were not identified by Stojek et al. (2018); for example, Duchesne et al. (2010); Balodis et al. (2013); Manasse et al. (2015); Galioto et al. (2012); Kittel et al. (2017); Lavender et al. (2014); Sperling et al. (2017).

When compared with the results from existing literature for AN and BN, this review indicates that the BED group do not exhibit greater differences in AB scores compared with other EDs (Brooks et al., 2011). In particular, Brooks et al. (2011) found that the ED group yielded a small standard mean difference of .34. This is in stark contrast to the non-effect in this paper.

As the meta-analysis investigated BE, rather than the psychological impact of the consequence of the behaviour (i.e. weight gain), studies were excluded if they did not include a clinical or subthreshold BED group. The decision was made not to limit the inclusion of the control group to weight-matched controls, to increase the number of studies that could be included. Interestingly, the papers that met inclusion criteria predominantly recruited a weight-matched or an overweight comparison group, enabling the paper to demonstrate that with the exception of one paper (Schmidt et al, 2016), the BED group produced larger AB scores compared with controls. However, the overall effect was close to zero, demonstrating that there was not any real difference between groups.

The decision to include all of the studies that could not be meta-analysed highlighted the extent of the heterogeneity in the current literature's methodological approaches. Firstly, the studies varied considerably both in the use of paradigms used to investigate AB and in the measures used to calculate AB. A greater number of studies

have used the Stroop task to measure AB in BED compared with other paradigms. These studies ranged considerably in the methods they used to record AB, resulting in an inability to include all of the studies in the analysis. The Stroop task is considered a limited paradigm to test AB in EDs, due to the possibility of 'interference effect' (Starzomska, 2017). Secondly, the way in which the results are presented is varied. Of those that used RT as the primary measure of AB, few papers presented the raw RT scores for salient and neutral cues, for each experimental group. Again, this resulted in a number of papers being withdrawn. This information would not have been as evident if the studies that could not be meta-analysed had been omitted from the review. However, including them has resulted in a greater understanding of the current state of the literature in this area, and has highlighted areas in which future research can improve.

There appeared to be limitations to the samples used in the studies. For example, the papers varied in the type of control and/or comparison group used. The studies also predominately recruited females. Although this further reduces the generalisability of the findings, it represents the gender balance in the clinical population (Lacovino et al., 2012).

Strengths and Limitations

A significant strength of the meta-analysis is that it is novel and adds to the literature by quantifying the results of the current research. This paper has tentatively highlighted that the BED group do not respond in the way that is theorised, when comparing them to a control group.

The review was limited by the number of studies included in the analysis. It was not possible to meta-analyse each paradigm individually, which significantly hinders the validity of the findings. This is because the paradigms measure different constructs of attention.

The use of an adapted quality assessment tool, rather than a standardised measure, is a further limitation of the review. This is because it reduced the validity of the scores. Attempts were made to address this, by piloting the tool.

One potential further limitation is that the review did not include Electroencephalogram (EEG) and eye-tracking studies. Adding these experimental methods may have added depth to our understanding of how people with BE behaviours may exhibit AB towards salient stimuli, as these methods utilise 'direct' methods of assessing AB. The search included in this review resulted in the identification of two eye-tracking studies (Popien, Frayn, von Ranson, & Sears, 2015; Schag et al., 2013) and two EEG studies (Wolz et al., 2017; Svaldi, Tuschen-Caffier, Peky, & Blechert, 2010). Of these studies, Wolz et al. (2017) reported RT towards salient and neutral stimuli, while the others reported the dwell time or late positive potential scores towards high- and low-calorie food. Stojek et al. (2018) found the same number of studies, when including these paradigms in their searches. Therefore, the literature in this area is too sparse to currently meta-analyse.

Theoretical and Clinical Implications

The non-significant findings and lack of effect when comparing AB scores between groups has theoretical and clinical implications. Firstly, the results query the applicability of the cognitive theory of EDs to BED. The results of this meta-analysis failed to reject the null hypothesis, which tentatively suggest that there is no evidence to support the theoretical underpinnings of an information processing component of the cognitive model of BED. The tentative findings from this study suggest that people with BED do not necessarily exhibit ABs any more than people without BED. It is noted that the studies reviewed in this meta-analysis did not manipulate negative affect as a possible moderator of BE. However, this has been proposed as a potentially important factor in BE. The cognitive model of BN (Cooper, Wells, & Todd, 2004) suggests that

people with BN experience negative self-beliefs, which they have learnt to manage by eating. This is because eating lowers arousal levels and re-directs attention inwards. According to this model, people with BN experience conflict in their appraisal of food cues as they hold positive beliefs that permit them to binge, while also holding negative beliefs about the consequence of eating (Cooper et al., 2004). This model postulates that BE occurs while the permissive thoughts about food are strong and following negative emotional states, such as anxiety or loneliness (Cooper et al., 2004). BE acts as a mechanism to escape negative emotional states triggered by ABs. It is possible that people with BED experience stronger permissive thoughts related to eating, and fewer negative beliefs. They may also be sensitive to negative arousal; however, neither of these factors was measured in the studies included in this meta-analysis. Therefore, the nature of the binges in BED may be distinct from other EDs, as the ABs present in BED may trigger more pronounced uncomfortable mood-states, as opposed to dysfunctional schema related to body weight and shape, which is proposed by the cognitive model for AN and BN (Fairburn, 1981).

Secondly, if the cognitive theory does adequately explain the mechanisms maintaining BED, it is possible that the use of the experimental paradigms does not sufficiently test the theory. This meta-analysis indicates that the experimental tests may not be measuring the intended effect. It is possible that unlike AN and BN, people with BED do not experience the same level of fear response to food cues. If so, it is possible that exposure to other disorder-salient cues, such as threat-based cues, body cues, or cues related to negative permissive thoughts about eating may trigger a stronger response (Stojek et al., 2018).

Lastly, the comparison groups may have been too similar to the BED group, resulting in any true differences being masked. While the literature indicates that the psychopathology of BED is different to the non-BED obese population (Burrows,

Skinner, McKenna, & Rollo, 2017), it is possible that this difference is not quite as distinct when considering their cognitions. This meta-analysis highlighted that the two groups respond very similarly to food cues, which may imply that the difference between the groups may be more pronounced when comparing their reaction to more ED-specific psychopathology, such as cues that elicit negative self-awareness (Cooper et al., 2004).

Therefore, this meta-analysis contributes towards the current literature, as it suggests the current theoretical framework for BED require further investigation and adaptation, as well as the methods used to research the population.

This may have a number of potential clinical implications. It is possible to speculate that if people with a diagnosis of BED are attending to their environment in a different way compared with those with BN and AN, it may be necessary to modify treatment accordingly. Treatment currently targets maladaptive behaviours and cognitions around eating, weight and shape; it may however, be more appropriate to take a broader psychological approach, targeting affect if the BED population are in fact presenting with more in common with an obese population experiencing depression (Luppino et al., 2010). However, it must be recognised that the current findings are tentative, and the limitations of the current meta-analysis are acknowledged.

Future Research

The number of current studies investigating AB in BED, particularly studies that have used the Stroop task, is enough to warrant a meta-analysis. However, the methodologies that have been used are varied, and the way the authors report their findings is highly variable. This does not easily allow for a robust meta-analysis to be conducted. Future research would benefit from streamlining the methodology of the research. This can be achieved by using the same measures of AB, and clearly reporting

the raw scores for each experimental group and setting. Studies would also benefit from increased sample sizes and using the same methodology within the paradigm being investigated, such as using the same stimulus duration for the dot-probe tasks.

Future research may also benefit from more clearly defined BED and control samples. Currently, reported research is highly variable in the use of terminology, as some research has used BED with either obesity or subthreshold BE interchangeably. However, the two are not exclusive.

The quality measure further highlights that research in this area is not of a particularly high standard, with six of the included studies being rated as moderately good, and the remainder being rated average-moderately poor. The confidence in the study findings is therefore reduced.

Furthermore, future research may benefit from investigations into the possible association between BED and AB towards body cues and threat-based cues. In particular, testing the validity of the escape theory of BE may influence the development of appropriate treatments. Moderating mood-states as a variable, may also be of interest when investigating threat-based cues, as ABs are known to be prevalent in people who experience anxiety (Amianto et al., 2015).

Future research would also benefit from comparing the BED population with a group other than obese controls. Instead, it may be useful to compare the clinical BED group with other distinct EDs, such as AN and BN, in order to fully understand the psychopathological similarities and differences between the differing eating disorders.

Conclusions

The findings from this meta-analysis highlight possible discrepancies between the current conceptualisation and experimental testing methods of the cognitive component of BED. The BED group do not appear to exhibit ABs to a greater degree

than controls. However, the number of studies is too small and the way that ABs has been tested is currently too varied to able to make firm conclusions. There is a clear need for more robust research to be conducted within this field, in order to better understand the underlying mechanisms of BED, and how best to treat it.

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Conflict of Interest

None reported.

Declarations of Interest

None.

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Appendix A

Journal guidelines for Appetite

AUTHOR INFORMATION PACK
Appetite is an international research journal specializing in cultural, social, psychological, sensory and physiological influences on the selection and intake of foods and drinks. It covers normal and disordered eating and drinking and welcomes studies of both human and non-human animal behaviour toward food. Appetite publishes research reports, reviews and commentaries. Thematic special issues appear regularly. From time to time the journal carries abstracts from professional meetings.

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Appendix B

Quality Rating Scale

Description of subjects

Item 1. Diagnostic method and criteria for inclusion and exclusion

0	poor description and inappropriate method/criteria
1	full description or appropriate method/criteria
2	full description and appropriate method/criteria

Item 2. Documentation or demonstration of reliability of diagnostic methodology

0	poor or no reliability documentation
1	brief reliability documentation (documentation in the literature is sufficient, even if it is not explicitly cited)
2	full reliability documentation (documentation of within-study reliability necessary)

Item 3. Description of relevant comorbidities

0	poor or no description of relevant comorbidities
1	brief description of relevant comorbidities
2	full description of relevant comorbidities

Item 4. Description of numbers of subjects screened, included, and excluded

0	poor or no description of numbers screened, included, and excluded
1	brief description of numbers screened, included, and excluded
2	full description of numbers screened, included, and excluded

Item 5. Was the participation rate of eligible persons at least 50%?

0	No
1	Yes, does not clearly state this
2	Yes- Clearly states this and close to or all eligible persons participated

Definition and delivery of paradigm

Item 6. Paradigm(s) (including control/comparison groups) are sufficiently described or referenced to allow for replication

0	poor or no paradigm description or references
1	brief paradigm description or references (also if full description of one group and poor description of another)
2	full paradigm description or references

Item 7. Is the paradigm being used to measure the primary variable of interest?

0	poor description or not stated
1	Partial description of primary variable

2	Full description of primary variable

Outcome measures

Item 8. Does the study report the raw scores for the primary variable of interest?

0	No
1	Partial reporting (e.g. provide reaction times for food, but not for each group)
2	All of the raw scores reported for all groups and for all stimuli-types

Item 9. Were key potential confounding variables measured and adjusted statistically for their impact on the relationship between the paradigm and outcome?

0	poor or no description of confounding variables
1	brief description of confounding variables
2	full description adjustment for confounding variables

Item 10. Outcome assessment by raters blinded to participant group and with established reliability

0	poor or no blinding of raters to participant group (eg, rating by therapist, non-blind independent rater, or patient self-report) and reliability not reported
1	blinding of independent raters to participant group or established reliability
2	blinding of independent raters to participant group and established reliability

Item 11. Discussion of safety and adverse events during study experiment(s)?

0	poor or no discussion of safety and adverse events
1	brief discussion of safety and adverse events
2	full discussion of safety and adverse events

Data analysis

Item 12. Description of dropouts and withdrawals in the procedure

0	poor or no description of dropouts and withdrawals
1	brief description of dropouts and withdrawals
2	full description of dropouts and withdrawals (must be explicitly stated and include reasons for dropouts and withdrawals)

Item 13. Were all of the recruited participants included in the analysis?

0	No description
1	Not all participants were not included in the analysis, or a poor description was provided
2	Yes, all of the participants were included and adequately described

Item 14. Appropriate statistical tests (eg, use of Bonferroni correction, longitudinal data analysis, adjustment only for a priori identified confounders)

0	inappropriate statistics, extensive data dredging, or no information about appropriateness of statistics
1	moderately appropriate, though unsophisticated, statistics and/or moderate data

	dredging
2	fully appropriate statistics and minimal data dredging in primary findings

Item 15. Adequate sample size

0	inadequate justification and inadequate sample size
1	adequate justification or adequate sample size
2	adequate justification and adequate sample size

Experimental group assignment

Item 16. A priori relevant hypotheses that justify comparison group(s)

0	poor or no justification of comparison group(s)
1	brief or incomplete justification of comparison group(s)
2	full justification of comparison group(s)

Item 17. Comparison group(s) from same population and time frame as experimental group

0	comparison group(s) from significantly different population and/or time frame
1	comparison group(s) from moderately different population and/or time frame
2	comparison group(s) from same population and time frame

Overall quality of study

Item 18. Conclusions of study justified by sample, measures, and data analysis, as presented (note: useful to look at conclusions as stated in study abstract)

0	poor or no justification of conclusions from results as presented or insufficient information to evaluate (eg, sample or treatment insufficiently documented, data analysis does not support conclusions, or numbers of withdrawals or dropouts makes findings unsupportable)
1	some conclusions of study justified, or partial information presented to evaluate
2	all conclusions of study justified, and complete information presented to evaluate

Item 19. Omnibus rating: please provide an overall rating of the quality of the study, taking into account the adequacy of description, the quality of study design, data analysis, and justification of conclusions.

18 items in total/score range 0-36

1 = exceptionally poor (0-3)
2 = very poor (4-8)
3 = moderately poor (9-12)
4 = average (13-17)
5 = moderately good (18-23)
6 = very good (24-30)
7 = exceptionally good (31-36)

Chapter Two. Bridging Chapter

Word count: 3649

Chapter Two. Bridging Chapter

This chapter provides an overview of both i) the meta-analysis in chapter one on attentional biases (ABs) in binge eating disorder (BED) and subthreshold binge eating (BE) without compensatory behaviours, and ii) the meta-analysis in chapter three on eating disorder-focused family therapy (ED-FT) and multifamily therapy (MFT) for children and adolescents with a diagnosis of anorexia nervosa (AN) or bulimia nervosa (BN). Firstly, the theoretical underpinnings of the meta-analysis from chapter one are discussed. The results from this meta-analysis are then discussed in relation to the theory that has been outlined.

This chapter then discusses how the meta-analysis in chapter three relates to the previous paper by providing an overview of the current first-line treatments for all eating disorders (EDs) across the lifespan. The second meta-analysis reviews the literature on the current treatment for EDs in children and adolescents (ED-FT) and compares the results from these gold-standard randomised controlled trials (RCTs) to the developing evidence base of an alternative treatment, MFT, that is receiving increasing attention. The theoretical framework for these treatment models is also discussed. This chapter concludes by outlining the aims of the meta-analysis presented in chapter three.

Theoretical Underpinning of BED

There is no widely accepted specific theoretical model of BED (Burton & Abbott, 2017). Instead, research to date has explored whether BED fits into any of the existing theoretical models of eating disorders such as those for anorexia nervosa (AN) and bulimia nervosa (BN). This appears to have led researchers to trial treatments of

BED based on the theoretical understanding of EDs more generally. Therefore, an evidence-based understanding of the theoretical underpinnings of the psychopathological mechanisms that are specific to BED has not yet been developed. It is important that this is developed, so that researchers and clinicians can be sure that the intervention being delivered is targeting the correct psychopathological factors that maintain the disorder.

One theoretical ED model that has been applied to BED is the 'escape' theory (Heatherton & Baumeister, 1991), which proposes that people who binge-eat escape by averting their attention away from internal or external threat information. In particular, the urge to avoid negative affect (an internal threat) can trigger a binge episode (Stojek et al., 2018), and is thought to maintain the behaviour (Shafran, Lee, Cooper, Palmer & Fairburn, 2007). To date, however, research that has used threat-based cues when investigating ABs in BED has not been conducted (Stojek et al., 2018). Furthermore, this theory does not account for the possibility that BE is maintained by a decrease in negative affect post- binge (Haedt-Matt & Keel, 2011). Therefore, it does not adequately explain the maintaining factors in BED.

The emotional regulation model is another framework in which BED has been considered. This model theorises that episodes of BE are triggered by negative emotions. Over time, the act of BE becomes a mechanism to reduce the negative affect by providing emotional comfort and distraction (Haedt-Matt & Keel, 2011). Therefore, the emotional regulation model consists of two hypotheses: a) negative or uncomfortable emotions become associated with a trigger for BE, and b) BE is associated with the immediate decrease in negative affect. Several studies have supported the first aspect of the theory (Haedt-Matt & Keel, 2011). For example, Chau, Touyz, & Hill (2004) found that a negative mood condition was associated with a larger consumption of food compared to a neutral condition. However, there is inconsistent

empirical support for the second aspect of the theory, weakening the applicability of this model to BED. A recent meta-analysis also failed to support the model by finding a larger effect (.50) for an increase in negative affect following BE (Haedt-Matt & Keel, 2011). This model is further weakened by the use of retrospective research as this relies on memory which may not be accurate and is not as robust as other experimental designs (Shiffman, Stone, & Hufford, 2008).

The transdiagnostic model of eating disorders posits that there are common psychopathological processes that maintain all eating disorders, including BED (Fairburn, Cooper & Shafran, 2003). The model theorises that common clinical features such as an over-evaluation of weight, shape and control, perfectionism, and low self-esteem maintain ED symptoms, such as BE. However, this theory can be difficult to apply to BED, as very few studies have explored how BED fits within this model. In particular, previous research has not explicitly explored how possible biases in information processing may relate to the dysfunctional schema for self-evaluation, which is seen as integral to the model (Burton & Abbott, 2017). Furthermore, recent literature has argued that this theory requires updating to consider the core maintenance factors of the extreme spectrum of EDs and biological factors, which it currently does not (Treasure, Leslie, Chami, & Fernandez-Aranda, 2018). This is because the current treatment for BED is based on this model, and is only moderately effective, indicating that it does not fully address all relevant factors (Treasure et al., 2018).

The most empirically tested model of EDs is the cognitive model (Fairburn, 1981). This model suggests that the meaning placed on stimuli in the environment is influenced by internal schema, and this affects how information is processed. The way in which information is processed is thought to affect cognitions and emotions, which in turn has an impact on an individual's behaviours. However, this model has been

criticised for basing some of its empirical findings on community samples (Cooper, 2003), and for placing undue emphasis on the importance of dieting as maintaining BE, which has not been supported empirically (Cooper, Wells, & Todd, 2004). In particular this has been highlighted by research such as Allen, Byrne, and McLean (2012) who have used structural equation modelling to indicate that this model does not account for all variables in BED such as dietary restraint.

The cognitive model of BN (Cooper et al., 2004) provides a comprehensive framework for the development and maintenance of BE, significant components of which appear to be relevant to BED. This model suggests that triggering events can be either related to the ED, such as cues that relate to eating, body weight and shape, or unrelated, such as an argument. This event activates negative self-beliefs, leading to negative appraisals about the self.

The model then theorises that people with EDs have learnt to manage these appraisals through eating. This is because eating is associated with lowering arousal levels and facilitates an ability to escape from negative self-awareness. Individuals with EDs also have thoughts that they may 'lose control' of their negative thoughts if they do not eat. These thoughts about eating are considered 'permissive thoughts' about eating, as they allow the individual to believe the positive consequence of eating, at least in the short term.

However, people with EDs have also developed negative beliefs about food, that can be first-order thoughts that relate to assumptions linking eating with weight gain, such as 'I will get fat if I eat', or second-order thoughts that link eating with core beliefs, such as 'If I eat, then I've failed'. The conflict arising from having negative thoughts along with the permissive thoughts about eating are thought to cause feelings of distress, which precipitates the binging episode. When positive beliefs about BE give way to the negative, compensatory behaviours take place in order to avoid weight gain

and reduce negative self-worth. Therefore, it is possible that people with BED may not experience negative appraisals of eating which are as prominent. It is also possible that they may not experience the same degree of fear of weight gain, compared with people with a diagnosis of other EDs.

Results of the MA in BED and its Relation to Theory

Information processing relies on three main components: cognitive or ABs, working memory, and judgement (Brooks, Prince, Stahl, Campbell & Treasure, 2011). The meta-analysis in chapter one explored whether people with a diagnosis of clinical or subthreshold BED exhibit greater ABs towards food cues, compared with controls. The aim was to test the relevance of the cognitive models of EDs.

Of the 13 studies that met inclusion criteria, five provided the necessary data to be able to meta-analyse the findings. It was noted that few meta-analyses examine components of cues separately, and they in fact calculate an 'AB score' which they use for their analyses (Brooks et al., 2011; Field, Munafo & Franken, 2009). This meta-analysis replicated this: an AB score was calculated for each study by calculating the mean difference between reaction times for food cues and neutral cues.

The results indicated that the BED group produce marginally larger AB scores compared to controls. This may be explained by the approach avoidance notion highlighted by Cooper et al. (2004), as the BED group may have been exhibiting hypervigilance to cues that activate negative self-beliefs. However, the pooled Hedges's *g* of -.12 (95% CI: -.36, .12), showed a non-effect that was not significant ($p = .339$). This result indicates that the BED group find their attention is drawn towards disorder-salient cues faster compared with neutral cues. Crucially, this difference is not significantly larger than those without an ED.

This does not support the cognitive model or integrated cognitive-behavioural model as these models state that people with EDs hold dysfunctional schemas about body weight and shape, which has an impact on how they attend to stimuli in their environment (Fairburn, 2008). Therefore, according to this model, the BED group should be hypervigilant towards cues that activate distorted schemas, such as food.

It may be possible that the BED group differ to those with AN and BN as they may be more sensitive to other cues that trigger negative cognitions about eating, body weight and shape. For example, it may be that cues related to their appearance or self-worth are more effective than traditional food cues (Svaldi, Bender & Tuschen- Caffier, 2010). It is also possible that the results may be better explained by other theoretical models, which may have clinical implications on the treatment of BED, which are discussed in chapter five.

Interventions for Eating Disorders

The transdiagnostic model (Fairburn et al., 2003) and cognitive behavioural model (Fairburn, 2008) of AN and BN have led to the development of cognitive behavioural therapy (CBT) for EDs. Cognitive behavioural therapy (CBT) is increasingly considered the most effective treatment for BED (Hilbert et al., 2018). It is based on the components of CBT for BN, focusing treatment on breaking the diet-binge cycle by encouraging regular and structured eating (Lacovino, Gredysa, Altman & Wilfley, 2012). It aims to modify negative self-beliefs, improving self-esteem and concerns about weight and shape. It also encourages weight-control behaviours by self-monitoring and goal-setting. Therefore, the aim is to eradicate BE behaviours, with a secondary aim being weight-stabilisation (Lacovino et al., 2012). CBT has been shown to result in remission in 50% of patients with BED (Brownley et al., 2016), with 65-70% of patients experiencing a reduction in BE within the first four weeks of treatment

(Grilo, 2017). Currently, the National Institute for Health and Care Excellence (NICE, 2017) guidelines recommend a self-help version of CBT as first-line treatment of BED. However, a systematic review of psychological treatments for BED highlighted that CBT has been shown to be ineffective at lowering participants' weight (Brownley, Berkman, Sedway, Lohr & Bulik, 2007). This is an important limitation of CBT for BED, because although psychological functioning is improved by reducing the frequency of BE, weight gain and obesity continue to increase an individual's risk of secondary health complications and psychiatric co-morbidities (Pacanowski et al., 2018). The focus of weight stabilisation is pivotal in the treatment of AN and BN, and successful outcomes of treatment are often measured by rates of remission, which includes weight gain (Couturier, Szatmari & Kimber, 2013). Furthermore, studies have shown that people with a diagnosis of BED experience significant weight fluctuation, compared with non-BED controls (Amianto, Ottone, Daga & Fassino, 2015). Therefore, the measure of successful treatment of BED is thought to be abstinence from BE, and weight stabilization or weight loss, depending on the patient's baseline presentation (Amianto et al., 2015).

CBT has also been adapted to treat AN or BN, under the name of CBT-ED (Waller et al., 2011). While the National Institute for Health and Care Excellence (NICE; 2017) guidelines currently recommend CBT-ED as first-line treatment for adults with a diagnosis of AN or BN, this does not extend to working with children and adolescents with AN or BN (NICE, 2017; Couturier et al., 2013). Instead, eating disorder-focused family therapy (ED-FT) is considered to have the strongest evidence-base for treating this population (NICE, 2017).

While the meta-analysis on ABs in BED enables further exploration of how the disorder fits in with existing theoretical and treatment models for EDs, the second meta-

analysis examines the clinical implications of the current treatment model used to treat AN and BN in children and adolescents.

Theoretical Underpinnings of ED-FT and MFT

ED-FT is rooted within a family systems framework; drawing on a range of systemic and narrative principles and intervention techniques, while specifically focusing on the treatment of EDs (Eisler, Simic Blessitt, & Dodge, 2016). The theoretical underpinnings of the treatment have led to the development of treatment manuals and, in total, three manuals have been produced: Robin et al. (1994) were the first to produce a manual on behavioural family systems therapy for AN. This manual aims to treat AN within a multidisciplinary framework, by implementing treatment via a range of professionals such as dieticians, paediatricians and therapists. Its aims are to support the young person to restore their weight, change eating habits and help them to regain a developmentally-appropriate level of autonomy within the family system (Robin, 2003). This was followed by Eisler et al. (2016), who published a manual for anorexia-focused family therapy (FT-AN), and Lock, Le Grange, Agras, & Dare (2001), who produced a similar manual of family-based treatment for AN (FBT-AN) and BN (FBT-BN; Lock & Le Grange, 2013). FBT-BN places slightly more emphasis on giving the adolescent time alone in sessions as well as with their parents, which FT-AN does not (Lock & Le Grange, 2013). Otherwise, FBT-AN, FBT-BN and FT-AN are very similar in their approach, particularly as Lock originally worked at the Maudsley Hospital under Eisler's team, before branching off to develop the family-based treatment (Eisler et al., 2016). They all emphasise the importance of supporting the family to challenge the ED (Jewell, Blessitt, Stewart, Simic, & Eisler, 2016).

The majority of the research to date has centred on the efficacy of FT-AN, FBT-AN and FBT-BN. However, not enough research has been conducted on each manual to

be able to investigate them separately. Therefore, the term 'ED-FT' is commonly used within research (Jewell et al., 2016) and by NICE guidelines (2017) as an umbrella term to refer to all of the treatment approaches. While it is beyond the scope of this thesis to discuss in detail the theoretical underpinnings of ED-FT, the key shared principles from these manuals are discussed.

The first principle of ED-FT is to collaboratively work with the family to bring about change within the system. The treatment uses systemic techniques such as circular questioning to help the family explore multiple narratives for the same event and highlight differing perspectives (Dallos & Draper, 2000). Exploring the meaning attached to events enables the family to understand the differing function of the behaviours or situations.

The second principle is that the young person is not in control of their behaviours, but rather, the ED is in control of them (Lock & Le Grange, 2013). The aim of the treatment is to support the parents to temporarily 'take control' of their child's eating, until the child regains the strength to fight the ED themselves. The family life-cycle theory (Carter & McGoldrick, 2005) proposes that the family evolves through predictable developmental cycles; however, it can be difficult to adapt and change the homeostasis when unexpected events such as illness occur (Eisler et al., 2016). ED-FT advises the therapist to hold the family in positive regard at all times, encouraging the family to challenge their own processes rather than initiating direct interventions (Lock & Le Grange, 2013).

Finally, focus on the here-and-now (Eisler et al., 2016) is used within ED-FT, to encourage diminishing the behaviours and routines that are thought to be maintaining the ED. Principles derived from the maintenance framework are used to not make assumptions about the predisposing factors, but focus instead on how the family are organising themselves around the problem at present. This is achieved by sharing

tentative hypotheses based on any observations and inviting the family to give their perspectives (Eisler et al., 2016).

Multifamily therapy (MFT) is a method of delivering treatment to more than one family in group form (Jewell et al., 2016). Various models of MFT have emerged; however, the focus of this thesis is specifically on MFT that has been derived from ED-FT. This model of MFT uses the same principles of ED-FT but treats five-to-seven families at a time. The rationale is that families benefit from learning from each other and reduce feelings of isolation and shame (Jewell et al., 2016). Therefore, here on in, the term 'MFT' will be used to refer to MFT that uses the ED-FT framework.

MFT was pioneered by Laqueur (1972), who worked with patients hospitalised for symptoms of schizophrenia. They found that treating multiple families in one group improved the communication styles within and between families. The treatment model has since been applied to EDs and, more recently, applied to treating children and adolescents with EDs (Sholz &Asen, 2001). Currently, NICE (2017) guidelines recommend that ED-FT for AN can be delivered in MFT format. The theoretical framework on which MFT was developed is the same as the systemic principles that have been used in ED-FT. However, a key addition is that it draws on the systemic idea that negative feedback loops across sub-systems can cause difficulties in relationships (Asen, 2002). Changes within subsystems are made by exposing families to other families, as this allows them to gather different perspectives and mutual learning (Sholz & Asen, 2001).

While these systemic principles are clearly defined by the treatment models, they are difficult to measure and therefore difficult to investigate. Historically, there have been challenges in evidencing the efficacy of family therapy, with CBT lending itself more easily to RCTs (Frude, 1980). However, ED-FT has shown itself to be a strong contender when treating children and adolescents with EDs, which in part may

be due to it being manualised (Lock & Le Grange, 2013; Eisler et al., 2016). This is reflected in the 2017 change to NICE guidelines, which made clear recommendations to deliver ED-FT as first-line treatment to children and adolescents with EDs. Specifically, it advises the delivery of individual ED-FT for AN and BN, or a combination of individual ED-FT and MFT when treating children with a diagnosis of AN. The NICE guidelines (2017) do not specify whether a specific form of ED-FT should be followed when delivering the intervention. The research in this field reflects this, by using interchangeable terms when investigating the treatment model. Furthermore, it is unclear from the existing literature whether there are particular factors that influence the success of ED-FT above other family therapy approaches.

The evidence-base for MFT is still in its infancy and is hindered by the quality of the research, as much of the existing literature to date is from case studies. A recent RCT comparing MFT to ED-FT found that the MFT group had higher rates of remission at end of treatment (EOT) and follow-up (Eisler, 2016).

This result highlighted the need to investigate whether other forms of family therapy, including MFT, are effective when compared with ED-FT. This has not previously been investigated as previous meta-analyses have focused on comparing ED-FT with individual treatment (Couturier et al., 2013). Therefore, it was decided to conduct a meta-analysis specifically comparing rates of remission at EOT and follow-up for ED-FT to other forms of FT. As the RCT by Eisler et al. (2016) is the only one to have been completed in MFT, a meta-analysis of pre- and post-group outcomes for MFT was conducted, in order to investigate preliminary trends in the intervention.

Aims of the Meta-Analysis in Chapter Three

ED-FT is not found to be successful with all families (Le Grange et al., 2016). There are many possible factors that may contribute towards successful treatment

outcomes, including treatment fidelity, the quality of the research, and the impact of the service context in which the treatment is delivered (Hartling, Dryden & Klassen, 2009; Le Grange, Lock, Agras, Bryson & Jo, 2015; Prowse & Nagel, 2015). It is still also unclear whether ED-FT owes its success to a particular aspect of the treatment model, such as the use of a family meal (Cook- Darzens, 2016), delivering ED-FT to multiple families at one time (as in MFT), or adherence to the systemic principles described above. This has potential clinical implications as, at best, the current treatment model is successful in just over 60% of cases (Eisler et al., 2016). The proportion of children and adolescents who do not respond to treatment are at risk of suffering from an ED into adulthood, making their symptoms chronic and harder to successfully treat (Watson & Bulik, 2013). EDs are associated with low quality of life, high levels of disease burden and psychological co-morbidity (Le, Hay, & Mihalopoulos, 2017). Critically, AN has the highest mortality rate of any psychological diagnosis (Sadock & Sadock, 2017). Therefore, developing a greater understanding of how to successfully treat the disorder in childhood is vital. It is hoped that the results of the meta-analysis in chapter three will highlight key areas for future research that can inform the development of more robust treatment approaches. In particular, it hopes to answer whether ED-FT is as successful when compared with other forms of FT or modifications to the model, such as MFT.

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Chapter Three. Meta-Analysis of ED-FT and MFT in Children and Adolescents with Eating Disorders

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How Does the Emerging Evidence-Base for Multifamily Therapy Compare to the Current First-Line Treatment for Children and Adolescents with Eating Disorders? A Systematic Review and Meta-Analysis.

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Abstract

Eating disorder-focused family therapy (ED-FT) is the recommended treatment for young people with eating disorders (EDs). ED-FT has not previously been compared with other forms of family therapy (FT) or modifications to ED-FT, such as multifamily therapy (MFT). This review aimed to compare randomised controlled trials (RCTs) of ED-FT with the emerging evidence-base for MFT. Literature searches using PsychINFO, EMBASE, Medline, Pubmed and CINAHL were conducted in January 2018. Inclusion criteria comprised RCTs for ED-FT, and any quantitative study that investigated MFT with young people with EDs. Seven studies compared ED-FT with individual treatment, yielding a pooled odds ratio (OR) effect of 2.04 (95% CI: 1.13, 3.66). Five studies compared ED-FT to other forms of FT, generating a pooled OR effect of .63 (95% CI: .34, 1.15). The seven studies investigating MFT yielded a pooled Hedges's *g* effect of .64 (95% CI: .23, 1.05), indicating a significant effect on weight gain post-treatment. The generation of evidence in support of ED-FT compared with individual treatment has increased; however, the review indicated that the efficacy of ED-FT is more variable when compared with other forms of FT, highlighting the need for further investigation. The methodological quality of the studies on MFT needs to be improved before firm conclusions can be made about the efficacy of delivering ED-FT in MFT format.

Keywords: Children, adolescents, eating disorder, treatment, Multifamily therapy

Introduction

The theoretical origin of multi-family therapy (MFT) for eating disorders (EDs) lies within eating disorder- focused family therapy (ED-FT). MFT is comparatively under-researched compared with ED-FT's solid evidence-base, however is gaining increasing attention as an additional method to deliver ED-FT. The literature on ED-FT was last meta-analysed by Couturier, Szatmari, and Kimber in 2013. This review will synthesise the preliminary trends in MFT, and compare this to an updated review of ED-FT.

Eating Disorders in Youth

EDs are life-threatening mental health disorders, characterised by an excessive preoccupation with and over-evaluation of body weight and shape. Anorexia nervosa (AN) and bulimia nervosa (BN) are the most established diagnostic categories of EDs (American Psychiatric Association, 2013). AN is characterised by a significantly low body weight and intense fear of gaining weight, leading to a restriction in nutritional intake. BN is characterised by frequent episodes of binge eating, followed by compensatory behaviours such as purging (Costa & Melnik, 2016). Both AN and BN have serious physical consequences, with AN having the highest mortality rate of any psychiatric disorder (Sadock & Sadock, 2007). Approximately 13% of adolescents develop an ED by the age of 20 years (Stice, Marti & Rhode, 2013) and many go on to experience co-morbid difficulties, requiring hospital care (Le, Hay & Mihalopoulos, 2017).

In the last decade the development of evidence-based psychological treatments for children and adolescents with AN and BN has received increased attention. In particular, early intervention models of EDs are being developed to prevent the ED

from becoming chronic. In addition, the 'access and waiting time standard' stipulates that young people should begin treatment within four weeks of referral to community services (NHS England, 2015). This is important as AN in particular becomes more difficult to successfully treat in adulthood (Watson & Bulik, 2013).

Background on ED-FT

ED-FT is recommended by the National Institute for Health and Care Excellence (NICE) guidelines in England as the first-line treatment for AN and BN in children and adolescents (NICE, 2017). The Maudsley Child and Adolescent Eating Disorders Service (Dare & Eisler, 1997) developed and manualised AN-focused family therapy (FT-AN). Lock and Le Grange (2013) also manualised the treatment for AN and BN (Lock, Le Grange, Agras, & Dare, 2001; Lock, Le Grange, Agras, & Dare, 2002; Le Grange & Lock, 2007), which is referred to as 'family-based treatment (FBT)'. The manuals by Lock and Le Grange (2013) were developed based on the early treatment studies at the Maudsley Hospital; the manuals are very similar and are often referred to using interchangeable terms (Eisler et al., 2016). Both therapeutic models draw on family systems theory and use a range of family therapy (FT) techniques to treat the ED in the young person's day-to-day environment. The theoretical premise of the intervention is that rather than being the cause of the ED, the parents are useful resources that should be empowered to challenge the ED on behalf of the child (Eisler, Simic, Blessitt, & Dodge, 2016). The focus of treatment is therefore on the 'here- and now' ED symptoms. It comprises four phases in which carers take initial responsibility for the child's eating, gradually giving responsibility back to the child when the ED symptoms have diminished, and they have weight-restored (Rienecke, 2017).

The evidence-base for ED-FT is developing, observable in the steady increase in randomised control trials (RCTs) published over the last 20 years. These RCTs have

been extensively reviewed (Couturier et al., 2013; Fisher, Hetrick, & Rushford, 2010; Watson & Bulik, 2013). For example, Couturier et al. (2013) indicated that family-based treatment that follows 'Maudsley principles' (i.e. either FT-AN or FBT) is superior to individual treatment for children and adolescents at six- and 12-month follow-ups. The meta-analysis was limited by the lack of quality assessment within the studies. They also did not conduct a meta-regression, and therefore did not investigate the possible impact of certain factors, such as treatment dose, on the effect sizes.

However, research has highlighted some limitations of ED-FT. For example, Eisler, Le Grange & Lock (2015) indicated that 10-20% of clients receiving FT-AN require additional treatment, while others have reported remission rates in ED-FT as less than 50% at end of treatment (EOT) or six- to 12-month follow-up (Le Grange et al., 2016; Lock et al., 2010).

Background on MFT

Multifamily therapy (MFT) has been proposed as an alternative method of delivering ED-FT. MFT is a therapeutic model that treats multiple families at the same time. It is based on the concept that families experiencing similar difficulties can support and learn from each other by sharing their experiences (Asen & Schuff, 2006). It is also thought to relieve parents from feeling isolated, and give them a sense of de-stigmatisation, by providing a space to share often-difficult emotions, such as fear, guilt and failure (Asen, 2002).

The NICE (2017) guidelines recommend that FT-AN for children and adolescents can be delivered as a combination of single ED-FT and MFT. Research into the effectiveness of MFT for children and adolescents with BN has also been conducted (Depestele et al., 2017; Gelin, Fuso, Hendrick, Cook-Darzens, & Simon, 2015; Marzola et al., 2015; Stewart, Voulgari, Eisler, Hunt, & Simic, 2015); however, it

is not currently recommended by NICE. This may in part be due to a lack of research, as the majority of the research has focused on MFT for AN (Rienecke, 2017).

Unlike single-family ED-FT, research into the effectiveness of MFT for children and adolescents with AN and BN appears to be in its infancy, as much of the existing literature has focused on the theoretical underpinnings of the treatment model or reporting of preliminary findings from service audits. Recent years have seen an increase in pilot studies (usually lacking a comparison or control group) and a few control trials. Eisler et al. (2016) conducted the only RCT, which found that 75% of the MFT group achieved good or intermediate outcomes on the Morgan-Russell Scale (Morgan & Russell, 1975) compared with 60% of the FT-AN group. While the methodological rigour of the research in MFT is not as robust as the RCTs on ED-FT, MFT does appear to be producing promising findings. However, to date, the research on MFT has not been systematically reviewed.

As the evidence-base for MFT for the treatment of EDs is still growing, it may benefit from the findings being synthesised and compared with the evidence-base for the established treatment model for AN and BN. To the author's knowledge, this will be the first meta-analysis comparing MFT for AN and BN with ED-FT for children and adolescents.

Aims of the Review

The aims of this review are twofold. First, it aims to provide an up-to-date meta-analysis of ED-FT for children and adolescents with AN and BN. This review will differ to the one conducted by Couturier et al. (2013) as it will include studies that have compared ED-FT to other forms of FT or modification to ED-FT. Second, it aims to explore the effectiveness of MFT for children and adolescents with EDs by conducting a preliminary meta-analysis of the findings to date. It will also review the quality of

research for MFT alongside the research in ED-FT. It is hoped that synthesising the results will highlight themes that may have contributed towards the effectiveness of interventions, such as increase in weight; use of outcome measures that capture changes in ED psychopathology; adherence to specific treatment manuals; and the healthcare service contexts in which the studies were conducted.

This meta-analysis aims to answer the following questions, in line with the study population, intervention, comparison, outcome and study type (PICOS) tool (Centre for Reviews and Dissemination Guidelines, 2009). In relation to this paper, the population is defined as children and adolescents with a diagnosis of AN or BN; the intervention is ED-FT or MFT; the comparison is other forms of treatment including other forms of family therapy or individual treatment; the outcome is measured as remission or factors pertaining to this, such as changes in weight; and the study type is RCTs for the studies on ED-FT and any quantitative experimental design for studies on MFT. Based on these criteria, the research questions this meta-analysis hopes to answer are:

1. What are the remission rates for ED-FT compared with individual treatment at end of treatment and follow-up?

2. What are the remission rates for ED-FT compared with other forms of FT at end of treatment and follow-up?

3. Does the treatment dose (number of sessions) and study quality affect the pooled effect sizes for ED-FT, individual treatment and other forms of FT?

4. Do patients receiving MFT experience an increase in weight from pre-intervention to post-intervention?

Method

Search

Prior to commencing the search, The Cochrane Library and the international prospective register of systematic reviews (PROSPERO) were searched for existing similar reviews. The review protocol was registered on PROSPERO (CRD42018087286). The preferred reporting items for systematic reviews and meta-analyses (PRISMA) guidelines were used to guide the search and analysis (Liberati et al, 2009). A systematic literature search was conducted from the following electronic databases: PsycINFO, EMBASE, Medline, Pubmed and CINAHL. Hand-searching of journals was also conducted by checking reference lists and search engines, such as Google Scholar. Grey literature was searched using the electronic thesis online service database, and journals that have published articles in the field were searched for relevant articles that were in press. Relevant papers were identified using the following search terms: "(eating disorder focused family therapy OR anorexia focused family therapy OR Maudsley Family-based therapy OR Family Therapy OR Maudsley Family therapy OR family based treatment OR Maudsley service model OR family-based approach OR Multi-family therapy OR multiple family therapy OR single family therapy)" AND (eating disorder* OR anorexia nervosa OR bulimia nervosa OR anorexia OR bulimia) AND (young people OR adolescen* OR youth OR teen* OR child*).

Study Selection

Studies were limited to those published in English and in peer-reviewed journals. A date restriction was not placed on the search to maximise the number of studies that could be included in the review, particularly as there is limited research into MFT. Different inclusion and exclusion criteria were developed for the studies on MFT and

ED-FT, as the existing evidence-base for ED-FT is more established than the emerging literature on MFT.

The methodology of the studies included for ED-FT was restricted to RCTs. This is because RCTs are commonly considered 'gold standard' for establishing a causal effect of interventions (Watson & Bulik, 2013). The RCTs were included for review if they solely or predominantly included children and adolescents, and if they investigated anorexia- or bulimia-focused family therapy. Treatment in the control or comparison group was restricted to either forms of individual treatment, or different forms of FT including variants of ED-FT. Studies were excluded if changes in ED symptomatology was not a measured outcome of the intervention.

Studies on MFT were selected if they included participants that were solely or predominantly children and adolescents with a diagnosis of AN or BN, as defined by the DSM-5. They were also included if they investigated ED-focused MFT as defined by the NICE guidelines (NICE, 2017).

Studies were excluded if they were qualitative; however, a restriction on quantitative methodology was not employed beyond excluding single-case studies as, to date, only one RCT has been conducted on MTF.

For both searches, studies were excluded if they recruited solely or predominantly adults (over-20-year-olds) since the focus of the review was young people. As this review was primarily interested in AN and BN, papers were excluded if they solely investigated eating disorder not otherwise specified (EDNOS); binge eating disorder; or avoidant restrictive food intake disorder. Finally, studies were excluded if they did not follow principles from an ED-FT treatment manual.

Study Identification

A preliminary literature search was conducted in January 2018 by the primary author, using the 'healthcare database advanced search'. The search resulted in 2839 papers. After accounting for duplications, 1839 studies remained, of which all of the titles and abstracts were read by the primary author. Forty full papers for ED-FT and 26 full papers for MFT were then obtained and reviewed by the primary author. Fifteen relevant papers for ED-FT and seven relevant papers for MFT remained, based on the eligibility criteria. Two full papers were unavailable as they were abstracts from conferences. Figure 1 highlights the process of study selection, in line with the PRISMA consort diagram. Data on participant features, clinical settings, findings, outcome measures and any effect sizes were extracted from the studies.

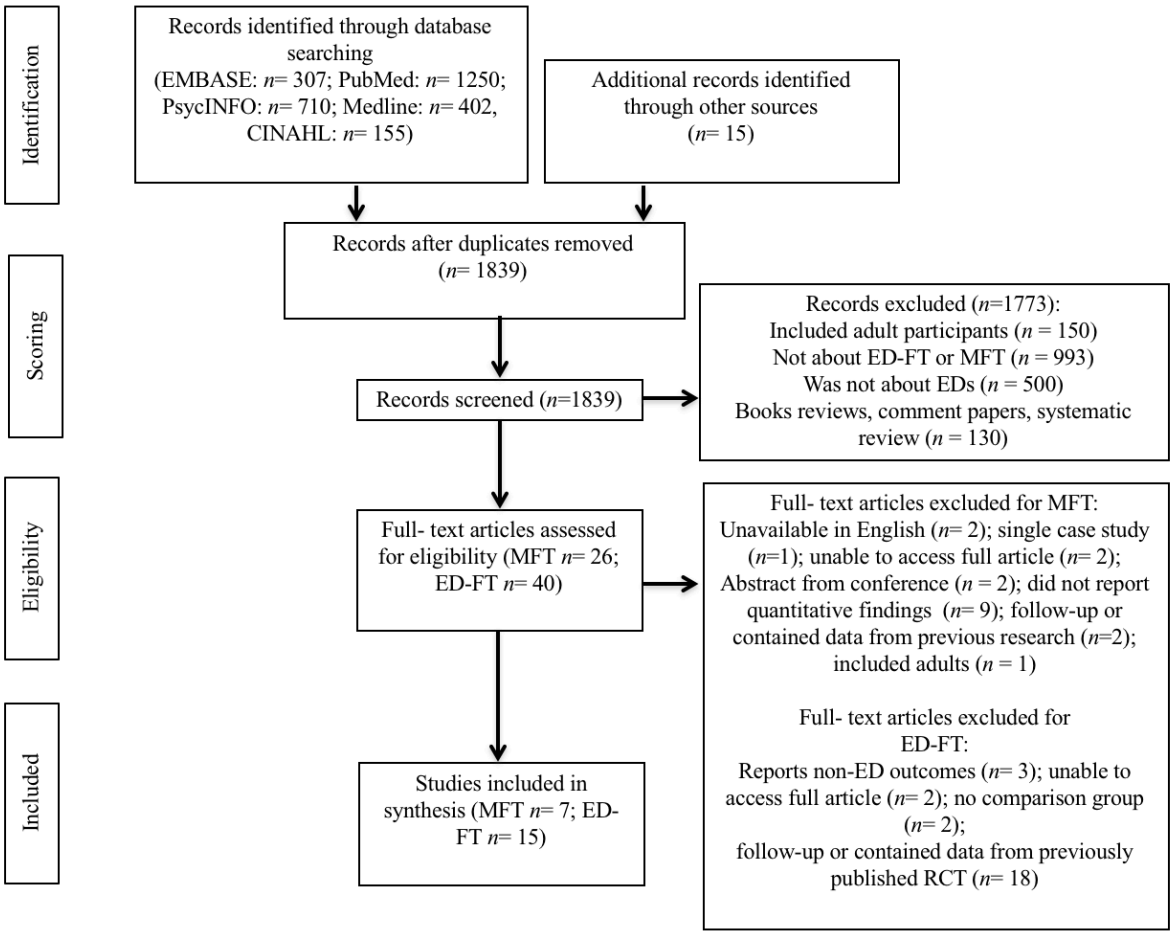

Figure 1. PRISMA flow diagram

Coding and Data Extraction

Data were coded and extracted from each study. This information was related to 1) descriptions of the trials, including authorship, country, year of publication, experimental design, experimental setting and methodology; 2) characteristics of the included participants, including total number of participants, age-range, diagnoses and how the diagnoses were determined; 3) characteristics of the treatment arm, including model used, whether this was manualised, treatment dose including duration and number of sessions; 4) characteristics of the control arm, including intervention type and dose; 5) the primary outcome and how this was defined, including the outcome measures used. The primary author (RJ) developed an electronic database, onto which the extracted data was inputted.

Quality Assessment

The RCT of Psychotherapy Quality Rating Scale (RCT-PQRS; Kocsis et al., 2010) was used to assess the quality of the included papers. This tool contains 24 items which assess different factors that contribute towards high-quality research (see appendix B). A value of zero-to-two was assigned to each item, with the sum of scores being out of 48. This translated to a qualitative category ranging from 'exceptionally poor' for scores between zero to five, to 'exceptionally good' for scores of 42 to 48. The RCT-PQRS has been shown to have good internal reliability (Gerber et al., 2011) and external validity (Kocsis et al., 2010). RJ independently scored each article and the secondary author assessed 20% of the papers, before comparing results. Any discrepancies were discussed and resolved by referring to the inclusion criteria. Minimal discrepancies occurred, and full agreement was reached on discussion. The inter-rater reliability index was calculated using Cohen's kappa (Landis & Koch, 1977). The kappa coefficients were calculated from the ratings of all 24 items for the four

studies that both primary and secondary authors rated. Cohen's kappa result was .56, which is considered moderate agreement (Landis & Koch, 1977). The percentage of agreement was 71.4%.

Analysis

Comprehensive meta-analysis (CMA) software (version 3; Borenstein, Hedges, Higgins & Rothstein, 2005) was used to conduct the meta-analysis.

Publication Bias

Studies that produce a large effect are more likely to be published (Borenstein et al., 2009). Risk of publication bias was calculated by using the 'trim and fill' method (Duval & Tweedie, 2000) to estimate the number of studies that would be needed to produce a symmetrical funnel plot. The funnel plot was visually inspected to confirm this. The Classic Fail-safe N (Rosenthal, 1979) was also used to calculate the number of studies needed to nullify the effect. Rosenthal (1979) advises that if this number is large, it can be inferred that the treatment effect is not nil.

Analysis of ED-FT

An Odds ratio (OR) was calculated in order to standardise the findings from the RCTs. An OR was chosen because the outcome of interest, rates of remission, is a dichotomous outcome and therefore, the OR indicates the relative odds of remission from AN or BN after having received ED-FT, compared with the odds of remission after having received either individual therapy or other forms of FT. In order to calculate the OR, the numbers of cases that met the studies' criteria for remission and the total number of participants was collected from the ED-FT arm and comparison arm of each study. The 95% confidence interval was calculated for this, determining the range in effect (Cuijpers, 2016).

The pooled effect size was computed within a random-effects model. This was chosen over a fixed-effect model because the random-effects model assumes that the treatment effects are randomly distributed across populations and allows for greater variability both within and between studies (Borenstein, Hedges, Higgins, & Rothstein, 2009).

In order to assess heterogeneity, Cochran's Q-statistic (Q) and I-squared (I^2) were computed (Borenstein et al., 2009). While the Q indicates whether heterogeneity is likely to be present and statistically significant, the I^2 highlights the extent to which this variability is due to real-life differences in effect sizes between studies (Borenstein et al., 2009). The I^2 ranges from 0-100%, signifying small (25%), medium (50%) and large (75%) heterogeneity (Borenstein et al., 2009).

Additional Analyses of ED-FT

A random-effects meta-regression was conducted in order to further distinguish any possible relationship between factors that may impact on treatment success. Specifically, the possible relationship between treatment dose as measured by number of sessions, and study quality on rates of remission was analysed. A sensitivity analysis was also conducted in order to ascertain whether the exclusion of the lowest-quality studies had an impact on the overall effect.

Analysis of MFT

The pre- and post-intervention changes in weight for MFT were analysed, due to the majority of the studies not using a control or comparison group. The effect sizes were calculated using the mean weight values, p-value and t-value, based on paired t-tests (Borenstein et al., 2009). Hedges's g was used when reporting the effect size. This is because Hedges's g takes into account small sample sizes (Cuijpers, 2016) and

provides a more precise illustration of the relationship between two groups compared with Cohen's *d* (Borenstein et al., 2009). The 95% confidence intervals were also computed. The effect sizes were reported using a random effects model, for the same reasons outlined above.

Results

Part One: Review of ED-FT

Excluded studies. In total, 40 RCTs investigating ED-FT with children and adolescents were found. Twenty-five of these did not meet inclusion criteria for the following reasons: 18 studies were either follow-up studies or reported different aspects of an original RCT; two studies did not have a distinct comparison group; three studies focused on non-ED outcomes; and two full articles were unavailable when the full-texts were requested.

Included studies. Fifteen published RCTs (*N*= 1153 participants) were identified (Table 1). Of these, seven compared ED-FT with individual therapy and eight studies compared ED-FT with either another form of family intervention, or adaptations to ED-FT.

ED-FT versus Individual therapy. There appears to be a rather slow output of published studies with one RCT (Le Grange, Lock, Agras, Bryson, & Jo, 2015) being published since the review by Couturier et al. (2013). Le Grange et al. (2015) compared the efficacy of FBT-BN with cognitive behavioural therapy adapted for adolescents (CBT-A) in a sample of 130 adolescents. They found that post-intervention the FBT-BN group reported higher abstinence rates from binge eating and vomiting than the CBT-A group at the EOT (39% and 20% respectively) and six-month follow-up (44% and 25% respectively).

ED-FT versus other forms of FT. Four of the eight studies comparing ED-FT to other forms of FT, including adaptations to the model, have been published since the most recent meta-analyses (Watson & Bulik, 2013). Agras et al. (2014) compared FBT for AN with systemic FT and found no statistical difference in remission and expected body weight (EBW) between groups at EOT or follow-up. However, they found that FBT led to fewer days in hospital and lower treatment costs per patient at EOT compared with the systemic FT group. Le Grange et al. (2016) compared the efficacy of FBT for AN with parent-focused treatment (PFT), which delivers separate sessions for the parents and adolescent. They found that remission, as measured by >95% EBW and a mean score on the global Eating Disorder Examination (EDE) within one standard deviation, was higher in PFT than FBT at EOT, but did not differ statistically at six- to 12-month follow-up. Lock et al. (2015) investigated the feasibility of an adaptation to FBT for AN (intensive parental coaching, IPC). The study was not powered to examine treatment effects between groups, and so the primary outcome was attrition and treatment suitability. Eisler et al. (2016) randomised 169 adolescents diagnosed with AN or partial AN to receive either FT-AN or MFT for AN. They found that a larger number of participants in the MFT group (75%) achieved good to intermediate outcomes on the Morgan-Russell scale (Morgan & Russell, 1975) compared with the FT-AN group (60%). This was a statistically significant difference at EOT (OR= 2.55, 95% CI: 1.17, 5.52. p = .019), but not at six-month follow-up.

Measure of remission. Overall, the studies were aligned in their selection of primary outcomes and the measures used to assess this. Most studies measured remission by monitoring changes in EBW or body mass index (BMI) and abstinence from binge-purging behaviours. The outcome measures used to measure remission were predominantly changes in the mean global Eating Disorder Examination (EDE; Fairburn & Cooper, 1993) score within one standard deviation, or achieving a good to

intermediate outcome on the Morgan Russell Scale (Morgan & Russell, 1975). Many of studies that used the latter measure found that the majority of participants in the ED-FT group achieved good to intermediate results at EOT (Ball & Mitchell, 2004; Eisler et al., 2000; Russell, Szmukler, Dare, & Eisler, 1987). A 'good' outcome was determined by participants' weight being above 85% EBW, who were menstruating if appropriate and whose ED symptoms has ceased entirely. An 'intermediate' outcome was achieved if the same weight was gained, but who were either not menstruating or who had occasional symptoms (Le Grange, Eisler, Dare, & Russell, 1992; Eisler et al., 2016).

Quality assessment. The quality of the studies varied considerably. Seven studies met the RCT-PQRS quality criteria for the moderately good and very good rating (Kocsis et al., 2010), including randomisation, allocation concealment, use of blind or independent assessors, use of and adherence to a manualised intervention, and intent-to-treat analysis (Eisler et al., 2016; Le Grange et al., 2016; Le Grange et al., 2015; Le Grange, Rathouz, Crosby, & Leventhal, 2007; Lock, Agras, Bryson, & Kraemer, 2005; Lock et al., 2010; Lock & le Grange, 2005; Schmidt et al., 2007). The remaining studies did not report these criteria in their methodology. The results of these studies should be given greater credence, as the methodology used is more robust compared with the other studies.

Treatment fidelity. The studies also varied in their use or adherence to a specific treatment manual. Half of the studies reported using either FT-AN, FBT-AN or FBT-BN, while the other half reported using principles from these manuals. The majority of the studies poorly reported their attempts to adhere to treatment fidelity, with only four studies (Agras et al., 2014; Geist, Heinmaa, Stephens, Davis, & Katzman, 2000; Robin et al., 1999; Russell et al., 1987) detailing this. It is therefore unclear how closely the studies followed the treatment manual. This may have an impact on the generalisability of the findings, as it is difficult for future research to replicate the results. It is also

difficult to understand which elements of the treatment may be particularly effective in facilitating and sustaining change.

Service context. Five studies (Agras et al., 2014; Le Grange et al., 2015; Lock et al., 2010; Lock et al., 2015; Schmidt et al., 2007) were conducted across multiple research sites and one study recruited participants from an inpatient and outpatient service (Russell et al., 1987). The remainder of the studies took place in outpatient settings from specialist ED services. The therapists delivering the treatment all had previous experience of working in the field and were often members of the multidisciplinary team (MDT) at the service from which the participants were recruited. Few studies considered the impact that the service context or therapist might have on outcomes. Russell et al. (1987) and Ball and Mitchell (2004) acknowledged a possible therapist bias, including individual differences and therapeutic skills. They sought to overcome this by assigning the therapists to deliver both treatment arms. Le Grange et al. (2015); Lock et al. (2010) also considered the study sites used in their research a limitation as they were established services, experienced in conducting research into FBT.

Table 1. Studies included in review: RCTs of ED-FT

Study	Sample	Methods	Setting	Treatment	Primary outcome	Primary outcome measure(s)	Findings	Odds ratio Effect Size (95% CI)	Study quality rating (raw score)
Schmidt et al. (2007)	85 adolescents (aged 13-20), DSM-IV diagnosis of BN or EDNOS	Random allocation to FBT or CBT guided self-care. Allocation concealment, intent-to-treat analysis, blind assessors	Outpatient across four NHS services in the UK	Adapted from FT-AN six-month duration (Up to 13 FBT sessions and two individual sessions)	Abstinence from binge-purge episodes at EOT and six-month follow-up	SEED	FBT group had 4/41 in remission compared with the CBT group (6/46) at EOT. This difference was reversed by six-month follow-up (FBT: 12/41 vs CBT: 9/44)	EOT: .69 (.18, 2.63) Follow-up: 1.61 (.60, 4.35)	Moderately good (32)
Le Grange et al. (2007)	80 adolescents (aged 12-19 years), DSM-IV of BN or partial BN	Random allocation to FBT or SPT. Allocation concealment, intent-to-treat analysis, independent assessors	Outpatient	FBT-BN six-month duration (20 sessions)	Abstinence from binge-purge episodes	EDE	The FBT-BN group had 16/41 in remission compared with 7/39 in the SPT group at EOT. This difference was reversed at 6-month follow- up (FBT: 12/41 vs	EOT: 2.93 (1.04, 8.20) Follow-up: 3.62 (1.05, 12.44)	Very good (35)

							SPT: 4/39)		
Lock et al. (2010)	121 adolescents (aged 12-18 years), AN (Diagnostic system unspecified)	Two-site random allocation to FBT or AFT, allocation concealment, intent-to-treat analysis	Outpatient	FBT-AN 12-month duration (24 sessions)	Remission from AN (>95% EBW, mean global EDE score within one standard deviation)	EDE	No difference between groups at EOT (FBT: 21/61 vs AFT: 12/60). At 12-month follow-up, the FBT group had a significantly higher number of participants in remission (22/61) compared to the AFT group (11/60)	EOT: 2.10 (.92, 4.79) Follow-up: 2.51 (1.09, 5.80)	Very good (38)
Le Grange et al. (2015)	130 adolescents (aged 12-18 years), DSM-IV diagnosis of BN or partial BN	Two-site random allocation to FBT-BN or CBT, intent-to-treat analysis, allocation concealment, blind assessors	Outpatient	FBT-BN Six-month duration (18 sessions)	Abstinence from binge-purge episodes for four weeks	EDE	FBT-BN produced significantly higher abstinence rates (20/51) compared with CBT (11/58) at EOT. This was not significant at 12-month follow-up (FBT-BN: 25/51 vs CBT: 19/58)	EOT: 2.76 (1.16, 6.54) Follow-up: 2.38 (1.05, 5.40)	Very good (37)
Le Grange	107 adolescents	Random allocation to	Outpatient	FBT-AN	Remission from AN	EDE	PFT produced significantly higher	EOT: .37	Very good (37)

et al. (2016)	(aged 12-18 years), DSM-IV diagnosis of AN or partial AN	FBT or PFT, intent-to-treat analysis, allocation concealment, independent assessors		Six-month duration (18 sessions)	(>95% EBW, mean global EDE score within one standard deviation)		rates of remission (22/51) than FBT-AN (12/55) at EOT. This was not significant at 12-month follow-up (FBT-AN: 16/55 vs PFT: 19/51).	(.16, .86) Follow-up: .69 (.31, 1.56)	
Agras et al. (2014)	164 adolescents (aged 12-18 years), DSM-IV diagnosis of AN	Random allocation to FBT or SyFT, intent-to-treat analysis, blind assessors	Outpatient across six clinical sites in the USA	FBT-AN Nine-month duration (16 one-hour sessions)	Remission from AN (>95% EBW)	Weight, EDE	No significant difference in remission between FBT (25/78) and SyFT (22/51) at EOT or 12-month follow-up.	EOT: 1.42 (.71, 2.83) Follow-up: 1.04 (.55, 1.98)	Average (26)
Lock et al. (2015)	45 adolescents (aged 12-18 years), DSM-IV diagnosis of AN	Two-site unbalanced allocation to FBT or FBT+IPC, blind assessors	Outpatient	FBT Six-month duration (15 sessions)	Attrition, treatment suitability, clinical outcomes	>95% EBW, recruitment and attrition rates, TSPE, EDE	FBT (5/10) produced lower rates of remission compared with FBT+IPC (17/35) at EOT	EOT: 1.06 (.26, 4.32)	Average (20)

Study	Sample	Methods	Setting	Treatment	Primary outcome	Primary outcome measure(s)	Findings	Odds ratio Effect Size (95% CI)	Study quality rating (raw score)
Robin et al. (1999)	37 adolescents (aged 11-20 years), DSM-III-R diagnosis with AN	Random allocation to BFST (similar to FBT) or EOIT	Outpatient	BFST, using principles of FBT 12-month duration	Achieve target weight	BMI, EAT, EDI	BFST produced greater rates of remission (13/19) than the EOIT (12/18) at EOT This difference was increased at 12-month follow-up (FBT: 15/19 vs EOIT: 12/18)	EOT: 1.08 (.27, 4.29) Follow-up: 1.86 (.43, 8.20)	Average (22)
Eisler et al. (2000)	40 adolescents (aged 11-17 years), DSM-IV diagnosis with AN	Random allocation to conjoint FT or separated FT, intention-to-treat analysis, independent assessors	Outpatient	Conjoint FT, using principles of FBT 12-month duration	End of treatment results	EDI, EAT, Morgan-Russell Scale	Separated FT produced greater rates of remission (10/21) compared with conjoint FT (5/19) at EOT	EOT: .39 (.10, 1.49)	Moderately poor (16)
Le Grange	18 adolescents	Random allocation to	Outpatient	Conjoint FT, using	Clinical feasibility	EBW, Morgan-	The groups did not differ in rates of	EOT: 1.00	Very poor (11)

et al. (1992)	(aged 12-17 years), DSM-III-R diagnosis of AN or BN, illness duration under three years	conjoint FT or family counselling, independent assessors		principles of FBT 32-week duration		Russell Scale, EAT, RSE	remission at EOT (Conjoint FT: 1/9 vs Family counselling: 1/9)	(.05, 18.92)	
Geist et al. (2000)	25 adolescents (aged 12-17 years), DSM-IV diagnosis of AN or partial AN	Random allocation to FT or family group psychoeducation	Inpatient	FT, using principals of FBT four-month duration (eight sessions)	EBW, psychological functioning, family functioning	EBW, Family Assessment Measure, EDI	Difference between groups for EBW, psychological functioning and family functioning was not significant. Remission not measured.		Moderately poor (15)
Ball & Mitchell. (2004)	25 adolescents (aged 13-23 years), DSM-IV diagnosis of AN, <90% expected weight-for-height	BFT compared with CBT	Outpatient	BFT, using principles of FBT 12-month duration (21-25 sessions)	Remission from AN (>85% EBW, good-intermediate outcome on the Morgan Russell Scale	Morgan-Russell Scale, EDE	No significant difference between groups at EOT (BFT: 7/12 vs CBT: 7/13). This was maintained at six-month follow-up.	EOT: 1.20 (.25, 5.84) Follow-up: 1.20 (.25, 5.84)	Average (21)

Russell et al. (1987)	21 adolescents (mean age 16.6 years ± 1.7), DSM-III diagnosis of AN or BN	Random allocation to FT or individual therapy in weight restored individuals.	Inpatient and outpatient	FT, using principles of FBT 12-month duration	Remission from AN (>85% EBW, good-intermediate outcome on the Morgan Russell Scale)	Morgan-Russell Scale	A significantly greater number of participants in the FT group were in remission (9/10) compared with individual therapy (2/11) at EOT	EOT: 40.50 (3.09, 530.29)	Average (27)
Lock et al. (2005)	86 adolescents (aged 12-18 years), DSM-IV diagnosis of AN	Random allocation to short-term or long-term FBT, intention-to-treat analysis, independent assessors	Outpatient	FBT-AN Six-month duration (10 sessions) or 12-month duration (20 sessions)	Change in BMI	BMI, EDE	No significant differences between the groups for BMI and EDE scores at EOT. Remission not measured.		Very good (34)
Eisler et al. (2016)	169 adolescents (aged 13-20 years), DSM-IV diagnosis of	RCT, Random allocation to FT-AN and MFT-AN, intent-to-treat analysis, blind	Across six NHS services in the UK	MFT, using principles of FT-AN	Healthy nutritional state (measured by the global outcome	Morgan-Russell Scale	FT-AN produced significantly fewer rates of remission (47/82) compared with MFT (64/85) EOT.	EOT: .44 (.23, .85)	Moderately good (31)

AN or partial AN, weight below 86% %mBMI or have lost 15% of body weight	independent assessors	12-month duration (10 sessions), involving five-seven families	scale on the Morgan Russell Scale)	This was maintained at 12-month follow-up: (FT-AN: 46/82 vs MFT: 66/85)	Follow-up: .37 (.19, .72)

ED, eating disorder; EDNOS, eating disorder not otherwise specified; SEED, short evaluation of eating disorders; EDE, eating disorder examination; FBT, family-based therapy; AFT, Adolescent-focused therapy; PFT, parent-focused treatment; CBT, cognitive behavioural therapy; AN, anorexia nervosa; BN, bulimia nervosa; EOT, end of treatment; EBW, expected body weight; BMI, body mass index; EDI, eating disorder inventory; BFST, behavioural family systems therapy; EOIT, ego orientated individual therapy; FT, family therapy; EAT, eating attitudes test; RSE, Rosenberg self-esteem scale; BFT, behavioural family therapy; DSM-IV, diagnostic statistical manual; SyFT, systemic family therapy**;** SPT, supportive psychotherapy; TSPE, therapy suitability and patient expectancy; FBT, family based treatment.

Meta-Analysis of ED-FT

ED-FT versus individual therapy. First, the difference in remission rates between ED-FT and individual therapy was meta-analysed (see Figure 2). An inclusive approach was taken by including all seven studies reporting data on remission rates in this analysis. The pooled Odds ratio effect was 2.04 (95% CI: 1.13, 3.66), indicating a significant effect in favour of ED-FT (p = .017). To test for heterogeneity, the Q-value was 9.90 (df = 6), which was not statistically significant (p = .129) and the I^2 was 39.39. These figures indicate small variance in effect sizes and support the inclusive approach taken in the selection of studies for analysis.

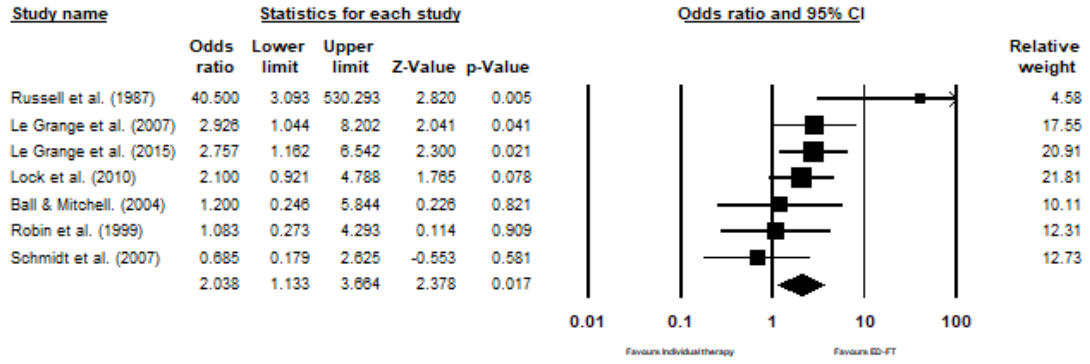

Figure 2. Forest plot depicting end of treatment outcomes for ED-FT and individual treatment.

The difference between ED-FT and individual therapy for remission was examined at follow-up. Six studies provided data for this, with Robin et al. (1999), Lock et al. (2010), and Le Grange et al. (2016) including data for 12-month follow-up, and Ball & Mitchell, 2004, Le Grange et al., 2007, Schmidt et al., 2007 conducted a six-month follow-up. The studies continued to produce odds ratio effect sizes, ranging between 1.20 (Ball & Mitchell, 2004), and 3.62 (Le Grange et al., 2007). The overall pooled effect size of 2.09 (95% CI: 1.37, 3.18) was significant (p < .001) indicating that the effect observed at EOT became stronger and therefore the patients ED symptoms

continued to improve (see Figure 3). This effect was not heterogeneous, as the Q-value was 1.73 (*df* = 5), which was not significant (*p* = .886), and the I^2 was .000.

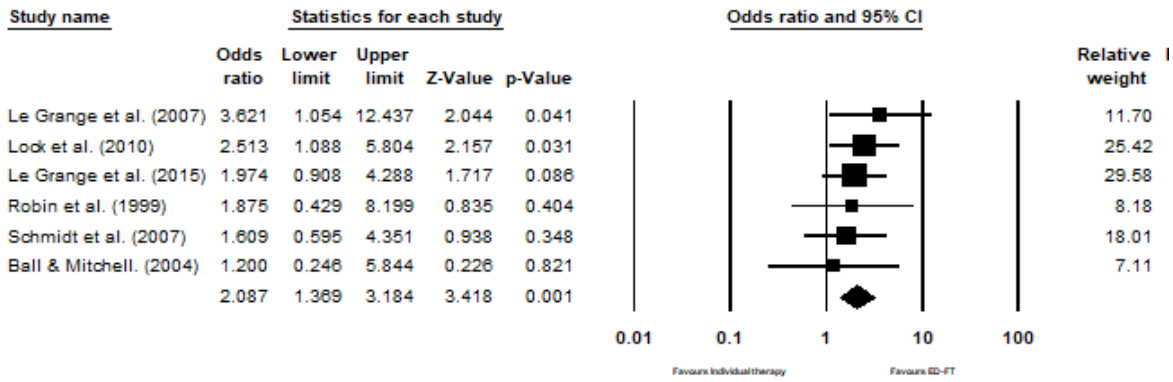

Figure 3. Treatment outcomes at follow-up for ED-FT and individual therapy

This finding is in line with Couturier et al. (2013), who also found that ED-FT produced a larger, more significant effect size at follow-up, compared with at EOT. It is possible that this effect is due to ED-FT ensuring that the family members become the 'therapist' and continue providing support to the young person once treatment has ended, compared with individual therapy as the external support reduces when treatment ends (Couturier et al., 2013).

Publication bias. The random-effects model 'trim and fill' method was utilised, to assess risk of bias across studies (Duval & Tweedie, 2000). The values did not change, indicating that no studies are missing as the funnel plot appeared symmetrical (see Figure 4). Rosenthal's (1979) Fail-safe N yielded a *z*-value of 3.29, with a two-tailed *p*-value of <.001. The fail-safe N was 13, meaning that 13 studies would be needed for the *p*-value to exceed .050.

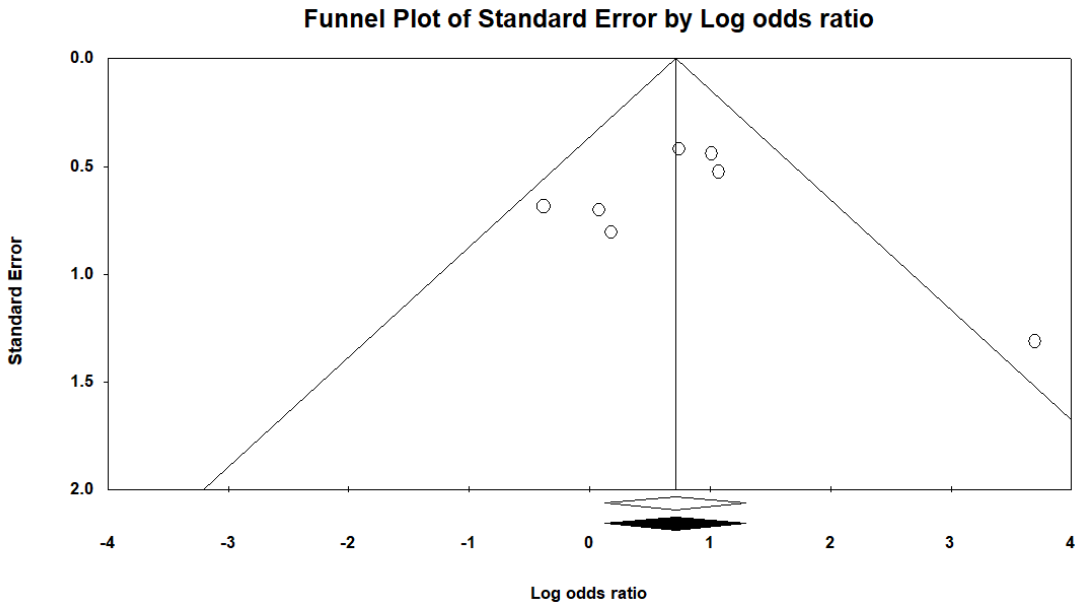

Figure 4. Funnel plot depicting low publication bias across studies

ED-FT versus other forms of FT. Finally, a meta-analysis was conducted, comparing ED-FT to other forms of FT. Six studies, including the recent RCT of EDT-FT versus MFT by Eisler et al. (2016) were included in the analysis. Again, an inclusive approach was taken and all the studies that used remission as a primary outcome measure were selected. This was chosen as it could be used as a direct comparison to the meta-analysis of ED-FT versus individual therapy. The analysis did not include data from Lock et al. (2005) and Geist et al. (2000) as they did not use remission as their primary outcome.

The results show that there was greater variability with this analysis (see Figure 5), as the individual effect sizes ranged from .37 (Le Grange et al., 2016) to 1.42 (Agras et al., 2014) indicating that ED-FT varied considerably in how successful it was compared to other forms of FT. At EOT, the random-effects model pooled Odds ratio was .64 (95% CI: .36, 1.12), which infers a non-significant effect (p = .117). This suggests that while the other FT interventions yielded a greater effect on rates of remission, the difference was not large enough to be significant.

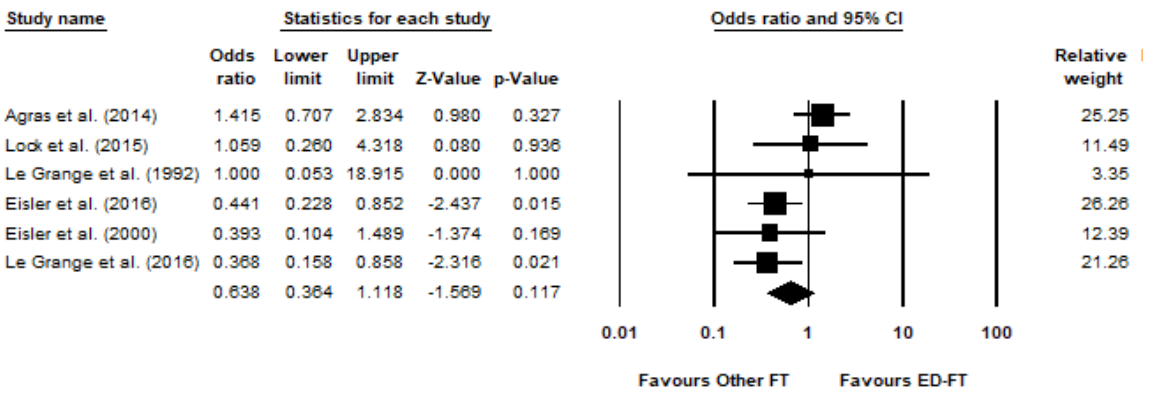

Figure 5. End of treatment outcome for ED-FT versus other forms of FT

The test for heterogeneity highlighted moderate heterogeneity, as the Q-value was 8.99 (*df* = 4), which was non-significant (*p* = .064), however the I^2 was 55.01.

Only three studies that compared ED-FT with other forms of FT included follow-up results within their papers (see Figure 6). This result was similar to the outcomes at EOT, as the pooled Odds ratio was .64 (95% CI: .34, 1.22) which was a non-significant effect (*p* = .174), in favour of the other forms of FT. This result should be interpreted with caution however, as the non-significant means that there was little difference between groups. Furthermore, the results were derived from only three studies.

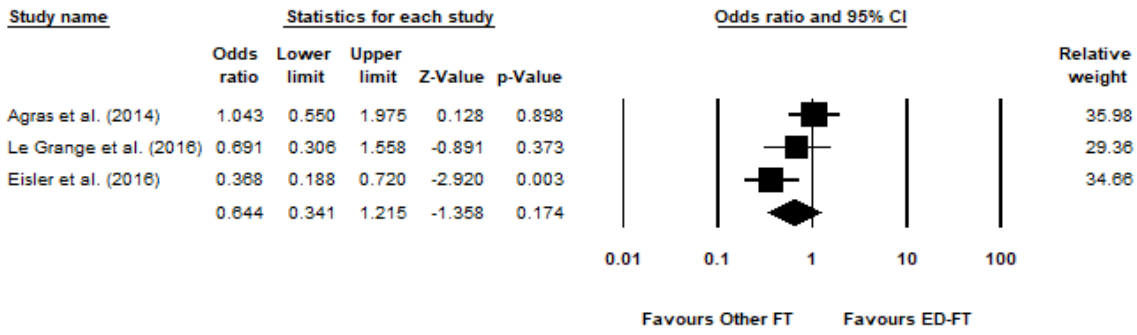

Figure 6. Forest plot of the effect of ED-FT and other FT on remission at follow-up

Again, heterogeneity was present, as the Q-value was 4.89 (*df* = 2) which was not significant (*p* = .087). The I^2 was 59.07, indicating that medium variance in the effect sizes (Borenstein et al., 2009). This may be explained by the range in forms of FT utilised as comparison groups.

Additional analyses. In order to further understand the mechanisms that might be underlying the variance, a meta-regression was conducted to examine the possible relationship between various moderators and rates of remission (Borenstein et al., 2009). The association between treatment dose and rates of remission was not significant (-.05, 95% CI: -.30, .19, p = .674). The relationship between study quality and rate of remission was also not significant (0.47, 95% CI: -2.29, 3.63, p = .772).

A sensitively test was also conducted in order to determine how robust the results were. For the studies that compared ED-FT to other forms of FT, the studies with the lowest quality ratings (poor) were removed from the analysis. This resulted in the inclusion of four studies in the analysis (Agras et al., 2014; Lock et al., 2015; Eisler et al., 2016; Le Grange et al., 2016). The pooled Odds ratio was 0.67 (95% CI: .45, .99), which just reached statistical significance (p = .048). The heterogeneity was still high as the Q-value was 8.34, which was significant (p = .039), and the I^2 was 64.03%. This analysis shows that while the inclusive approach taken in the earlier analysis resulted in less variance in scores, it produced a non-significant effect.

Part Two: Review of MFT

Excluded studies. In total, the search resulted in 26 studies. Nineteen of these did not meet inclusion criteria for the following reasons: the paper reported a single-case study (n = 1) or included adult participants (n = 1); the full-text was unavailable (n = 2); they were either comment papers or articles that did not report quantitative results (n = 11); the papers reported results from previous research (n = 2); or not published in English (n = 2).

Included studies. Seven studies investigating MFT in ED met inclusion criteria for review, totalling 378 participants. The methodological rigour was varied as five were pilot or outcome studies, one was an open trial, and one was a retrospective cohort

study. In regard to diagnosis, six studies included participants with a diagnosis of AN or partial AN, while three studies included participants with a diagnosis of BN.

Hollesen, Clausen, and Rokkedal (2013) published the first pilot study into MFT for AN. They conducted the pilot between 2007-2010, treating 32 families. They experienced high attrition rates, with 38.5% dropping out, leaving 20 families included in the analysis. They found that the majority (65%) of the participants who completed the intervention did not meet criteria for AN at EOT. This result was supported by Gelin et al. (2015), who found that 52.4% of participants (n = 61) achieved EBW above 85% at EOT. They had a higher retention, with 8.5% of participants dropping out. More recently, Salaminiou et al. (2017) conducted an open trial of 30 families who received intensive MFT for AN. They found a statistically significant increase in mean EBW from 75.8% at the start of treatment, to 86.1% at EOT (F = 26.8, p < .001). A retrospective case review of MFT by Gabel, Pinhas, Eisler, Katzman, and Heinmaa (2014) also found that patients enrolled in MFT restored weight to a higher EBW (M = 99.6%, SD = 7.27%) compared to controls (M = 95.4, SD = 6.88), which was statistically significant (p < .05).

One study did not find the same results. Depestele et al. (2017) conducted a pilot trial of 112 adolescents diagnosed with EDs in an inpatient unit. MFT or a multi-parent group (in which young people were not present) were offered as adjunctive to treatment. They did not find a significant difference between groups for ED symptoms. The fact that all participants received inpatient treatment, however, may have skewed the results.

Measure of remission. Every paper used changes in weight or ED psychopathology as their primary outcome measure. To assess this, they administered the Morgan-Russell scale, EDE, eating disorder examination questionnaire (EDE-Q; Fairburn & Beglin, 1994), or eating disorder inventory (EDI-2; Garner, 1991). See Table 2 for details and corresponding effect sizes.

Quality assessment. The quality of the studies did not vary to the same degree as the studies of ED-FT; however, the overall quality was substantially lower. With the exception of the RCT by Eisler et al. (2016), the studies were rated average to exceptionally poor, with the majority of studies rated moderately poor. This is unsurprising as the research is at an early stage and the methodological rigour of the studies in MFT is inferior compared with ED-FT. They also did not complete follow-up measures making it unclear whether any statistically significant findings were maintained over time.

Service context. The majority of the studies took place in outpatient settings of specialist ED services. Due to the methodology of the studies, the treatment was not delivered specifically for the research, but, rather, took place in clinical settings. Therefore, the therapists delivering the treatment were members of the MDT. Eisler et al. (2016) considered the impact this had on their findings. They discussed the importance of considering the service contexts in which the research has taken place, as the demographics of participants is different across cultures. Marzola et al. (2015) also discussed the importance of considering the impact of treatment fidelity on outcomes and that specialist ED services will be better resourced and have greater skills and experience to provide evidence-based treatments.

Treatment fidelity. None of the papers stated the degree of adherence to the treatment model. This may be because the studies took place in outpatient settings and therefore treatment was delivered within a regular environment.

Table 2. Studies included in review: MFT

Study	Sample	Methods	Setting	Treatment	Primary outcome	Primary outcome measure(s)	Findings	Hedges's g (95% CI)	Study quality rating (raw score)
Salaminiou et al, 2017	30 adolescents (mean age 15.4 years ± 1.8), with a diagnosis of AN or partial AN (diagnostic system unspecified)	Open trial of MFT	Outpatient	Intensive MFT using principles of FT-AN 9- month duration (10-12 sessions), involving five- seven families	Symptoms, psychological functioning and acceptability	%mBMI, Morgan-Russell Scale, EDI	Significant increase in %mBMI at six months (86.1% ± 8.7 compared with 75.8% ± 6.5 at the start of treatment); Significant improvement in ED psycho-pathology and 62.1% achieved a 'good/intermediate' outcome	.66 (.27, 1.06)	Very poor (10)
Gabel et al, 2014	50 adolescents (mean age 14.1 years ± 1.87), DSM-IV diagnosis of	Retro-spective cohort study of patients receiving either	The chart review was from an ED program at a Hospital in	MFT using principles of FT-AN	Weight restoration	%mBMI, EDI, EDE-Q	The MFT group had a significantly higher %mBMI at EOT (99.6% ± 7.27 compared with 78.4% ±	.40 (.00, .80)	Very poor (8)

	AN	MFT or T TAU	Toronto, Canada Outpatient				9.77). This was larger increase than the TAU group (95.4 ± 6.88 at EOT)		
Hollesen et al, 2013	20 adolescents (aged 12-18 years), diagnosed with AN or partial AN (Diagnostic system unspecified)	Pilot study of MFT	Outpatient	MFT using principles from the Dresden Model and Maudsley model 12- month duration (12 sessions)	ED symptoms and interpersonal functioning	EDE, EDI, inventory of inter-personal problems	BMI significantly increased at EOT (18.38 ± 1.36 compared with 16.21 ± 1.35). Post treatment, 13 (65%) no longer had an ED	1.27 (.69, 1.84)	Moderately poor (14)
Gelin et al, 2015	82 adolescents (aged 11-19 years), DSM-IV diagnosis of AN or BN	Outcome study	Outpatient	Maudsley-oriented MFT 11-month duration (21 sessions), involving	Weight changes, ED symptoms, quality of life	%EBW, EDI-2	52.4% achieved a %EBW above 85%, 24.4% of whom were >95%. Significant increase in %EBW was found at EOT (86.75 ± 11.20	.93 (.68, 1.20)	Moderately poor (17)

				five- six families			compared with 76.99 ± 9.75)		
Study	Sample	Methods	Setting	Treatment	Primary outcome	Primary outcome measure(s)	Findings	Hedges's g (95% CI)	Study quality rating (raw score)
Depestele et al, 2017	112 adolescents (aged 14-21 years), DSM-IV diagnosis of AN, partial AN, BN or partial BN	Pilot study Non-random assign-ment to MFT or MPT, intent-to-treat analysis	Inpatient ED unit in Belgium	MFT	ED symptoms, family functioning	EDI-2	ED symptoms improved in both groups however weight change not reported. There was a non-significant difference between groups in outcome scores post-intervention.	-	Moderately poor (17)
Marzola et al, 2015	74 adolescents (mean age 14.8 years ± 2.7), DSM-IV diagnosis of AN or partial AN	Retro-spective outcome study of M-IFT and S-IFT	Outpatient	M-IFT using principles from FT-AN	Full remission from AN (>95% EBW, mean global EDE score within one standard deviation)	EDE, EDE-Q	Over a 30-month follow-up, 60.8% of participants achieved full remission. Non-significant increase in &EBW found at EOT (97.85 ± 10.14 compared	.08 (-.19, .34)	Average (22)

							with 86.36 ± 8.74)		
Stewart et al, 2015	10 Adolescents (aged 13-18 years), diagnosed with BN (Diagnostic system unspecified)	Pilot study of MFT-BN	Outpatient	MFT-BN	ED symptoms, mood and coping.	EDE-Q, mood and feelings questionnaire, ways of coping checklist	Preliminary analysis indicated a non-significant reduction in ED symptoms, however weight not measured	-	Exception-ally poor (5)

EDI-2, eating disorder inventory; FT-AN, anorexia nervosa- focused family therapy; MFT, multifamily therapy; EDE, eating disorder examination; EDE-Q, eating disorder examination questionnaire; TAU, treatment as usual; M-IFT, multifamily intensive family therapy; S-IFT, single-family intensive family therapy; MPT, multi-parent therapy; ED, eating disorder; EBW, expected body weight; AN, anorexia nervosa; BN, bulimia nervosa; MFT-BN, bulimia nervosa- focused multifamily therapy; DSM-IV, diagnostic statistical manual

Meta-Analysis of MFT

A meta-analysis was conducted on the available data, in order to be able to begin quantifying the effectiveness of MFT. Due to the lack of control groups for the vast majority of the studies, it was not possible to conduct a comparison between treatment groups. Therefore, a within participants analysis was conducted, by examining the differences in weight pre-intervention and post-intervention. This measure was chosen as having an EBW >85% is part of the criteria for achieving good- intermediate outcomes on the Morgan Russell Scale (Eisler et al., 2016). As this was the most commonly used measure of remission in the ED-FT studies, using it to measure the effectiveness of MFT seemed an appropriate comparison.

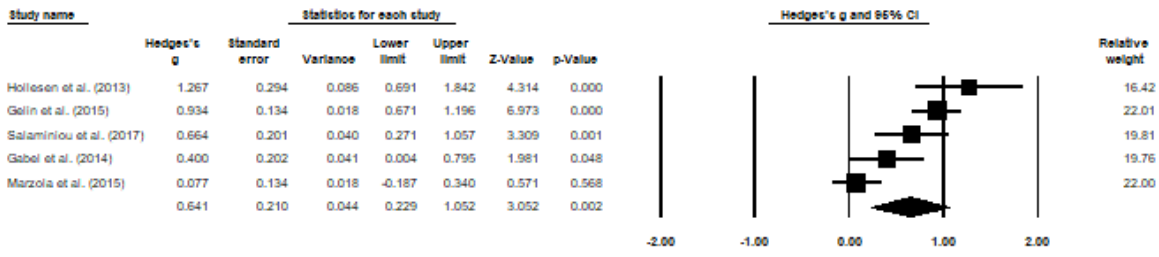

Figure 7. Forest plot of the effect of MFT on changes in EBW pre- and post-intervention

Five studies provided the mean weight pre- and post- MFT alongside either the paired samples t- value (Hollesen et al., 2013; Gelin et al., 2015) or the paired sample *p* value (Salaminiou et al., 2015; Gabel et al., 2014; Marzola et al., 2015). One study provided mean BMI measures (Hollesen et al., 2013), with the rest providing mean EBW scores. This information was used to compute the effect sizes, with Figure 7 showing that the meta-analysis produced a pooled effect of 0.64 (95% CI: .23, 1.05), which was a statistically significant medium effect ($p < .002$). This indicates that receiving MFT produces a significant effect on weight gain at EOT, particularly as all of the included studies found that the mean EBW was above the 85% threshold.

Although this indicates that MFT is an effective treatment for AN and BN, the results must be interpreted with caution as the studies did not include a control or comparison group, and therefore the causation of change cannot be confirmed.

The heterogeneity of the effect sizes was high, as shown by the Q- value being 27.42 ($df = 4$), and the I^2 being 85.4. This indicated that the high level of observed variance in the studies is likely due to real differences between effect sizes (Borenstein et al., 2009).

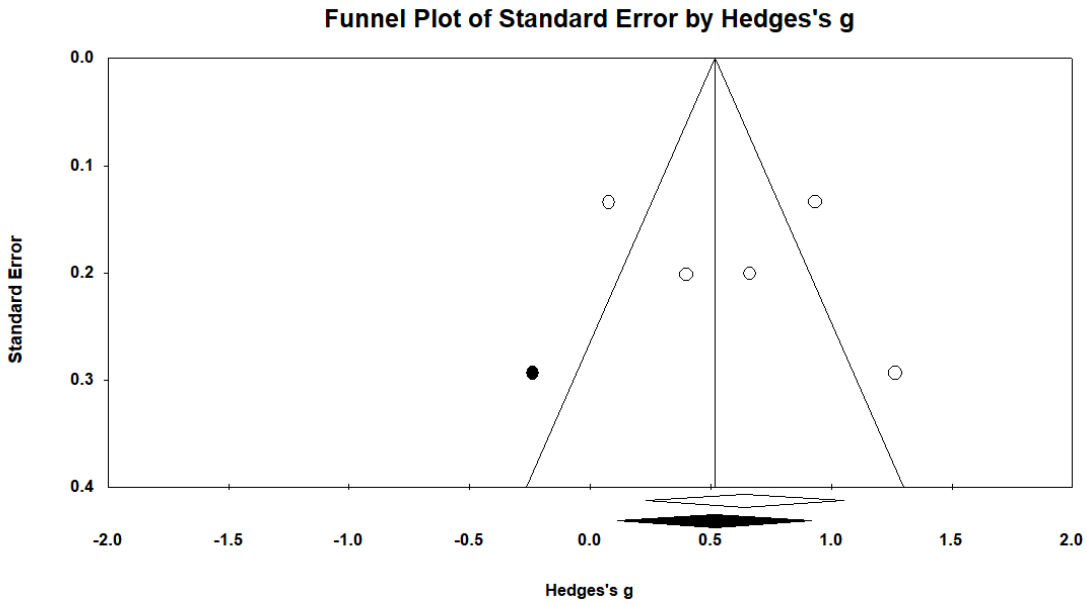

Figure 8. Funnel plot indicating publication bias for MFT studies.

Publication bias. A test for publication bias also highlighted a possible bias in reporting of successful outcomes. The Classic Fail-safe N yielded a z-value of 7.67 ($p <$ .001), indicating that 72 'null' studies would be needed in order for the combined two-tailed *p*-value to exceed .50. Duval and Tweedie's Trim and Fill method indicated that under the random effects model, the point estimate was .52 (95% CI: .23, 1.05), while Figure 8 shows that one study would need to be imputed to produce a symmetrical funnel plot.

Discussion

The current evidence-base for treating children and adolescents with EDs supports the use of family-based approaches compared with individual therapy (Couturier et al., 2013). This meta-analysis supported this argument by demonstrating that ED-FT was statistically significantly superior at achieving a greater rate of remission compared with a range of individual therapies, such as CBT and adolescent-focused therapy at EOT and follow-up.

While the efficacy of ED-FT is clear, the treatment model does not work for all families, as highlighted by the lack of 100% success-rate in remission in the included studies. This has led research to investigate whether the success of ED-FT is due to specific factors related to the models used, or whether more general FT factors are taking effect. This meta-analysis highlighted that there is considerable variation in outcomes between ED-FT and either other forms of FT, or adaptations to the model, and that the difference failed to reach statistical significance at EOT and follow-up. This result calls into question the efficacy of ED-FT over modifications to the model or other forms of FT, as it is currently unclear whether the success of ED-FT compared with individual therapy is due to the theoretical framework underpinning ED-FT, or the strength of implementing general FT principles.

The results from the meta-analysis comparing ED-FT with other forms of FT highlight firstly, that seeing the family separately seems to produce better outcomes (Le Grange et al., 2016; Eisler et al., 2000). Secondly, the results from the RCT by Eisler et al. (2016) showed that MFT produced an effect size of .44 at EOT (95% CI: .23, .85) which was significant ($p < .05$) and carried the largest weight of all the studies included in the analysis (26.4%). It also maintained the effect at follow-up, which further strengthens the result.

When synthesising the studies on MFT, the RCT-PQRS showed that the quality of the findings was weaker compared with the RCTs conducted for ED-FT. This is unsurprising, as the pilot studies for MFT were unable to replicate the robust methodology that RCTs encourage. The studies were also significantly different in the healthcare service contexts, as the studies often provided results on interventions that had taken place within therapeutic clinics. This is in contrast to the research into ED-FT, which was predominantly conducted by the developers of the ED-FT manuals and held within academic institutions. Therefore, it is possible that efficacy trials for ED-FT may have had more stringent recruitment processes and more stringent measures for treatment success.

The meta-analysis of MFT found that all the participants in the included studies experienced an increase in weight post-intervention and the pooled medium effect 0.64 (95% CI: .23, 1.05), which was a statistically significant ($p < .002$). This result must be interpreted with caution, however: firstly, the effect of weight gain from pre-to-post intervention is not as robust as the effect derived from the RCTs for ED-FT as a comparison group was not used; secondly, none of the studies provided follow-up data, and therefore it is difficult to know whether the effect was maintained. This is an important limitation as weight restoration over a period of time is more meaningful compared with a one-off time-point. Thirdly, the results of the quality tool highlighted the large discrepancy between ED-FT and MFT. The poor quality of MFT studies significantly reduces the credence that can be given to the findings, as they are at high risk of bias (Borenstein et al., 2009).

Strengths and Limitations

A primary strength of this review is that it is the first meta-analysis to compare ED-FT with other forms of FT. It is also the first meta-analysis to preliminarily analyse

the pre- and post-intervention data on MFT. This review adds to the literature by highlighting a number of new findings. Firstly, it has shown that there is variation in the efficacy of ED-FT when compared with other forms of FT or variations of the model. Secondly, it has provided tentative findings that indicate that participants that receive MFT experience statistically significant weight gain that is of an adequate level for weight restoration. This should be interpreted with caution as there was no comparison group, and the criteria for remission was particularly stringent. The review has shown that although MFT is recommended in NICE guidelines, the research in this area is still in its infancy and the quality of the studies is poor compared the RCTs on ED-FT.

A limitation of this meta-analysis is that not all of the studies included in the review were meta-analysed. This was due to the papers not providing the necessary data to meta-analyse. The authors of these papers were contacted; however, only one replied.

Some studies recruited participants with AN and/or BN, while other studies clearly favoured recruiting solely AN participants. Including participants with either diagnosis may be considered a strength as it can be seen to increase generalisability, as is commensurate with the transdiagnostic view of EDs (Fairburn, Cooper, & Shafran, 2003). However, it can also be seen as a limitation, due to the differing psychopathological presentations and treatment goals, such as weight gain (NICE, 2017). Furthermore, some studies failed to report the diagnostic system that was used in the study, and due to the range in dates in which the studies took place, there was a range in diagnostic systems used.

The review highlights the need for consideration of local service contexts when considering the findings. A number of the studies presented in this article took place in research centres that are renowned for research into ED-FT for children and adolescents (Eisler et al., 2016; Le Grange et al., 2015; Lock et al., 2010; Schmidt et al., 2007). This may confound and inflate the results of ED-FT as these centres may have greater access

to training, include more experienced clinicians in their treatment trials, and may be more likely to not only follow the manualised treatment more closely but also adhere to higher treatment fidelity.

Finally, adherence to the manuals has not been the focus of the literature to date. This is an important limitation, as it further compounds the impact of local service contexts on the findings. Without explicit adherence to treatment fidelity, it is possible that the models are being delivered to varying degrees. If so, this makes the findings difficult to replicate and generalise, as the reason for treatment success may be difficult to determine.

Recommendations

Future research would benefit from identifying the familial factors that may predict treatment success, and whether relevant adaptions to the model could be made for those families for whom the treatment does not work. Research into manual fidelity and the relationship between therapist and service contexts on treatment success may help us further understand the factors that contribute towards treatment success.

This meta-analysis has shown the need to conduct further research into the mechanisms of ED-FT that contribute towards its success. It has highlighted that when ED-FT is delivered in an MFT framework, participants not only gain appropriate levels of weight, but they also achieve better outcomes for overall remission, at least in the short-term at EOT, compared to when ED-FT is delivered to individual families. As only one RCT of MFT has been completed to date, it is important future studies replicate the study with more participants in order to consolidate the research findings. Therefore, more robust evidence is needed to support the use of MFT as although the findings have indicated promising outcomes, more RCTs are needed in order to ascertain the treatment effects against a control or comparison group.

There is a clear trend towards researching AN over BN, with more RCTs having been conducted on the former. Three studies (Depestele et al, 2017; Stewart et al., 2015; Gelin et al, 2015) investigated the use of MFT with this population; however, the research is still in its infancy and no RCT for MFT for BN has been conducted to date. Therefore, higher quality future research would benefit from further exploring whether young people with a diagnosis of BN benefit as much from MFT compared to those with a diagnosis of AN.

The meta-analysis also highlighted the need for research that specifically investigates the components of ED-FT and FT principles. The focus of future research should be on fine-tuning our understanding of exactly what mechanisms are responsible for the strength of the treatment model. Having a greater understanding of this will enable the development of more robust versions of the current treatment protocol. It is hoped that this will lead to a greater number of families experiencing a successful outcome and fewer young people entering adulthood with a chronic illness.

Clinical Implications

This is the first meta-analysis to indicate that the effects of ED-FT are more variable when compared to variations of ED-FT, such as providing a short form intervention over six months or delivering ED-FT in MFT format. This suggests that services may need to consider the benefit of tailoring their treatment plans to suit the needs of the individual. In particular, offering separate sessions for the parents and child may be helpful.

The use of remission within these studies is a particularly stringent measure, as it was often defined as an EBW of above 95% and cognitive recovery as measured by changes in the global score of the eating disorder examination (EDE) within one standard deviation (SD) of community norms. Clinical services may find that they are

not seeing the same number of patients improve as seen in these studies if they use the same definition of treatment success. Furthermore, this definition does not account for other aspects of change, such as a reduction in physical risk and parental expressed emotion (Le Grange, Hoste, Lock, & Bryson, 2012; Royal College of Psychiatrists, 2015).

Finally, the results of the meta-analysis on MFT indicate that this treatment may be an effective treatment that further reduces ED symptoms. If this method of delivery is successful, it may provide a boost in treatment intensity (Jewell et al., 2016). Delivering ED-FT in this format may increase the strength and confidence of parents and families to challenge the ED, by allowing them to share their experiences with each other (Jewell et al., 2016). It may also be a feasible and cost-effective format for treating multiple families, as it could decrease pressure on clinical services by reducing treatment waiting times, if more than one family is treated at a time. This requires formal testing.

Conclusion

The variation in effect of ED-FT when it is compared with other forms of FT or modifications to the model highlights the need for more research in the area to better understand the mechanisms that make this treatment model successful. In particular research in to factors that may moderate treatment success, such as seeing parents separately, using groups such as MFT and treatment fidelity may be useful.

The preliminary literature on MFT is promising as the majority of studies reported statistically significant improvement in weight post-treatment. However, the methodological quality of the studies is poor. Therefore, the number and quality of future studies needs to increase in order for firm conclusions to be made about the effectiveness of delivering ED-FT in MDT format.

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Conflict of Interest

None.

Declarations of Interest

None.

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Appendix B

RCT of Psychotherapy Quality Rating Scale (RCT-PQRS)

Description of subjects

Item 1. Diagnostic method and criteria for inclusion and exclusion

0	poor description and inappropriate method/criteria
1	full description or appropriate method/criteria
2	full description and appropriate method/criteria

Item 2. Documentation or demonstration of reliability of diagnostic methodology

0	poor or no reliability documentation
1	brief reliability documentation (documentation in the literature is sufficient, even if it is not explicitly cited)
2	full reliability documentation (documentation of within-study reliability necessary)

Item 3. Description of relevant comorbidities

0	poor or no description of relevant comorbidities
1	brief description of relevant comorbidities
2	full description of relevant comorbidities

Item 4. Description of numbers of subjects screened, included, and excluded

0	poor or no description of numbers screened, included, and excluded
1	brief description of numbers screened, included, and excluded
2	full description of numbers screened, included, and excluded

Definition and delivery of treatment

Item 5. Treatment(s) (including control/comparison groups) are sufficiently described or referenced to allow for replication

0	poor or no treatment description or references
1	brief treatment description or references (also if full description of one group and poor description of another)
2	full treatment description or references (manual not required)

Item 6. Method to demonstrate that treatment being studied is treatment being delivered (only satisfied by supervision if transcripts or tapes are explicitly reviewed)

0	poor or no adherence reporting
1	brief adherence reporting with standardized measure or full adherence reporting with non-standardized measure (eg, non-independent rater)
2	full adherence reporting with standardized measure (must be quantitative and completed by an independent rater)

Item 7. Therapist training and level of experience in the treatment(s) under investigation

0	poor description and underqualified therapists
1	full description or well-qualified therapists
2	full description and well-qualified therapists

Item 8. Therapist supervision while treatment is being provided

0	poor description and inadequate therapist supervision
1	full description or adequate therapist supervision
2	full description and adequate therapist supervision

Item 9. Description of concurrent treatments (eg, medication) allowed and administered during course of study (if patients on medication are included, a rating of 2 requires full reporting of what medications were used; if patients on medications are excluded, this alone is sufficient for a rating of 2).

0	poor or no description of concurrent treatments
1	brief description of concurrent treatments
2	full description of concurrent treatments

Outcome measures

Item 10. Validated outcome measure(s) (either established or newly standardized)

0	poor or no validation of outcome measure(s)
1	brief validation of outcome measure(s) (shown or cited)
2	full validation of outcome measure(s) (shown or cited)

Item 11. Primary outcome measure(s) specified in advance (although does not need to be stated explicitly for a rating of 2)

0	poor or no specification of primary outcome measure(s) in advance
1	brief specification of primary outcome measure(s) in advance
2	full specification of primary outcome measure(s) in advance

Item 12. Outcome assessment by raters blinded to treatment group and with established reliability

0	poor or no blinding of raters to treatment group (eg, rating by therapist, non-blind independent rater, or patient self-report) and reliability not reported
1	blinding of independent raters to treatment group or established reliability
2	blinding of independent raters to treatment group and established reliability

Item 13. Discussion of safety and adverse events during study treatment(s)?

0	poor or no discussion of safety and adverse events
1	brief discussion of safety and adverse events
2	full discussion of safety and adverse events

Item 14. Assessment of long-term posttermination outcome (should not be penalized for failure to follow comparison group if this is a wait- list or nontreatment group that is subsequently referred for active treatment)

0	poor or no posttermination assessment of outcome
1	medium-term assessment of posttermination outcome (2-12 months posttermination)
2	long-term assessment of posttermination outcome (≥12 months posttermination)

Data analysis

Item 15. Intent-to-treat method for data analysis involving primary outcome measure

0	no description or no intent-to-treat analysis with primary outcome measure
1	partial intent-to-treat analysis with primary outcome measure
2	full intent-to-treat analysis with primary outcome measure

Item 16. Description of dropouts and withdrawals

0	poor or no description of dropouts and withdrawals
1	brief description of dropouts and withdrawals
2	full description of dropouts and withdrawals (must be explicitly stated and include reasons for dropouts and withdrawals)

Item 17. Appropriate statistical tests (eg, use of Bonferroni correction, longitudinal data analysis, adjustment only for a priori identified confounders)

0	inappropriate statistics, extensive data dredging, or no information about appropriateness of statistics
1	moderately appropriate, though unsophisticated, statistics and/or moderate data dredging
2	fully appropriate statistics and minimal data dredging in primary findings

Item 18. Adequate sample size

0	inadequate justification and inadequate sample size
1	adequate justification or adequate sample size
2	adequate justification and adequate sample size

Item 19. Appropriate consideration of therapist and site effects

0	therapist and site effects not discussed or considered
1	therapist and site effects discussed or considered statistically
2	therapist and site effects discussed and considered statistically

Treatment assignment

Item 20. A priori relevant hypotheses that justify comparison group(s)

0	poor or no justification of comparison group(s)
1	brief or incomplete justification of comparison group(s)
2	full justification of comparison group(s)

Item 21. Comparison group(s) from same population and time frame as experimental group

0	comparison group(s) from significantly different population and/or time frame
1	comparison group(s) from moderately different population and/or time frame
2	comparison group(s) from same population and time frame

Item 22. Randomized assignment to treatment groups

0	poor (eg, pseudo-randomization, sequential assignment) or no randomization
1	adequate but poorly defined randomization procedure
2	full and appropriate method of randomization performed after screening and baseline assessment

Overall quality of study

Item 23. Balance of allegiance to types of treatment by practitioners

0	no information or poor balance of allegiance to treatments by study therapists (eg, therapy in experimental and control groups both administered by therapists with strong allegiance to therapy being tested in the experimental group)
1	some balance of allegiance to treatments by study therapists
2	full balance of allegiance to treatments (eg, therapies administered by therapists with allegiance to respective techniques)

Item 24. Conclusions of study justified by sample, measures, and data analysis, as presented (note: useful to look at conclusions as stated in study abstract)

0	poor or no justification of conclusions from results as presented or insufficient information to evaluate (eg, sample or treatment insufficiently documented, data analysis does not support conclusions, or numbers of withdrawals or dropouts makes findings unsupportable)
1	some conclusions of study justified or partial information presented to evaluate
2	all conclusions of study justified and complete information presented to evaluate

Item 25. Omnibus rating: please provide an overall rating of the quality of the study, taking into account the adequacy of description, the quality of study design, data analysis, and justification of conclusions.

24 items in total/score range 0-48

1 = exceptionally poor (0-5)
2 = very poor (6-12)
3 = moderately poor (13-19)

4 = average (20-27)
5 = moderately good (28-33)
6 = very good (34-41)
7 = exceptionally good (42-48)

Chapter Four. Extended Methodology and results

Word count: 3348

Chapter Four. Extended Methodology and Results

This chapter expands upon the methodologies and analyses chosen for both the meta-analysis on attentional biases (ABs) in binge eating disorder (BED), and the meta-analysis on eating disorder-focused family therapy (ED-FT) and multifamily therapy (MFT). It aims both to explain the rationale for why these approaches were undertaken when analysing the data and discuss alternative approaches that could have been used. It also presents additional analyses that were conducted for the meta-analysis of ABs in BED, which were not included in the paper. Comprehensive meta-analysis was used for all statistical analyses (version 3; Borenstein, Hedges, Higgins & Rothstein, 2005).

Assessing Quality and Risk of Bias within Studies

Quality assessment tools were utilised in order to assess the quality of the studies included in the meta-analyses. Assessing the quality of studies is different to assessing the risk of bias. This is because the former measures how well a study was designed and conducted (Cuijpers, 2016), while the latter measures the degree to which a study may be flawed in the design, potentially inflating the results. Therefore, careful consideration needs to be made when deciding which tool to use. Historically, the approach towards measuring the quality of the studies in systematic reviews has been either inconsistent or incomplete. This step is now considered an important aspect of the meta-analysis because it enables the researcher to infer how closely researchers adhered to factors that are known to contribute towards bias, such as issues with recruitment, blinding, randomisation and intent-to-treat analysis (Borenstein, Hedges, Higgins, & Rothstein, 2009).

Using a validated measure to assess the quality and risk of bias within studies increases the robustness of a systematic review or meta-analysis, as it reduces subjective

speculation regarding the studies (Greco, Zangrillo, Biondi-Zoccai & Landoni, 2013). Using a standardised tool also increases the strength of the conclusions drawn from a systematic review or meta-analysis, as the inclusion of low-quality studies or studies that have used methodological approaches that make them vulnerable to bias can significantly impact the treatment effect and the robustness of the conclusions being drawn from the findings (Hartling, Dryden &Klassen, 2009).

Meta-analysis of AB in BED. For the meta-analysis on attentional biases (AB) in binge eating disorder (BED), the RCT- PQRS (Kocsis et al., 2010) was adapted (see Appendix A for the original tool). Items from the quality assessment tool for the observational cohort and cross-section studies (National Heart Lung and Blood Institute, 2014), and Cochrane risk of bias tool (Higgins & Green, 2011) were integrated (see Appendix B and C respectively). For example, risk of bias due to confounding was used from the Cochrane risk of bias tool (Higgins & Green, 2011) to inform item nine on the adapted tool. Items three and six from the assessment tool for the observational cohort and cross-section studies (National Heart Lunch and Blood Institute, 2014) were included in the adapted tool. This was done because an appropriate tool for cross-sectional experimental designs was not available, and the research literature on quality tools for experimental designs is underdeveloped (Jarde, Losilla, Vives, & Rodrigo, 2013). A decision was made not to use the Newcastle Ottawa Scale for cohort studies (Wells et al., 2013) because it has been shown to have low reliability (Hartling et al., 2013). Because a validated tool was not used to assess the quality of the studies included in the MA on AB in BED, the adapted tool was piloted to increase its reliability and construct validity (Morgado, Meireles, Neves, Amaral, & Ferreira, 2017; Lo, Mertz, & Loeb, 2014).

The first stage of the pilot involved seeking supervision from the supervisory team on a draft version of the tool. The tool was then amended following discussions

and feedback. In particular, advice was given to change the wording of two items (items seven and eight) in order to make them less specific to the meta-analysis topic, and more generalisable. This was done to make the adapted tool more broadly useable by other studies. Once the items were agreed, they were piloted by a trainee in the cohort using two randomly selected papers from the meta-analysis. These papers were scored by the primary author and the trainee, resulting in a high level of agreement on scoring for both papers. The inter-rater agreement was quantitatively measured and Cohen's kappa (Landis & Koch, 1977) was .78, with an agreement score of 87.5%, indicating almost perfect agreement (Landis & Koch, 1977). The wording on item 10 was also amended following feedback to increase the clarity. The quality tool was then used by the primary author to assess the quality of all of the included papers in the meta-analysis (see Appendix D for the adapted quality assessment tool). The secondary author then rated four randomly selected papers. The Cohen's kappa score of inter-rater reliability was calculated to be .52 with an agreement score of 68.8%, which indicated moderate agreement (Landis & Koch, 1977).

While the adaption of the quality tool aimed to follow a scientific method, there are limitations of not using a tool that has been shown to be reliable and externally valid. However, due to the limited choice in suitable and validated tools for experimental designs, this method of quality assessment was considered appropriate.

Meta-analysis of ED-FT and MFT. For the meta-analysis of eating disorder-focused family therapy (ED-FT) and multifamily therapy (MFT) the randomised control trial of Psychotherapy Quality Rating Scale (RCT-PQRS; Kocsis et al., 2010) was used. This tool contains 24 items, which each assesses a different aspect of study quality. Each item is assigned a value of between zero and two, and the total score is the sum of the scores assigned to each item, totalling a maximum of 48. The total score is then assigned a qualitative rating, of which there are seven, ranging from 'exceptionally

poor' to 'exceptionally good'. The quality tool has been shown to have good internal consistency and reliability (Gerber et al., 2011), and external validity (Koscis et al., 2010).

Assessing Risk of Bias Between Studies

The studies in both meta-analyses were also assessed for possible publication bias. This is important as publications that show a large and significant effect in the preferred direction (such as interventions) are often favoured (Cujipers, 2016). Therefore, there is a risk that studies indicating a small or non-significant effect may not be published. This has an impact on meta-analyses, as the aim is to estimate the true or mean effect of a phenomenon; however, there is a risk of over-estimation of the true effect, if the studies included are biased (Cujipers, 2016).

In order to test for publication bias, the Fail-safe N (Rosenthal, 1979) was consulted. This method indicates the number of studies that would be needed in order to nullify the effect. Rosenthal (1979) suggests that if this number is small, then this would call into question the robustness of the results (Borenstein et al., 2009). The funnel plot was visually inspected, and the 'trim and fill' method (Duval & Tweedie, 2000) was applied. This estimates the number of studies that are missing from the funnel plot (Cujipers, 2016).

Effect Sizes

Meta-analysis of AB in BED. For the meta-analysis on attentional biases (AB) in binge eating disorder (BED), the mean reaction time (RT) and standard deviation (SD) scores were extracted from the included studies, which were continuous variables.

The standardised mean difference was then calculated from these values. This type of effect size is used when studies use different outcome measures (Borenstein et

al., 2009). The standardised mean difference is calculated by dividing the mean difference from each study by the study's standard deviation (Borenstein et al., 2009). This creates an index that can be compared across all studies, regardless of the different outcomes used to measure the same effect. Hedges's *g* (Hedges, 1981) and Cohen's *d* (Cohen, 1988) are two forms of standardised mean difference effect sizes. Particularly pertinent to this paper was the fact that the RT data were collated from different paradigms and therefore Hedges's *g* accounted for this by transforming the scores to a common value (Borenstein et al., 2009).

The reason why Hedges's *g* was used in the meta-analysis over Cohen's *d* is because Cohen's *d* does not account for small samples and, as such, is argued to overestimate the standardised mean difference in small samples (Borenstein et al., 2009). On the other hand, Hedges's *g* adjusts for possible biases that may impact on heterogeneity, such as by providing an 'unbiased' corrected effect size which takes into account small samples when providing an estimate of the pooled effect size. It is also thought to be more accurate when assessing the relationship between two groups (Borenstein et al., 2009).

To add further strength to the results meta-analysis on AB in BED, the difference between means and pooled standard deviation was calculated, to create an AB score. This analysis was used to compute the Hedges's *g* effect sizes. This not only ensured greater standardisation of scores across paradigms by reducing the variation in what the scores mean, it also provides results that can be directly compared to other meta-analyses of AB in AN and BN (Brooks, Prince, Stahl, Campbell, & Treasure, 2011).

Meta-analysis of ED-FT and MFT. The meta-analysis of ED-FT and MFT was divided into two sections, with each form of therapy being meta-analysed separately. When collating the relevant data for analysing ED-FT, it was noted that the results

reported by the studies were binary, such as rates of remission. As reported in the paper, the decision was made to use the Odds ratio (OR) when reporting the effect sizes over the relative risk (RR) or risk difference (RD). This was because the OR is less sensitive to baseline events (Cujipers, 2016) compared with the RD and the OR accounts for risk of events when they are low (Borenstein et al., 2009). In order to report the OR, the raw data items were log transformed (Borenstein et al., 2009). This means that the log OR and standard error of the log OR were calculated and these were used to conduct the meta-analysis. At this stage, it is considered important for the treatment group or control group to consistently be placed as the numerator or denominator, to ensure that the direction of the effect is consistent across studies (Borenstein et al., 2009). The ED-FT group were consistently inputted first (as the numerator) and therefore, a positive effect (above zero) inferred a greater effect for the ED-FT group and therefore a greater rate of remission. A negative effect (below zero) denoted that the individual treatment or other forms of family therapy (FT) yielded a greater rate of remission. The scores were then converted back to ratio values when reporting the results.

When analysing the data for MFT, the OR was not appropriate as the studies did not use dichotomous variables, such as reporting the number of patients who met criteria for remission (Cujipers, 2016). Instead, they used continuous variables when reporting their outcomes, such as mean changes in weight from pre-intervention to post-intervention. This was reported as either expected body weight (EBW) or body mass index (BMI). Because the majority of studies did not use a control or comparison group, a within-participants analysis was conducted. This was achieved by collating data from the paired-samples *t*-tests that the studies conducted and using either the reported *t*-value or *p*-value (Cujipers, 2016), depending on what information was reported. Hedges's *g* was chosen for the reasons stated above. In this case, using Hedges's *g* allowed the different measurements used to assess changes in weight (EBW and BMI)

to be transformed to a common value, allowing all studies that reported the mean changes to be included in the analysis.

Models

A random-effects model was chosen when reporting both meta-analyses, rather than a fixed-effects model. The fixed-effects model assumes that there is 'one true effect size' (Borenstein et al., 2009), which means that all studies share the same effect and any dispersion in effects are thought to be due to sampling error. With this model, the pooled effect size represents the common effect size (Borenstein et al., 2009). On the other hand, the random-effects model assumes that studies will range in effect and this deviation can be due to sampling error, but also due to random variation as each study is different and therefore the effect size will be influenced by this (Cujipers, 2016). Here, the pooled effect denotes the estimate of the mean distribution of effects (Borenstein et al., 2009). Therefore, random-effects models allow for greater variability within studies. This was considered important for both meta-analyses, as the studies in both papers ranged considerably both in terms of participants used and in methodology. For example, the meta-analysis on AB in BED included studies from clinical and non-clinical populations.

For both meta-analyses, the random-effects 95% confidence interval was reported alongside the effect sizes. This depicts how precise the effect size is, as the larger the confidence interval, the less precise the estimates (Borenstein et al., 2009). The 95% confidence interval can be affected by many factors, namely sample size and the methodology used (Borenstein et al., 2009). Therefore, the studies that had smaller confidence intervals (and therefore more precise estimates) were given more weight in the meta-analysis (Borenstein et al., 2009).

Heterogeneity

Heterogeneity refers to the variation in the 'true' effect sizes (Borenstein et al., 2009). However, part of the observed variation is due to true variation in effect sizes as well as random error. It is therefore referred to as 'spurious' (Borenstein et al., 2009). The Q-statistic (*Q*) is a standardised measure used to isolate the true variance. When used with the reported degrees of freedom (*df*), the figure illustrates whether the level of heterogeneity is statistically significant. If it is, it can be concluded that the effect sizes are heterogeneous and do not share a common effect (Borenstein et al., 2009). However, *Q* is sensitive to factors such as the number of studies. This means that a non-significant result may indicate too little power to detect a trend, rather than a homogeneous result (Borenstein et al., 2009). It also does not explain the extent of the variability. The I-squared statistic (I^2) provides a ratio of true heterogeneity to total variance of the observed estimates (Borenstein et al., 2009). It provides the degree of overlap of the confidence intervals and reports this as a ratio ranging from 0-100% (Borenstein et al., 2009). Specifically, a ratio of 25% signifies small heterogeneity, 50% signifies medium heterogeneity, and 75% signifies large heterogeneity (Borenstein et al., 2009). As described in the meta-analyses, the larger the percentage, the greater the extent of the heterogeneity. I^2 was given more credence in the meta-analyses as it is not affected by the number of studies included in the analyses (Borenstein et al., 2009).

Meta-Regression

Meta-regressions are often conducted when the results of a meta-analysis indicate heterogeneity (Cuijper, 2016). Much like a regression, a meta-regression calculates the relationship between certain moderators and the outcome of interest. A general rule of thumb for meta-regression is that for every co-variate, there should be 10 studies (Borenstein et al., 2009). It is therefore inadvisable to conduct meta-regressions

with multiple variables when the number of studies being analysed is small. With this in mind, the decision was made not to conduct a meta-regression for the meta-analysis of ABs in BED, as the tests for heterogeneity integrated low variance between scores, and the number of studies included in the analysis was small. A meta-regression was conducted on the data included in the meta-analysis of ED-FT and MFT, as the analysis highlighted a high degree of variation between results. It therefore felt important to explore what variables might be moderating the results. In particular, study quality and treatment dose were chosen as variables of interest. The study quality referred to the quality rating that was derived from the RCT-PRQS (Kocsis et al., 2010). This was considered an important potential moderator as research has shown that inadequate methodological approaches, such as poor allocation concealment and lack of blinding, can impact treatment effects by exaggerating the results (Hartling et al., 2009). The treatment dose referred to the length of treatment and was also considered an important variable, as the length of treatment may confound the results if some studies offer more sessions to their clients compared with others.

Additional Results from the Meta-Analysis in Chapter One

A within-group analysis of the BED group was conducted in order to ascertain whether the BED group exhibit greater AB towards food stimuli, compared with non-food stimuli. A negative effect size denotes a greater effect for food stimuli. Five studies were included in the analysis (Lee et al., 2017; Schmitz et al., 2014; Shank et al., 2015; Schmidt et al., 2016; Sperling et al., 2017), as these were the only studies that supplied mean RT and SD for food and non-food stimuli.

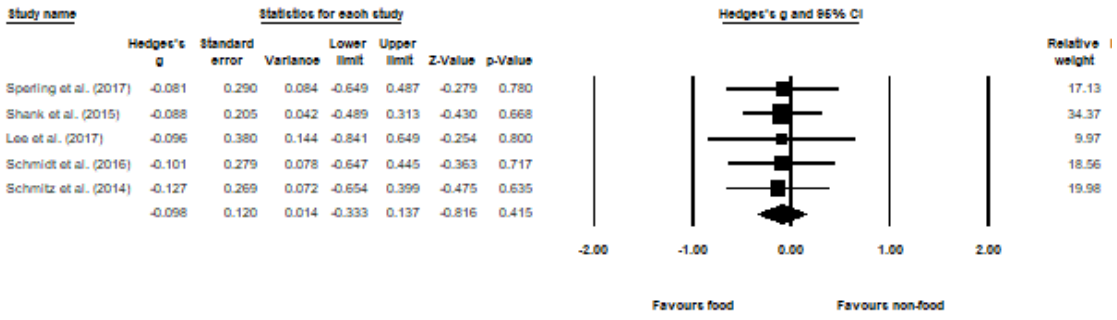

Figure 1. Forrest plot of the difference in RT towards food stimuli compared with non-food stimuli for the BED group.

Every study included in the analysis found that people in the BED group responded faster to food stimuli than non-food stimuli, indicating an AB towards disorder-salient cues. However, the pooled Hedges's *g* was -.10 (95% CI: -.33, .14), indicating a non-significant effect (p = .415). The overall effect size is very small, indicating that there was very little difference between RT scores for food and non-food stimuli (see Figure 1).

This analysis was repeated for the control group, in order to investigate whether the BED group or control group had greater effect sizes for food and non-food stimuli (see Figure 2). Again, the pooled Hedges's *g* was .11 (95% CI: -.14, .36), indicating a non-significant minimal effect size (p = .388).

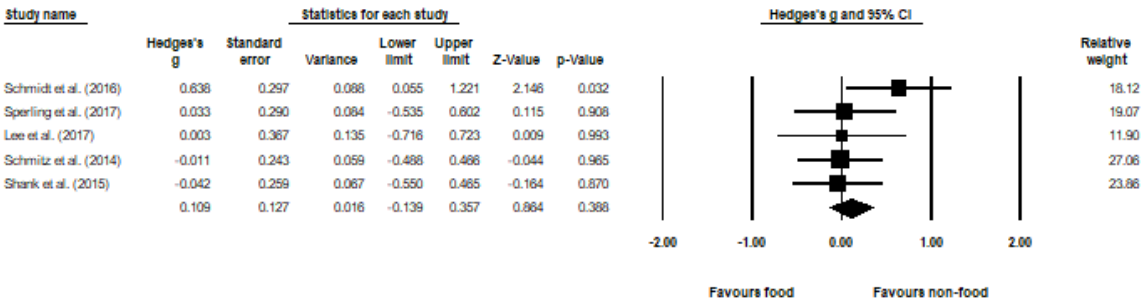

Figure 2. Forest plot of the difference in RT towards food stimuli compared with non-food stimuli for the control group.

Additional Results from the Meta-Analysis in Chapter Three

The numbers needed-to-treat (NNT) were calculated from results of the RCTs of ED-FT. This was conducted in order to determine how many patients would need to be treated in order to see one successful outcome (Kraemer & Kupfer, 2006). This is a

helpful calculation, as the effect sizes alone are difficult to translate the meaning back into real-world settings (Cuijpers, 2016).

The NNT is the inverse of the absolute risk reduction between two conditions, such as the control event rate (CER) and the experimental event rate (EER; Cuijpers, 2016). There are different formulas available to calculate the NNT value, however the following formula was used: 1/(CER-EER).

The NNT value ranges from one- to- infinity, with a smaller number indicating fewer people needing to be treated in order for a successful outcome to be found (Kraemer & Kupfer, 2006). Tables 1 and 2 show a considerable range in the number of cases needed to find a successful outcome. Interestingly, the majority of the studies that were deemed as being the highest quality (Le Grange et al., 2007; Le Grange et al., 2016; Lock et al., 2010; Le Grange, 2015; Le Grange et al., 2016) all had the smallest NNT.

Table 1. NNT for ED-FT versus individual treatment at end of treatment

Study	Number of events for ED-FT	Number of events for individual therapy	Absolute risk reduction (%)	Numbers needed to treat (NNT)
Le Grange et al., 2007	16/41	7/39	21.1	5
Lock et al., 2010	21/61	12/60	14.4	7
Schmidt et al., 2007	4/41	6/44	3.8	26
Ball & Mitchell, 2004	7/12	7/13	4.5	22

Robin et al., 1987	13/19	12/18	1.7	59
Russell et al., 1987	9/10	2/11	51	2
Le Grange et al., 2015	20/51	11/58	20.2	5

Table 2. NNT for ED-FT versus other forms of FT at end of treatment

Study (n)	Number of events for ED-FT	Number of events for other FT	Absolute risk reduction (%)	Numbers needed to treat (NNT)
Le Grange et al., 2016	12/55	22/55	18.2	5
Agras et al., 2014	25/78	20/80	7.1	14
Lock et al., 2015	5/10	17/35	1.4	71
Eisler et al., 2000	5/19	10/21	21.3	5
Le Grange et al., 1992	1/9	1/9	0.0	-
Eisler et al., 2016	47/82	64/85	18	6

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Appendix A

RCT of Psychotherapy Quality Rating Scale (RCT-PQRS)

Description of subjects

Item 1. Diagnostic method and criteria for inclusion and exclusion

0	poor description and inappropriate method/criteria
1	full description or appropriate method/criteria
2	full description and appropriate method/criteria

Item 2. Documentation or demonstration of reliability of diagnostic methodology

0	poor or no reliability documentation
1	brief reliability documentation (documentation in the literature is sufficient, even if it is not explicitly cited)
2	full reliability documentation (documentation of within-study reliability necessary)

Item 3. Description of relevant comorbidities

0	poor or no description of relevant comorbidities
1	brief description of relevant comorbidities
2	full description of relevant comorbidities

Item 4. Description of numbers of subjects screened, included, and excluded

0	poor or no description of numbers screened, included, and excluded
1	brief description of numbers screened, included, and excluded
2	full description of numbers screened, included, and excluded

Definition and delivery of treatment

Item 5. Treatment(s) (including control/comparison groups) are sufficiently described or referenced to allow for replication

0	poor or no treatment description or references
1	brief treatment description or references (also if full description of one group and poor description of another)
2	full treatment description or references (manual not required)

Item 6. Method to demonstrate that treatment being studied is treatment being delivered (only satisfied by supervision if transcripts or tapes are explicitly reviewed)

0	poor or no adherence reporting
1	brief adherence reporting with standardized measure or full adherence reporting with non-standardized measure (eg, non-independent rater)

2	full adherence reporting with standardized measure (must be quantitative and completed by an independent rater)

Item 7. Therapist training and level of experience in the treatment(s) under investigation

0	poor description and underqualified therapists
1	full description or well-qualified therapists
2	full description and well-qualified therapists

Item 8. Therapist supervision while treatment is being provided

0	poor description and inadequate therapist supervision
1	full description or adequate therapist supervision
2	full description and adequate therapist supervision

Item 9. Description of concurrent treatments (eg, medication) allowed and administered during course of study (if patients on medication are included, a rating of 2 requires full reporting of what medications were used; if patients on medications are excluded, this alone is sufficient for a rating of 2).

0	poor or no description of concurrent treatments
1	brief description of concurrent treatments
2	full description of concurrent treatments

Outcome measures

Item 10. Validated outcome measure(s) (either established or newly standardized)

0	poor or no validation of outcome measure(s)
1	brief validation of outcome measure(s) (shown or cited)
2	full validation of outcome measure(s) (shown or cited)

Item 11. Primary outcome measure(s) specified in advance (although does not need to be stated explicitly for a rating of 2)

0	poor or no specification of primary outcome measure(s) in advance
1	brief specification of primary outcome measure(s) in advance
2	full specification of primary outcome measure(s) in advance

Item 12. Outcome assessment by raters blinded to treatment group and with established reliability

0	poor or no blinding of raters to treatment group (eg, rating by therapist, non-blind independent rater, or patient self-report) and reliability not reported
1	blinding of independent raters to treatment group or established reliability
2	blinding of independent raters to treatment group and established reliability

Item 13. Discussion of safety and adverse events during study treatment(s)?

0	poor or no discussion of safety and adverse events

1	brief discussion of safety and adverse events
2	full discussion of safety and adverse events

Item 14. Assessment of long-term posttermination outcome (should not be penalized for failure to follow comparison group if this is a wait- list or nontreatment group that is subsequently referred for active treatment)

0	poor or no posttermination assessment of outcome
1	medium-term assessment of posttermination outcome (2-12 months posttermination)
2	long-term assessment of posttermination outcome (≥12 months posttermination)

Data analysis

Item 15. Intent-to-treat method for data analysis involving primary outcome measure

0	no description or no intent-to-treat analysis with primary outcome measure
1	partial intent-to-treat analysis with primary outcome measure
2	full intent-to-treat analysis with primary outcome measure

Item 16. Description of dropouts and withdrawals

0	poor or no description of dropouts and withdrawals
1	brief description of dropouts and withdrawals
2	full description of dropouts and withdrawals (must be explicitly stated and include reasons for dropouts and withdrawals)

Item 17. Appropriate statistical tests (eg, use of Bonferroni correction, longitudinal data analysis, adjustment only for a priori identified confounders)

0	inappropriate statistics, extensive data dredging, or no information about appropriateness of statistics
1	moderately appropriate, though unsophisticated, statistics and/or moderate data dredging
2	fully appropriate statistics and minimal data dredging in primary findings

Item 18. Adequate sample size

0	inadequate justification and inadequate sample size
1	adequate justification or adequate sample size
2	adequate justification and adequate sample size

Item 19. Appropriate consideration of therapist and site effects

0	therapist and site effects not discussed or considered
1	therapist and site effects discussed or considered statistically

2	therapist and site effects discussed and considered statistically

Treatment assignment

Item 20. A priori relevant hypotheses that justify comparison group(s)

0	poor or no justification of comparison group(s)
1	brief or incomplete justification of comparison group(s)
2	full justification of comparison group(s)

Item 21. Comparison group(s) from same population and time frame as experimental group

0	comparison group(s) from significantly different population and/or time frame
1	comparison group(s) from moderately different population and/or time frame
2	comparison group(s) from same population and time frame

Item 22. Randomized assignment to treatment groups

0	poor (eg, pseudo-randomization, sequential assignment) or no randomization
1	adequate but poorly defined randomization procedure
2	full and appropriate method of randomization performed after screening and baseline assessment

Overall quality of study

Item 23. Balance of allegiance to types of treatment by practitioners

0	no information or poor balance of allegiance to treatments by study therapists (eg, therapy in experimental and control groups both administered by therapists with strong allegiance to therapy being tested in the experimental group)
1	some balance of allegiance to treatments by study therapists
2	full balance of allegiance to treatments (eg, therapies administered by therapists with allegiance to respective techniques)

Item 24. Conclusions of study justified by sample, measures, and data analysis, as presented (note: useful to look at conclusions as stated in study abstract)

0	poor or no justification of conclusions from results as presented or insufficient information to evaluate (eg, sample or treatment insufficiently documented, data analysis does not support conclusions, or numbers of withdrawals or dropouts makes findings unsupportable)
1	some conclusions of study justified or partial information presented to evaluate
2	all conclusions of study justified and complete information presented to evaluate

Item 25. Omnibus rating: please provide an overall rating of the quality of the study, taking into account the adequacy of description, the quality of study design, data analysis, and justification of conclusions.

24 items in total/score range 0-48

1 = exceptionally poor (0-5)
2 = very poor (6-12)
3 = moderately poor (13-19)
4 = average (20-27)
5 = moderately good (28-33)
6 = very good (34-41)
7 = exceptionally good (42-48)

Appendix B

Quality Assessment Tool for Observational Cohort and Cross-Section Studies

Criteria	Yes	No	Other (CD, NR, NA)*
1. Was the research question or objective in this paper clearly stated?			
2. Was the study population clearly specified and defined?			
3. Was the participation rate of eligible persons at least 50%?			
4. Were all the subjects selected or recruited from the same or similar populations (including the same time period)? Were inclusion and exclusion criteria for being in the study prespecified and applied uniformly to all participants?			
5. Was a sample size justification, power description, or variance and effect estimates provided?			
6. For the analyses in this paper, were the exposure(s) of interest measured prior to the outcome(s) being measured?			
7. Was the timeframe sufficient so that one could reasonably expect to see an association between exposure and outcome if it existed?			
8. For exposures that can vary in amount or level, did the study examine different levels of the exposure as related to the outcome (e.g., categories of exposure, or exposure measured as continuous variable)?			
9. Were the exposure measures (independent variables) clearly defined, valid, reliable, and implemented consistently across all study participants?			
10. Was the exposure(s) assessed more than once over time?			
11. Were the outcome measures (dependent variables) clearly defined, valid, reliable, and implemented consistently across all study participants?			
12. Were the outcome assessors blinded to the exposure status of participants?			
13. Was loss to follow-up after baseline 20% or less?			
14. Were key potential confounding variables measured and adjusted statistically for their impact on the relationship between exposure(s) and outcome(s)?			

Appendix C

Cochrane risk of bias tool

Bias Domain	Signalling questions	Elaboration	Response Options
Bias due to confounding	1.1 Is there potential for confounding of the effect of intervention in this study? **If N/PN to 1.1:** the study can be considered to be at low risk of bias due to confounding and no further signalling questions need be considered	In rare situations, such as when studying harms that are very unlikely to be related to factors that influence treatment decisions, no confounding is expected and the study can be considered to be at low risk of bias due to confounding, equivalent to a fully randomized trial. There is no NI (No information) option for this signalling question.	Y / PY / PN / N
	If Y/PY to 1.1: determine whether there is a need to assess time-varying confounding:		
	1.2. Was the analysis based on splitting participants' follow up time according to intervention received? **If N/PN**, answer questions relating to baseline confounding (1.4 to 1.6) **If Y/PY**, proceed to question 1.3.	If participants could switch between intervention groups then associations between intervention and outcome may be biased by time-varying confounding. This occurs when prognostic factors influence switches between intended interventions.	NA / Y / PY / PN / N / NI
	1.3. Were intervention discontinuations or switches likely	If intervention switches are unrelated to the outcome, for example when the outcome is an unexpected harm, then time-varying	NA / Y / PY / PN / N / NI

	to be related to factors that are prognostic for the outcome? **If N/PN**, answer questions relating to baseline confounding (1.4 to 1.6) **If Y/PY**, answer questions relating to both baseline and time-varying confounding (1.7 and 1.8)	confounding will not be present and only control for baseline confounding is required.	
	Questions relating to baseline confounding only		
	1.4. Did the authors use an appropriate analysis method that controlled for all the important confounding domains?	Appropriate methods to control for measured confounders include stratification, regression, matching, standardization, and inverse probability weighting. They may control for individual variables or for the estimated propensity score. Inverse probability weighting is based on a function of the propensity score. Each method depends on the assumption that there is no unmeasured or residual confounding.	NA / Y / PY / PN / N / NI
	1.5. **If Y/PY to 1.4**: Were confounding domains that were controlled for measured validly and reliably by the variables available in this study?	Appropriate control of confounding requires that the variables adjusted for are valid and reliable measures of the confounding domains. For some topics, a list of valid and reliable measures of confounding domains will be specified in the review protocol but for others such a list may not be available. Study authors may cite references to support the use of a particular measure. If authors control for confounding variables with no indication of their validity or reliability pay attention to the subjectivity of the measure. Subjective measures (e.g. based on self-report) may have lower validity and reliability than objective measures such as lab findings.	NA / Y / PY / PN / N / NI
	1.6. Did the authors control for any post-intervention variables that could have been affected by the intervention?	Controlling for post-intervention variables that are affected by intervention is not appropriate. Controlling for mediating variables estimates the direct effect of intervention and may introduce bias. Controlling for common effects of intervention and outcome	NA / Y / PY / PN / N / NI

		introduces bias.	
	Questions relating to baseline and time-varying confounding		
	1.7. Did the authors use an appropriate analysis method that adjusted for all the important confounding domains and for time-varying confounding?	Adjustment for time-varying confounding is necessary to estimate the effect of starting and adhering to intervention, in both randomized trials and NRSI. Appropriate methods include those based on inverse probability weighting. Standard regression models that include time-updated confounders may be problematic if time-varying confounding is present.	NA / Y / PY / PN / N / NI
	1.8. **If Y/PY to 1.7**: Were confounding domains that were adjusted for measured validly and reliably by the variables available in this study?	See 1.5 above.	NA / Y / PY / PN / N / NI
	Risk of bias judgement		Low / Moderate / Serious / Critical / NI
	Optional: What is the predicted direction of bias due to confounding?	Can the true effect estimate be predicted to be greater or less than the estimated effect in the study because one or more of the important confounding domains was not controlled for? Answering this question will be based on expert knowledge and results in other studies and therefore can only be completed after all of the studies in the body of evidence have been reviewed. Consider the potential effect of each of the unmeasured domains and whether all important confounding domains not controlled for in the analysis would be likely to change the estimate in the same direction, or if one important confounding domain that was not controlled for in the analysis is likely to have a dominant impact.	Favours experimental / Favours comparator / Unpredictable
Bias in selection of participants into the study	2.1. Was selection of participants into the study (or into the analysis) based on participant characteristics observed after the	This domain is concerned only with selection into the study based on participant characteristics observed *after* the start of intervention. Selection based on characteristics observed *before* the start of intervention can be addressed by controlling for imbalances	Y / PY / PN / N / NI

	start of intervention? **If N/PN to 2.1:** go to 2.4 2.2. **If Y/PY to 2.1**: Were the post-intervention variables that influenced selection likely to be associated with intervention? 2.3 **If Y/PY to 2.2**: Were the post-intervention variables that influenced selection likely to be influenced by the outcome or a cause of the outcome?	between experimental intervention and comparator groups in baseline characteristics that are prognostic for the outcome (baseline confounding). Selection bias occurs when selection is related to an effect of either intervention or a cause of intervention **and** an effect of either the outcome or a cause of the outcome. Therefore, the result is at risk of selection bias if selection into the study is related to both the intervention and the outcome.	NA / Y / PY / PN / N / NI
	2.4. Do start of follow-up and start of intervention coincide for most participants?	If participants are not followed from the start of the intervention then a period of follow up has been excluded, and individuals who experienced the outcome soon after intervention will be missing from analyses. This problem may occur when prevalent, rather than new (incident), users of the intervention are included in analyses.	Y / PY / PN / N / NI
	2.5. **If Y/PY to 2.2 and 2.3, or N/PN to 2.4**: Were adjustment techniques used that are likely to correct for the presence of selection biases?	It is in principle possible to correct for selection biases, for example by using inverse probability weights to create a pseudo-population in which the selection bias has been removed, or by modelling the distributions of the missing participants or follow up times and outcome events and including them using missing data methodology. However such methods are rarely used and the	NA / Y / PY / PN / N / NI

		answer to this question will usually be “No”.	
	Risk of bias judgement		Low / Moderate / Serious / Critical / N
	Optional: What is the predicted direction of bias due to selection of participants into the study?	If the likely direction of bias can be predicted, it is helpful to state this. The direction might be characterized either as being towards (or away from) the null, or as being in favour of one of the interventions.	Favours experimental / Favours comparator / Towards null /Away from null / Unpredictable
Bias in classification of interventions	3.1 Were intervention groups clearly defined?	A pre-requisite for an appropriate comparison of interventions is that the interventions are well defined. Ambiguity in the definition may lead to bias in the classification of participants. For individual-level interventions, criteria for considering individuals to have received each intervention should be clear and explicit, covering issues such as type, setting, dose, frequency, intensity and/or timing of intervention. For population-level interventions (e.g. measures to control air pollution), the question relates to whether the population is clearly defined, and the answer is likely to be ‘Yes’.	Y / PY / PN / N / NI
	3.2 Was the information used to define intervention groups recorded at the start of the intervention?	In general, if information about interventions received is available from sources that could not have been affected by subsequent outcomes, then differential misclassification of intervention status is unlikely. Collection of the information at the time of the intervention makes it easier to avoid such misclassification. For population-level interventions (e.g. measures to control air pollution), the answer to this question is likely to be ‘Yes’.	Y / PY / PN / N / NI
	3.3 Could classification of intervention status have been affected by knowledge of the outcome or risk of the outcome?	Collection of the information at the time of the intervention may not be sufficient to avoid bias. The way in which the data are collected for the purposes of the NRSI should also avoid misclassification.	Y / PY / PN / N / NI
	Risk of bias judgement		Low / Moderate / Serious / Critical / NI

	Optional: What is the predicted direction of bias due to measurement of outcomes or interventions?	If the likely direction of bias can be predicted, it is helpful to state this. The direction might be characterized either as being towards (or away from) the null, or as being in favour of one of the interventions.	Favours experimental / Favours comparator / Towards null /Away from null / Unpredictable
Bias due to deviations from intended interventions	**If your aim for this study is to assess the effect of assignment to intervention, answer questions 4.1 and 4.2**		
	4.1. Were there deviations from the intended intervention beyond what would be expected in usual practice?	Deviations that happen in usual practice following the intervention (for example, cessation of a drug intervention because of acute toxicity) are part of the intended intervention and therefore do not lead to bias in the effect of assignment to intervention. Deviations may arise due to expectations of a difference between intervention and comparator (for example because participants feel unlucky to have been assigned to the comparator group and therefore seek the active intervention, or components of it, or other interventions). Such deviations are not part of usual practice, so may lead to biased effect estimates. However these are not expected in observational studies of individuals in routine care.	Y / PY / PN / N / NI
	4.2. **If Y/PY to 4.1**: Were these deviations from intended intervention unbalanced between groups *and* likely to have affected the outcome?	Deviations from intended interventions that do not reflect usual practice will be important if they affect the outcome, but not otherwise. Furthermore, bias will arise only if there is imbalance in the deviations across the two groups.	NA / Y / PY / PN / N / NI
	If your aim for this study is to assess the effect of starting and adhering to intervention, answer questions 4.3 to 4.6		
	4.3. Were important co-interventions balanced across intervention groups?	Risk of bias will be higher if unplanned co-interventions were implemented in a way that would bias the estimated effect of intervention. Co-interventions will be important if they affect the outcome, but not otherwise. Bias will arise only if there is imbalance in such co-interventions between the intervention groups. Consider the co-interventions, including any pre-specified co-interventions,	Y / PY / PN / N / NI

		that are likely to affect the outcome and to have been administered in this study. Consider whether these co-interventions are balanced between intervention groups.	
	4.4. Was the intervention implemented successfully for most participants?	Risk of bias will be higher if the intervention was not implemented as intended by, for example, the health care professionals delivering care during the trial. Consider whether implementation of the intervention was successful for most participants.	Y / PY / PN / N / NI
	4.5. Did study participants adhere to the assigned intervention regimen?	Risk of bias will be higher if participants did not adhere to the intervention as intended. Lack of adherence includes imperfect compliance, cessation of intervention, crossovers to the comparator intervention and switches to another active intervention. Consider available information on the proportion of study participants who continued with their assigned intervention throughout follow up, and answer 'No' or 'Probably No' if this proportion is high enough to raise concerns. Answer 'Yes' for studies of interventions that are administered once, so that imperfect adherence is not possible. We distinguish between analyses where follow-up time after interventions switches (including cessation of intervention) is assigned to (1) the new intervention or (2) the original intervention. (1) is addressed under time-varying confounding, and should not be considered further here.	Y / PY / PN / N / NI
	4.6. **If N/PN to 4.3, 4.4 or 4.5**: Was an appropriate analysis used to estimate the effect of starting and adhering to the intervention?	It is possible to conduct an analysis that corrects for some types of deviation from the intended intervention. Examples of appropriate analysis strategies include inverse probability weighting or instrumental variable estimation. It is possible that a paper reports such an analysis without reporting information on the deviations from intended intervention, but it would be hard to judge such an analysis to be appropriate in the absence of such information. Specialist advice may be needed to assess studies that used these approaches.	NA / Y / PY / PN / N / NI

		If everyone in one group received a co-intervention, adjustments cannot be made to overcome this.	
	Risk of bias judgement		
	Optional: What is the predicted direction of bias due to deviations from the intended interventions?	If the likely direction of bias can be predicted, it is helpful to state this. The direction might be characterized either as being towards (or away from) the null, or as being in favour of one of the interventions.	
Bias due to missing data	5.1 Were outcome data available for all, or nearly all, participants?	"Nearly all" should be interpreted as "enough to be confident of the findings", and a suitable proportion depends on the context. In some situations, availability of data from 95% (or possibly 90%) of the participants may be sufficient, providing that events of interest are reasonably common in both intervention groups. One aspect of this is that review authors would ideally try and locate an analysis plan for the study.	Y / PY / PN / N / NI
	5.2 Were participants excluded due to missing data on intervention status?	Missing intervention status may be a problem. This requires that the *intended* study sample is clear, which it may not be in practice.	Y / PY / PN / N / NI
	5.3 Were participants excluded due to missing data on other variables needed for the analysis?	This question relates particularly to participants excluded from the analysis because of missing information on confounders that were controlled for in the analysis.	Y / PY / PN / N / NI
	5.4 **If PN/N to 5.1, or Y/PY to 5.2 or 5.3**: Are the proportion of participants and reasons for missing data similar across interventions?	This aims to elicit whether either (i) differential proportion of missing observations or (ii) differences in reasons for missing observations could substantially impact on our ability to answer the question being addressed. "Similar" includes some minor degree of discrepancy across intervention groups as expected by chance.	NA / Y / PY / PN / N / NI
	5.5 **If PN/N to 5.1, or Y/PY to 5.2 or 5.3**: Is there evidence that results were robust to the presence of missing data?	Evidence for robustness may come from how missing data were handled in the analysis and whether sensitivity analyses were performed by the investigators, or occasionally from additional analyses performed by the systematic reviewers. It is important to assess whether assumptions employed in analyses are clear and	NA / Y / PY / PN / N / NI

		plausible. Both content knowledge and statistical expertise will often be required for this. For instance, use of a statistical method such as multiple imputation does not guarantee an appropriate answer. Review authors should seek naïve (complete-case) analyses for comparison, and clear differences between complete-case and multiple imputation-based findings should lead to careful assessment of the validity of the methods used.	
	Risk of bias judgement		Low / Moderate / Serious / Critical / NI
	Optional: What is the predicted direction of bias due to missing data?	If the likely direction of bias can be predicted, it is helpful to state this. The direction might be characterized either as being towards (or away from) the null, or as being in favour of one of the interventions.	Favours experimental / Favours comparator / Towards null /Away from null / Unpredictable
Bias in measurement of outcomes	6.1 Could the outcome measure have been influenced by knowledge of the intervention received?	Some outcome measures involve negligible assessor judgment, e.g. all-cause mortality or non-repeatable automated laboratory assessments. Risk of bias due to measurement of these outcomes would be expected to be low.	Y / PY / PN / N / NI
	6.2 Were outcome assessors aware of the intervention received by study participants?	If outcome assessors were blinded to intervention status, the answer to this question would be 'No'. In other situations, outcome assessors may be unaware of the interventions being received by participants despite there being no active blinding by the study investigators; the answer this question would then also be 'No'. In studies where participants report their outcomes themselves, for example in a questionnaire, the outcome assessor is the study participant. In an observational study, the answer to this question will usually be 'Yes' when the participants report their outcomes themselves.	Y / PY / PN / N / NI
	6.3 Were the methods of outcome assessment comparable across	Comparable assessment methods (i.e. data collection) would involve the same outcome detection methods and thresholds, same time	Y / PY / PN / N / NI

	intervention groups?	point, same definition, and same measurements.	
	6.4 Were any systematic errors in measurement of the outcome related to intervention received?	This question refers to differential misclassification of outcomes. Systematic errors in measuring the outcome, if present, could cause bias if they are related to intervention or to a confounder of the intervention-outcome relationship. This will usually be due either to outcome assessors being aware of the intervention received or to non-comparability of outcome assessment methods, but there are examples of differential misclassification arising despite these controls being in place.	Y / PY / PN / N / NI
	Risk of bias judgement		Low / Moderate / Serious / Critical / NI
	Optional: What is the predicted direction of bias due to measurement of outcomes?	If the likely direction of bias can be predicted, it is helpful to state this. The direction might be characterized either as being towards (or away from) the null, or as being in favour of one of the interventions.	Favours experimental / Favours comparator / Towards null /Away from null / Unpredictable
Bias in selection of the reported result	Is the reported effect estimate likely to be selected, on the basis of the results, from... 7.1. ... multiple outcome *measurements* within the outcome domain?	For a specified outcome domain, it is possible to generate multiple effect estimates for different measurements. If multiple measurements were made, but only one or a subset is reported, there is a risk of selective reporting on the basis of results.	Y / PY / PN / N / NI
	7.2 ... multiple *analyses* of the intervention-outcome relationship?	Because of the limitations of using data from non-randomized studies for analyses of effectiveness (need to control confounding, substantial missing data, etc), analysts may implement different analytic methods to address these limitations. Examples include unadjusted and adjusted models; use of final value vs change from baseline vs analysis of covariance; different transformations of variables; a continuously scaled outcome converted to categorical data with different cut-points; different sets of covariates used for	Y / PY / PN / N / N

		adjustment; and different analytic strategies for dealing with missing data. Application of such methods generates multiple estimates of the effect of the intervention versus the comparator on the outcome. If the analyst does not pre-specify the methods to be applied, and multiple estimates are generated but only one or a subset is reported, there is a risk of selective reporting on the basis of results.	
	7.3 ... different *subgroups*?	Particularly with large cohorts often available from routine data sources, it is possible to generate multiple effect estimates for different subgroups or simply to omit varying proportions of the original cohort. If multiple estimates are generated but only one or a subset is reported, there is a risk of selective reporting on the basis of results.	Y / PY / PN / N / NI
	Risk of bias judgement		Low / Moderate / Serious / Critical / NI
	Optional: What is the predicted direction of bias due to selection of the reported result?	If the likely direction of bias can be predicted, it is helpful to state this. The direction might be characterized either as being towards (or away from) the null, or as being in favour of one of the interventions.	Favours experimental / Favours comparator / Towards null /Away from null / Unpredictable
Overall bias	**Risk of bias judgement**		Low / Moderate / Serious / Critical / NI
	Optional: What is the overall predicted direction of bias for this outcome?	Favours experimental / Favours comparator / Towards null /Away from null / Unpredictable	Optional: What is the overall predicted direction of bias for this outcome?

Appendix D

Adapted Quality Assessment Tool

Description of subjects

Item 1. Diagnostic method and criteria for inclusion and exclusion

0	poor description and inappropriate method/criteria
1	full description or appropriate method/criteria
2	full description and appropriate method/criteria

Item 2. Documentation or demonstration of reliability of diagnostic methodology

0	poor or no reliability documentation
1	brief reliability documentation (documentation in the literature is sufficient, even if it is not explicitly cited)
2	full reliability documentation (documentation of within-study reliability necessary)

Item 3. Description of relevant comorbidities

0	poor or no description of relevant comorbidities
1	brief description of relevant comorbidities
2	full description of relevant comorbidities

Item 4. Description of numbers of subjects screened, included, and excluded

0	poor or no description of numbers screened, included, and excluded
1	brief description of numbers screened, included, and excluded
2	full description of numbers screened, included, and excluded

Item 5. Was the participation rate of eligible persons at least 50%?

0	No
1	Yes, does not clearly state this
2	Yes- Clearly states this and close to or all eligible persons participated

Definition and delivery of paradigm

Item 6. Paradigm(s) (including control/comparison groups) are sufficiently described or referenced to allow for replication

0	poor or no paradigm description or references
1	brief paradigm description or references (also if full description of one group and poor description of another)

2	full paradigm description or references

Item 7. Is the paradigm being used to measure the primary variable of interest?

0	poor description or not stated
1	Partial description of primary variable
2	Full description of primary variable

Outcome measures

Item 8. Does the study report the raw scores for the primary variable of interest?

0	No
1	Partial reporting (e.g. provide reaction times for food, but not for each group)
2	All of the raw scores reported for all groups and for all stimuli-types

Item 9. Were key potential confounding variables measured and adjusted statistically for their impact on the relationship between the paradigm and outcome?

0	poor or no description of confounding variables
1	brief description of confounding variables
2	full description adjustment for confounding variables

Item 10. Outcome assessment by raters blinded to participant group and with established reliability

0	poor or no blinding of raters to participant group (eg, rating by therapist, non-blind independent rater, or patient self-report) and reliability not reported
1	blinding of independent raters to participant group or established reliability
2	blinding of independent raters to participant group and established reliability

Item 11. Discussion of safety and adverse events during study experiment(s)?

0	poor or no discussion of safety and adverse events
1	brief discussion of safety and adverse events
2	full discussion of safety and adverse events

Data analysis

Item 12. Description of dropouts and withdrawals in the procedure

0	poor or no description of dropouts and withdrawals
1	brief description of dropouts and withdrawals
2	full description of dropouts and withdrawals (must be explicitly stated and include reasons for dropouts and withdrawals)

Item 13. Were all of the recruited participants included in the analysis?

0	No description
1	Not all participants were not included in the analysis, or a poor description was provided
2	Yes, all of the participants were included and adequately described

Item 14. Appropriate statistical tests (eg, use of Bonferroni correction, longitudinal data analysis, adjustment only for a priori identified confounders)

0	inappropriate statistics, extensive data dredging, or no information about appropriateness of statistics
1	moderately appropriate, though unsophisticated, statistics and/or moderate data dredging
2	fully appropriate statistics and minimal data dredging in primary findings

Item 15. Adequate sample size

0	inadequate justification and inadequate sample size
1	adequate justification or adequate sample size
2	adequate justification and adequate sample size

Experimental group assignment

Item 16. A priori relevant hypotheses that justify comparison group(s)

0	poor or no justification of comparison group(s)
1	brief or incomplete justification of comparison group(s)
2	full justification of comparison group(s)

Item 17. Comparison group(s) from same population and time frame as experimental group

0	comparison group(s) from significantly different population and/or time frame
1	comparison group(s) from moderately different population and/or time frame
2	comparison group(s) from same population and time frame

Overall quality of study

Item 18. Conclusions of study justified by sample, measures, and data analysis, as presented (note: useful to look at conclusions as stated in study abstract)

0	poor or no justification of conclusions from results as presented or insufficient information to evaluate (eg, sample or treatment insufficiently documented, data analysis does not support conclusions, or numbers of withdrawals or dropouts makes findings unsupportable)
1	some conclusions of study justified, or partial information presented to evaluate
2	all conclusions of study justified, and complete information presented to evaluate

Item 19. Omnibus rating: please provide an overall rating of the quality of the study, taking into account the adequacy of description, the quality of study design, data analysis, and justification of conclusions.

18 items in total/score range 0-36

1 = exceptionally poor (0-3)
2 = very poor (4-8)
3 = moderately poor (9-12)
4 = average (13-17)
5 = moderately good (18-23)
6 = very good (24-30)
7 = exceptionally good (31-36)

Chapter Five. Discussion and Critical Appraisal

Word Count: 4062

Chapter Five. Discussion and Critical Appraisal

This final chapter provides a summary of the rationale and findings from the two meta-analyses, followed by a critical appraisal of the research process. It also considers the theoretical implications of the findings, and how the two papers may have an impact on both clinical practice and future research. The chapter ends with a conclusion of the thesis portfolio and reflections on the research process.

Thesis Rationale

The aim of the thesis was to explore areas within eating disorder (ED) literature that are under-researched. The scientific understanding of EDs has changed significantly, from the belief that anorexia nervosa (AN) was caused by "ill and morbid state of the spirits" in the Renaissance period (Harris, 2014), to the acknowledgement of EDs being a psychological disorder in the latter half of the 19th century (Dell'Osso et al., 2016). The understanding of the mechanisms maintaining EDs has improved vastly in the last 30 years, particularly since the success of the cognitive model of EDs led to successful treatment outcomes when treating bulimia nervosa (BN) with cognitive behavioural therapy (Murphy, Staebler, Cooper, & Fairburn, 2010). However, there is still little understanding of how to successfully treat other types of EDs (Pike, Attia, & Brown, 2008), and how to prevent the development of chronic EDs in youth (Hurst, Read, & Wallis, 2012). This is important as EDs have the highest mortality rate compared to other mental health disorders, a rate that increases by 5% for every decade that an individual remains symptomatic (Couturier, Kimber & Szatmari, 2013).

Binge eating disorder (BED) is one of the lesser-known categories of ED. BED has a lifetime prevalence of 1.4% (Amianto, Ottone, Daga & Fassino, 2015). It is associated with shame and obesity, with a high rate of co-morbidity between BED and both depression and anxiety (Amianto et al., 2015; Vocks et al., 2010). As mentioned in

previous chapters, the mechanisms underlying and maintaining BED are poorly understood.

Experimental research has begun testing the applicability of various cognitive components of AN and BN to BED. In particular, the roles of information processing have been tested (Stojek et al., 2018). To date, a meta-analysis on components of information processing, such as attentional biases (ABs) and BED has not been conducted, and the recent systematic review of this area by Stojek et al. (2018) had a number of limitations. The basis for conducting the meta-analysis in chapter one was to add to the literature by testing a component of the cognitive theory to BED: ABs towards food cues.

Another developing area within EDs is the treatment of children and adolescents with AN and BN. Previously, young people were the focus of treatment; however, emphasis is now placed on helping the family mobilise around the ED to help the child challenge the ED (Eisler et al., 2016). Parents are viewed as a helpful resource in their child's recovery, rather than blame being placed on them for the illness developing. This has become an important principle of eating disorder-focused family therapy (ED-FT), which is the current leading treatment for children and adolescents (Couturier et al., 2013).

The mechanisms underlying the effectiveness of ED-FT remain elusive and requires continued investigation. In particular, it is unclear whether modifications to the treatment may produce more successful outcomes. For example, delivering ED-FT in a group format, as in multifamily therapy (MFT), was shown to be more successful in a recent randomised controlled trial (Eisler et al., 2016). The aim of the second paper was to investigate effectiveness of MFT compared with the research conducted on ED-FT.

Summary of the Meta-Analysis on ABs in BED

The meta-analysis in chapter one examined whether individuals with clinical or subthreshold BED exhibit greater ABs towards food cues, compared with controls. Thirteen studies were included in the review, including seven that used variants of the Stroop task; two studies that used the visual probe task; two that used the visual search task; and two that used the spatial cueing task. Five studies provided the necessary information to be able to meta-analyse. The remaining eight could not be included in the meta-analysis due to the variations in the methodology and reporting of the results.

An AB score was calculated for each study in order to standardise the measure of AB across paradigms. This was then meta-analysed, demonstrating that the size of the effect was close to zero, which indicates that there is in fact little difference between the groups and therefore no effect. The degree of heterogeneity as evidenced by the Q-statistic and I-squared was low. This indicated that this analysis may provide an accurate reflection of the difference between groups.

Summary of the Meta-Analysis on ED-FT and MFT

The meta-analysis in chapter three was divided into two sections. The first section demonstrated that ED-FT is statistically significantly more efficacious than individual therapy at end of treatment (EOT), with the size of the effect in favour of ED-FT increasing slightly at follow-up and becoming more statistically significant. However, there was no statistically significant difference at EOT or follow-up for rates of remission when an includive approach was taken to comparing ED-FT with other forms of family therapy (FT), although the effect favoured other forms of FT. There was also a greater range in individual effect sizes, indicating greater heterogeneity. This may be due to greater variance in the types of family therapy that were used as comparisons.

The meta-analysis of MFT on pre-treatment and post-treatment weight demonstrated a statistically significant medium effect in favour of receiving MFT, indicating that the patients receiving MFT experienced a significant increase in weight post-treatment. This result must be interpreted with caution, however, as a comparison group was not available due to the research methodologies of the papers. Therefore, cause and effect cannot be determined. A narrative synthesis of MFT was also provided, which explored the quality of the literature in this growing area of treatment, compared with the quality of the RCTs conducted within ED-FT. This highlighted that the quality of the literature on MFT is poor, particularly when compared with the RCTs in ED-FT.

Overall Strengths

A strength lies in conducting a quantitative analysis of studies, as well as providing a systematic narrative synthesis of the literature. Calculating the individual and pooled effect of studies can increase the validity of studies with small sample sizes, and enable effects to be found across studies, that would otherwise have been undetected due to the small power of individual studies (Walker, Hernandez, & Kattan, 2008). In particular, it allows for a greater number of variables to be analysed, such as conducting meta-regressions to identify subsets of participants or variables that might have an impact on the overall effect. Conclusions have been able to be drawn from pooling the data together, which otherwise would have been undetected by individual studies, due to inadequate power, such as the effect of MFT on weight gain.

Another strength lies within aspects of the methodological approach taken for both meta-analyses. During the search phase of both meta-analyses, an effort was made to identify unpublished studies. Although none met inclusion criteria, this act is important because published research tends to be skewed in favour of publishing studies that represent a significant result in favour of a particular intervention (Walker et al.,

2008). The meta-analyses were comprehensive as multiple data-bases were used when searching for papers. This insured that as many studies as possible were captured. Finally, conducting a sensitivity analysis in the meta-analysis of AB in BED and a meta-regression in the meta-analysis of ED-FT and MFT increases the robustness of the findings.

The generalisability of both papers is also a strength of the thesis. In regard to the meta-analysis in chapter one, the decision not to have an age-limit on the inclusion criteria enabled an investigation of ABs in BED across the lifespan. However, it is important to acknowledge that this also lowers the validity of the findings, as it is possible that adults and young people attend differently to stimuli in their environment (Kittel, Schmidt, & Hilbert, 2017). The studies included in the meta-analysis were from different countries, meaning the results can be applied cross-culturally. Finally, the decision to include participants meeting criteria for clinical BED and subthreshold BED may be contentious. While on the one hand this may be considered a limitation, it can also be seen as strengthening the paper by increasing the generalisability of the findings to the spectrum of BE.

Thesis Limitations

A major limitation with both meta-analyses was the small number of studies included in the analyses and degree of methodological rigour or standardisation across studies. In particular, the meta-analysis of AB in BED highlighted the lack of standardisation in how the outcomes were measured and reported. This resulted in the experimental paradigms needing to be collapsed in order to quantify the findings across studies. Therefore, this paper was limited in the conclusions that could be drawn. By being unable to review the impact of the paradigms on AB, the meta-analysis was unable to comment on the constructs that measure ABs, as the paradigms test different

concepts (Stojek et al., 2018). However, the homogeneous results of the forest plot support the use of this method of analysis. Typically, it becomes more difficult to justify the integration of results from different studies when the heterogeneity is high, as this indicates that the individual studies are very different to each other (Walker et al., 2008). As this was not the case, it can at least be concluded that the individual studies do not contradict each other, and therefore there is a tentative trend in the literature. However, the effects were small and non-significant, and therefore the results need to be interpreted with caution.

Some researchers postulate that meta-analyses should only be conducted on randomised controlled trials (RCT) because this form of research tends to be of a higher quality and reduces the risk of misleading conclusions being drawn (Walker et al., 2008). Both papers are limited as the meta-analysis in chapter one used cross-sectional experimental designs to investigate ABs in BED, and the meta-analysis in chapter three included pre- and post-intervention outcomes for the analysis of MFT. However, it can also be argued that these primary analyses encourage future research in the areas by highlighting possible gaps in our current knowledge.

A limitation of the methodological approach taken for both meta-analyses is that, due to time constraints, the study identification process was conducted by the primary author, as opposed to the studies being scored independently by two or more researchers and the final shortlist being agreed upon. This increases the risk of selection bias as the identification process relied on the decision-making of one researcher (Walker et al., 2008). However, the inclusion and exclusion criteria were discussed and agreed on with the supervisory team. This included discussions around the populations included in the synthesis, study design, criterion for the selection of controls and a time-frame for the searches. This increased the similarities of the selected papers, thus increasing the validity of the results (Walker et al., 2008). The symmetrical funnel plot

generated for both meta-analyses also supports the method of study selection, as neither analyses for publication bias suggested identification or selection bias (Borenstein, Hedges, Higgins, & Rothstein, 2009).

The range in quality of studies included in both papers, and the chosen quality assessment tools are further limitations of the thesis. The poor quality of the studies included in chapter three's meta-analysis hinders the conclusions that can be drawn, particularly from the analysis of MFT. The adaptation of the quality assessment tool in chapter one is a further limitation as the tool has not been robustly validated. Furthermore, only moderate agreement between raters was achieved in both papers, which reduces the confidence with which conclusions can be drawn from the quality assessment (McHugh, 2012).

Theoretical and Clinical Implications

While chapter two discussed the theoretical underpinnings of this thesis, it is important to consider the impact the findings from both meta-analyses may have on existing theory and practice.

Firstly, the meta-analysis on ABs in BED highlighted that the BED group do not appear to respond differently to the control group when attending to food and neutral stimuli. Concerning the theoretical implications, this paper signifies the need for more research on whether other components of the model, such as permissive thoughts and affect regulation, moderate BE (Cooper, Wells, & Todd, 2004). As discussed in the meta-analysis, it is possible that people with BED have a different experience of BE, as they may experience stronger permissive thoughts and stronger drives to escape negative appraisals, and weaker negative thoughts about eating. It may be possible that using food cues is not an adequate measure for the test, as other threat-based cues such as cues about the self or appraisals from others may trigger a stronger AB response.

This may have an impact on the current clinical treatment of BED. Currently, the National Institute for Health and Care Excellence (NICE, 2017) recommend CBT-based self-help is first-line treatment; however, a clear evidence-base for individual treatment does not exist. The focus of the self-help, and indeed CBT, is to modify cognitions, placing emphasis on the eating, shape and weight concerns that are pervasive across all EDs (Fairburn, 2008). However, the results of this meta-analysis suggest that people with a diagnosis of BED may not necessarily benefit as much from treatments targeting their negative appraisals of food, for example. Therefore, it is possible that alternative interventions such as dialectical behaviour therapy may be a more effective as they place more emphasis on coping skills and affect regulation as a mechanism for maintaining the disorder (Lacovino, Gredysa, Altman, & Wilfley, 2012).

Secondly, the meta-analysis on ED-FT and MFT confirmed the strength of ED-FT compared with individual treatment supporting the NICE guidelines (2017). However, it also highlighted the lack of significant effect when ED-FT was compared with other forms of FT, such as modification to ED-FT. This has theoretical implications as it tentatively implies that the systemic principles from which ED-FT is based may not need to be delivered from the current manualised format.

The synthesis of the literature also highlighted the stringent measure of remission that was used to measure treatment success. This may have clinical implications as services may be unable to follow such a high rating for treatment success. The paper also highlighted that the majority of studies delivered ED-FT over a six-month period, which is less than the recommended treatment dose recommended by NICE (2017). If studies increase the duration of treatment and number of sessions, and also adapt the measure of remission to be in line with clinical services Royal College of Psychiatrists, 2015) they may find that the success-rates could improve.

The lack of routine monitoring of treatment fidelity has clinical implications as different services may be at risk of delivering the treatment differently. This may be related to the service contexts in which the treatment is delivered, as it could be hypothesised that services more closely affiliated with the institutions that have developed the treatment will adhere more closely to the model (Le Grange, Lock, Agras, Bryson, & Jo, 2015).

Both meta-analyses have implications pertinent to the wider National Health Service (NHS) and society. Societal attitudes towards EDs can be seen to negatively impact people feeling able to access care (Puhl & Heuer, 2010): the societal pressure to be thin has increased over the past 30 years, evidenced by the ideal body image becoming increasingly thin, and increased value being placed on body weight (Dell'Osso et al., 2016). At the same time, access to highly palatable foods has increased. This dichotomous relationship appears to have resulted in more people being aware of EDs.

However, this awareness of EDs does not necessarily translate into successful treatment. EDs are notoriously secretive, with high levels of shame reported in this population (Fairburn, 2008). The low success rate of the current treatments for EDs is a particular concern, especially as there is an increased risk of physical complications and death if left untreated (Sadock & Sadock, 2007).

The meta-analysis of ED-FT and MFT highlighted the possible effect of MFT on weight gain at EOT. The promising findings for MFT indicate the need to further investigate which aspect of this treatment delivery is particularly helpful. It is possible that children and adolescents have not developed the degree of shame reported in the literature for adults and sharing their experiences may therefore aid recovery and reduce feelings of shame. This meta-analysis emphasises the need for more research to be conducted to test this theory further. More crucially, identifying alternative methods of

treatment delivery is vital in order to help the families for whom ED-FT is not beneficial. In particular, the application or modification of ED-FT to inpatient settings is very much needed, as there are currently no guidelines on how to apply this intervention to more intensive settings (NICE, 2017).

While it has not been researched within the BED population, research into obesity has shown that in some parts of the world, weight discrimination is comparable to racial discrimination (Puhl & Heuer, 2010). The notion that the quantity that someone eats is always controllable and therefore obesity is self-inflicted has been shown to have detrimental effects on treatment-seeking patients' participation in treatment programs (Carels et al., 2009). The meta-analysis of ABs in BED signifies the lack of understanding around the disorder, and the importance of research into understanding how to treat it. This is necessary to reduce stigma and bring the disorder to the forefront of public health awareness.

Future Research

Both papers emphasise the need for continued research within the field of EDs. A common theme from both papers is the need for research to improve its methodological rigour. Specifically, the meta-analysis in chapter one highlighted the need for the experimental methods to become more standardised. The disparate way in which outcomes are currently reported in research makes it difficult to appropriately meta-analyse the findings without collapsing variables, whether it be experimental paradigms, diagnostic groups or length of follow-up treatment. Therefore, research into ABs in BED would benefit from being more stringent in how it measures ABs, while research into ED-FT and MFT would benefit from clearly defining remission as the primary outcome measure and testing the effect beyond EOT by conducting more longitudinal research.

Studies that adhere to the gold-standard rigour provide more robust findings, which translates into clearer clinical conclusions (Borenstein et al., 2009). The meta-analysis in both chapters highlighted the need for higher-quality research. Chapter one highlighted the need for a standardised measure to be developed in order to assess the quality of the cross-sectional experimental designs. Chapter three highlighted the need for a greater number of RCTs to test the efficacy of ED-FT compared with other forms of FT or variations of the model, including MFT.

Further investigation into the strategies employed when implementing the therapies is also needed. For example, fidelity towards a treatment manual and the impact of therapeutic rapport need to be the focus of future research, in order to better understand how these factors may moderate treatment success. Conducting focus groups with families may provide further insight into the factors they deem most beneficial.

Future research needs to address the applicability of the interventions and theoretical models to minority groups, and male patients. Both papers in this thesis recruited predominately females whose ethnicity was 'white'. This is a significant limitation of both the current literature and this thesis as it reduces the generalisability of the findings. Men are less likely to meet full criteria for BED, which may indicate a difference in their experience of the disorder (Striegel-Moore & Franko, 2003). Similarly, minority groups may respond differently to ED-FT due to differing cultural norms (Jennings, Kelly-Weeder, & Wolfe, 2015). Therefore, research would benefit from making an effort to recruit male participants and those from minority groups, particularly as prevalence figures indicate that EDs including BED have lifetime prevalence of .3-2.0% in men (Hudson, Hiripi, Pope & Kessler, 2007) and are reported across all racial groups (Lee & Lock, 2007; Jennings et al., 2015).

Conclusions

This thesis portfolio has found two novel conclusions. Firstly, it has shown that people with a diagnosis of BED do not exhibit statistically significantly greater AB scores compared to controls, which is in contrast to the existing theoretical understanding of the disorder and research literature. This raises questions about the suitability of the current experimental methods used to test information processing in BED, and the applicability of aspects of the current theoretical frameworks used to understand the disorder. Secondly, the thesis has shown that ED-FT does not produce the same effects when compared with other FT or variations of ED-FT. In particular, one way of delivering ED-FT, MFT, has produced promising findings, as it indicated significant medium effects for weight gain following this intervention. However, the second meta-analysis also highlighted the discrepancy in the quality of the literature for ED-FT and MFT, and the need for more robust research to be conducted in the latter in order to increase the confidence with which conclusions can be drawn from the findings.

Reflections

My experience of conducting a doctoral thesis is one that I feel will not leave me for some time and, I suspect, has shaped my approach to research. I entered the doctorate with a keen interest in conducting research but had only ever supported established research groups. Looking back, I did not fully appreciate what it meant to create and take ownership of novel research, and the importance of how it informs clinical practice. Therefore, I did not anticipate how gripped I would become with a sense of responsibility to produce an original piece of research with clinical relevance to a particular field.

However, the development of this thesis was somewhat unfortunate as my original project had to be set aside due to it being rejected for ethical approval at the beginning of my final year on training. The reason for this was because the panel felt that the study was investigating too many variables and were unclear on aspects of the analysis. While this could have been perceived as (and at times did feel like) a failing, I mostly saw it as a golden opportunity to go back to the drawing board and find a fresh research area to which I could contribute.

The decision to conduct research in EDs was an easy one for me, due to my clinical interest in the area. However, finding a suitably under-researched topic was difficult as, on the one hand, research into areas such as CBT and AN felt rather saturated but, on the other hand, there were some topics on which I could not conduct meta-analyses because there were too few papers.

I did not anticipate that the process of finding appropriate topics would mirror the psychopathology of EDs. In particular, I began noticing a reflection in the 'perfectionism' trait within AN and BN, and the rigid way in which I tried to find a topic: The more I became immersed in the literature, the higher and less achievable my standards became. Much like the over-evaluation of body, weight and shape which is maintained by the drive to achieve perfection, I found that my personal evaluation of the quality of the thesis became dependant on finding the 'perfect' research question. I used research and clinical supervision in these instances and found that when I relaxed my expectation of finding the 'perfect' topic, I found two areas that are both innovative and interesting.

The process has taught me about my skills as a researcher and has emphasised personal qualities that I did not know existed within me. I have learnt that I have perseverance and will not easily be deterred from a goal. I feel I will complete my doctoral training with confidence in my resilience, but also respect for the research

process. I have learnt how to navigate the ethical approval process and conduct meta-analyses. I will also take away with me the importance of drawing strength from the knowledge and experience of those around me.

My interest in conducting research has not been shaken by the difficult, and at times uncertain, journey I took to complete the thesis. In fact, I feel the opposite has happened as I now have a newfound passion for forging those theory-practice links perceived as fundamental to the profession of Clinical Psychology.

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