Mindfulness-based therapies for

psychological health conditions: a meta-analysis

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Abstract

Introduction

Mindfulness-Based Therapies (MBT) are a current technology within the cognitive-behavioural tradition, which can be grouped according to whether mindfulness is a major or a minor component. A mindful approach to psychological difficulties attempts to change the relationship with unwanted inner experience. The model suggests this may help reduce affective symptomatology. There has been a considerable growth of interest in these therapies with an accompanying increase in the evidence base. A number of reviews have been conducted however they have not comprehensively appraised these therapies. The primary aim of my study was to contribute to ongoing research determining the effectiveness of MBT for the treatment of affective symptomatology. Depending on these results, a secondary aim of the study was to make recommendations for the use of MBT in clinical practice.

Methods

I conducted a meta-analysis which looked separately at therapies considered to have mindfulness as a major component; therapies considered to have mindfulness as a minor component; and a comparison of these two groups. Of the 598 unique citations identified in the literature, 113 were assessed for eligibility and 40 included in the pool of studies for the meta-analysis.

Results

For the major component therapies, there was a significant mean reduction score in depressive symptomatology as measured by the BDI of 8.73 points ($k = 11$; 95% CI = 6.61, 10.86). Evidence of effectiveness was also found for the minor component therapies ($k = 8$) in reducing anxiety symptomatology with a significant standardised mean difference of 1.24 (95% CI = 0.81, 2.10).

Discussion

I found evidence for the effectiveness of mindfulness-based therapies in reducing levels of depression or anxiety mainly in patients diagnosed with depressive or anxiety disorders. The robustness of these findings is discussed alongside the implications for research and practice within the context of the current literature.

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*"Feelings come and go like clouds in a windy sky.
Conscious breathing is my anchor."*

Thich Nhat Hanh

1. Introduction

1.1 *Chapter outline*

In this chapter I introduce the concept of mindfulness and detail how it is applied to clinical psychology practice in the form of the mindfulness-based therapies. I will detail some of the basic concepts and techniques of these therapies, show the linked factors, and illustrate how they work. The historical context of these therapies is then considered before introducing the current evidence base. The chapter ends by detailing the rationale, aims and objectives of my research.

1.2 *What is mindfulness?*

Mindfulness is a form of focused awareness that is intentional and directed to the present moment, with a specific motivation of accepting unconditionally what is revealed or apparent in that moment. A number of definitions of mindfulness exist (Mace, 2008), although they generally share these common characteristics. One of the main challenges that researchers face in the field was agreement as to what mindfulness is and on operationalised definitions enabling its quantification.

Jon Kabat-Zinn, a leading proponent of mindfulness in the West, describes it as "paying attention in a particular way, on purpose, in the present moment, and non-judgementally" (Kabat-Zinn, Lipworth, & Burney, 1985). Whether an emotional dimension is essential to the practice of mindfulness is the subject of debate. Some see mindfulness as having a neutral stance towards emotions – as emotionality can be seen as a form of judgement – whereas others see 'loving kindness' as an essential motivation and intrinsic to practice (Hanh, 1991).

I think the plethora of definitions reflects the inherent nature of mindfulness: it is essentially a subjective, personal experience as its core elements are awareness and focus.

1.3   *Mindfulness-based therapies*

Mindfulness-Based Therapies (MBT[1]) use mindfulness to cultivate an increased awareness in the moment with the intention of alleviating the distressing effects of physical and psychological health problems (Germer, 2005). Mindfulness has been at the centre of meditative traditions dating back thousands of years (See 1.7.1) however its formalisation as a 'Western' therapy is a relatively recent endeavour (Baer & Krietemeyer, 2006).

Mindfulness techniques have been very broadly applied within clinical psychology and this broad application has created a number of distinct therapies with considerable commonality. In some cases these applications are developed from existing models; for other therapies there was a parallel development with a purportedly different theoretical base.

1.3.1   'Mindfulness-based' practices

The first established mindfulness-based therapy was Mindfulness-Based Stress Reduction (MBSR). A number of other therapies with the prefix 'mindfulness-based' have been derived from MBSR and so will be considered together. The most prominent therapy developed from MBSR was Mindfulness-Based Cognitive Therapy (MBCT).

In MBSR and MBCT the mindfulness techniques form the substantive component of the therapy and both were designed for administration in a group format over an eight-week course (see 1.4 for more detail on specific techniques). MBSR was initially developed for stress reduction and the management of chronic pain. MBCT was specifically developed to prevent relapse in recurrent depression and its originators acknowledge that there was as much as an 80% overlap between MBSR and MBCT (Segal, Williams, & Teasdale, 2002).

Other therapies developed from MBSR include: Mindfulness-Based Eating Awareness Training (MB-EAT) for eating disorders (Kristeller & Hallett, 1999);

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[1] See Appendix 6.1 for a list of all abbreviations and acronyms used throughout this thesis

Mindfulness-Based Pain Management (MBPM) (Gardner-Nix, Backman, Barbati, & Grummitt, 2008); Mindfulness-Based Relapse Prevention (MBRP) for recovery from substance abuse and addiction (Witkiewitz, Marlatt, & Walker, 2005); and Mindfulness-Based Relationship Enhancement (MBRE; Carson, Carson, Gil, & Baucom, 2004). Similarly these therapies have considerable overlap with MBSR however they are specifically tailored for the target condition. There are also examples of therapies which draw heavily from mindfulness but do not have the prefix: for example Mindful Awareness Practices (MAP; Zylowska, et al., 2008).

The distinguishing feature of these mindfulness-based practices was that the mainstay of the therapy was the mindfulness technique and its practise. What differentiated them was their tailoring to specific conditions. These therapies could be considered to have mindfulness as a *'major'* component of the intervention. There are also several key themes behind this group of therapies which include acceptance; distancing from unwanted internal experience; and adopting a compassionate stance to one's psychological experience. These themes show some overlap with Acceptance and Commitment Therapy.

1.3.2   Acceptance and Commitment Therapy

Acceptance and Commitment Therapy (ACT – pronounced as the word) was developed by Steven Hayes and originally known as 'comprehensive distancing' (Steven C. Hayes, 1987). Hayes reported that this was a reference to Aaron Beck's use of the term 'distancing' to refer to an objective awareness of one's thoughts. This can be compared to metacognition: an awareness of one's own cognitive processes and the use of this awareness in their regulation (A. Brown, 1987). This reference to Beck's work was testament to the lineage of Hayes' model: he conceptualises ACT firmly within the CBT camp, built on an empirically tested theory of language and cognition (Relational Frame Theory: Steven C. Hayes, Barnes-Holmes, & Roche, 2001).

ACT is defined as:

> *"A therapy approach that uses acceptance and mindfulness processes, and commitment and behaviour change processes, to produce greater psychological flexibility."*

(p13: Steven C Hayes & Strosahl, 2004)

Psychological flexibility is established in ACT through a focus on the six core processes of the model (From Harris, 2006):

- **Cognitive defusion**: learning to perceive thoughts, images, emotions, and memories as what they are, not what they appear to be;
- **Acceptance**: allowing them to come and go without struggling with them;
- **Contact with the present moment**: awareness to the here and now experience with openness, interest, and receptiveness;
- **Self as context**: accessing a transcendent sense of self, a continuity of consciousness which is changing;
- **Values**: discovering what is most important to one's true self;
- **Committed action**: setting goals according to values and carrying them out responsibly.

Developments from the ACT model include Acceptance-Based Behaviour Therapy (ABBT; Roemer, Salters-Pedneault, & Orsillo, 2006), which lies more fully within the behavioural camp however it makes use of acceptance and mindfulness techniques. ACT and ABBT could be considered to have mindfulness as a relatively *'minor'* component of the intervention.

1.3.3 Metacognitive Therapy

Metacognitive Therapy (MCT) was developed independently by Adrian Wells, and also has its foundations in theory: the Self-Regulatory Executive Function (S-REF) model of understanding emotional disorder through attention processing (Adrian Wells, 2005). MCT proposes that a thinking style termed Cognitive Attentional Syndrome (CAS) lies at the foundation of *all* psychological disorders and was what prolonged and intensified negative emotion (Adrian Wells, 2008). CAS involves worry and rumination; threat monitoring; and maladaptive coping behaviours. It involves excessive focus on the content of our thoughts and on strategies to eliminate those

thoughts. MCT aims to shift our focus from the content of thoughts to the experience of the thoughts and how they are regulated. Therapy focuses on our metacognition – our thinking about our thoughts – rather than on the content of the original thoughts.

While this model holds a different explanation for why mindfulness may be effective, I think there is considerable overlap in clinical technique. Wells lists ten 'detached mindfulness' techniques (Adrian Wells, 2005) and I think they bare a striking similarity to techniques used in for example MBCT and ACT.

MCT could be considered to have mindfulness as a relatively *'minor'* component of the intervention.

1.3.4   Dialectical Behaviour Therapy

Dialectical Behaviour Therapy (DBT) is one of the most established and well-researched therapies that has a mindfulness component. DBT was developed by Marsha Linehan for a specific application: the treatment of Borderline Personality Disorder (BPD; Linehan, 1993). It is worth mentioning as it does employ mindfulness however it has not really been applied to treating the more common psychological difficulties outside of BPD.

DBT comprises four components. Mindfulness is one of these components, however it is recognised that mindfulness skills also feature in the other three components (emotional regulation, interpersonal effectiveness and distress tolerance). For example, 'observing the breath' is situated within the distress tolerance module.

A recent interesting study reported a component analysis of the skills used within DBT (Dewe & Krawitz, 2007). Of the 27 skills within the DBT manual, four from the mindfulness module appeared within the top 14 ranks. Another interesting study suggested mindfulness skills were some of the most frequently practised daily skills which had been taught as part of a DBT programme (Lindenboim, Comtois, & Linehan, 2007). DBT could be considered to have mindfulness as a relatively *'minor'* component of the intervention.

1.4 The active components of the therapies

To illustrate the practice of mindfulness and give a sense of how it may help provide therapeutic benefit, Table 1 details two common techniques within MBCT and their component parts. I have used MBCT to illustrate this as mindfulness is a major component of this therapy and MBCT has very much evolved within the cognitive paradigm. However the components are more generally applied across all of these therapies.

Table 1.   The active components of two mindfulness techniques

	How this component is achieved in example techniques:	
Component	**The body scan**	**Mindfulness of breath**
Attentional concentration	Focusing attention on a specific area of the body	Focusing attention on the breath
Attentional flexibility	Shifting this attention around the body	Following the in-breath and out-breath
Acceptance	When tension or difficulty is found, breathing in and focusing and staying with the difficulty. Experiencing pleasant and unwanted events.	Noticing and not trying to change the breath. Letting go of tension, no matter how small an amount
Present moment experience	Experiencing sensations in the body as they arise	"Riding the waves of the breath sensations"; staying with the breath for the full duration of the in- and out-breath
Change	Awareness of sensations ebbing and flowing	Paying attention to the changing patterns of sensations
Gaining perspective on mental processes, thought defusion'	Reconnecting with physiological experience	Noticing the wandering mind. This increases awareness of the nature of the mind. Paradoxically by paying attention to the breath, in the moment we catch ourselves having drifted we gain insights into the working of the mind.
Kindness (self-compassion to self and body)	When mind wanders, clients encouraged to gently bring attention back to breath	Gently, kindly escorting attention back to the breath. Attitude to meditation.

Mindfulness is a not a form of relaxation, although deep relaxation can and often is a by-product of it. There is considerable substance to the techniques themselves, with a number of 'active' components. Some of these components are inherently metacognitive. For example, being aware of ongoing and changing sensations

involves becoming aware of the flow of experience and the realisation that when we look at things in detail they are not as solid and permanent as we thought.

These techniques cultivate a more flexible and skilful approach to unwanted affect or other inner experience. A mindful approach can then lessen the impact these experiences have by, for example, providing more perspective so they are less compelling; reducing attachment and being able to let go and move on from unwanted thoughts; and seeing affective states as less permanent and solid. Furthermore, by for example training concentration it may provide further behavioural benefits.

1.5   How the therapies are linked

There are a number of common elements amongst these therapies. These include acceptance as there is a strong emphasis on this within ACT, MBCT and MBSR; MCT talks of the problem of avoidance; and DBT agrees with the importance of developing an accepting stance to one's inner experiences. The principal of defusion or distancing to achieve a meta-cognitive perspective on inner experience also seems to be a common principle. However it is achieved through different techniques: MBCT uses meditation, ACT uses metaphors, and MCT uses detached mindfulness.

Increased contact with the present moment is another shared element which again is achieved through different techniques. MBCT and MBSR teach meditation for this purpose; MCT uses attention training; and ACT and DBT use mindfulness practices. Finally the ability to shift attentional focus is a common element. MCT uses attention training. MBCT and MBSR use, for example, the body scan (letting go and moving onto the next area of the body) and meditation; for example letting go of breath and shifting attention to different sounds. ACT uses metaphors, paradox, and personal experience.

The purpose of Figure 1 is to help conceptualise the therapies included in this thesis; specifically their origins and the links between them. I recognise that there are many different ways to conceptualise the therapies and this is used mainly for illustrative purposes rather than as a definitive guide. There will inevitably be some blurring and

merging between the categories. Furthermore, the influences on the originators of the therapies are not always acknowledged or clear. For example, Adrian Wells would argue against meditation having had an influence on the development of MCT; despite the similarity between his detached mindfulness techniques and the acknowledged mindfulness techniques.

The dates indicated by the time line are approximate and based on the first publications of studies or articles detailing the therapy. To have a useful time scale on the diagram, meditation was left undated as its origins are considerably earlier than these therapies (see Section 1.7.1).

1.6   *How the therapies differ to traditional CBT*

In making sense of a mindfulness-based approach to psychological difficulties and how they may be effective in their treatment, it was helpful to consider the basic mindfulness model within the context of the wider cognitive-behavioural approach. CBT is an umbrella term for a large number of therapeutic approaches that all have a theoretical basis in the behaviourist and cognitivist traditions. Furthermore, the mindfulness-based therapies discussed here could be considered to be the latest technologies within CBT. For the purpose of this comparison therefore I will consider the more 'traditional' CBT approach which stems from Aaron Beck's conceptualisation of the model (Aaron T. Beck, 1976).

Using the example of depression, CBT and MBT have a number of similarities which are summarised here. Both recognise and differentiate between thoughts, behaviours and emotions; and believe working with thoughts and behaviours was possible and if done so, can help us cope with our difficult emotions. Both also aim to engender an increased awareness of these components and the effect they can have on the other components. Once this awareness was achieved they part company. In traditional Beckian CBT there is a significant emphasis on helping clients to *control* internal events for example by learning to challenge thoughts; whereas MBT aims to help individuals *accept* their internal events and let them be, recognising them as just thoughts or feelings or part of inner experience.

Time line

1960   *Behavioural therapy*

1970   *Cognitive therapy*

Meditation

1980   CBT   **MBSR**

1990

MCT   **DBT**   **ACT**   **MBCT**   **MB-EAT**

2000

ABBT   **MAP**

2009

'MINOR COMPONENT' THERAPIES   *'MAJOR COMPONENT' THERAPIES*
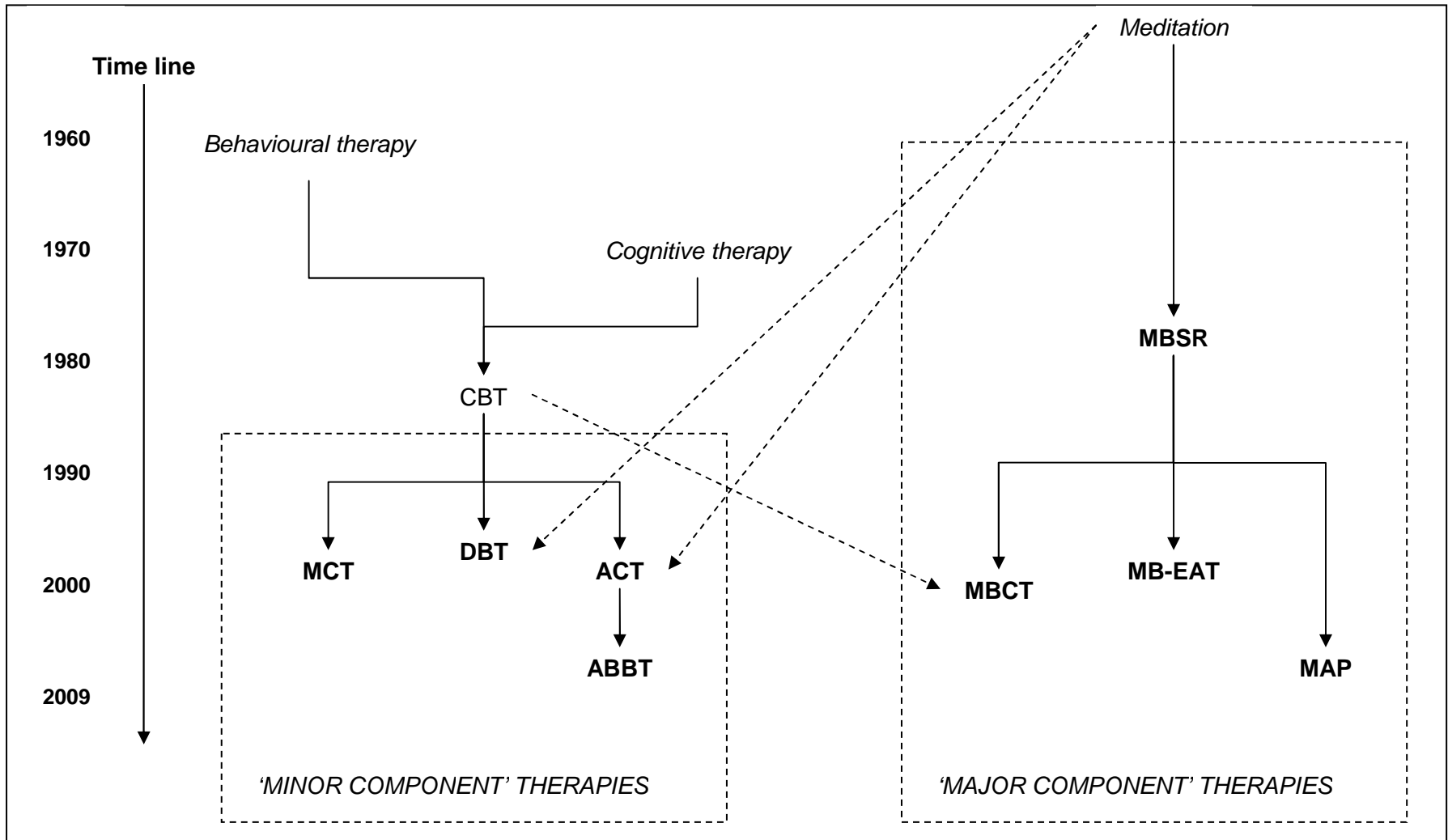
Figure 1.   **A conceptualisation of the origins and influences on mindfulness therapies**

A distinction is made between emotions that are appropriate and directly linked to an event, which are primary (or 'clean') emotions; and emotions that are a product of our response – through previous experience and learning, for example - to the primary emotion, which are known as secondary ('dirty') emotions (Steven C. Hayes, Luoma, Bond, Masuda, & Lillis, 2006). An event occurs, we may feel sad (primary emotion), however our struggle to not feel and accept this sadness – for whatever reason - causes us considerable additional suffering (secondary emotion). MBT aims to change the relationship with our internal events. It should also be recognised that while the *technique* differs they share the same goal: both approaches intend to reduce the negative impact we may experience from these events. To help conceptualise this idea the similarities and differences are shown in a *considerably* simplified way – and only for unwanted thoughts occurring through depression – in Figure 2.
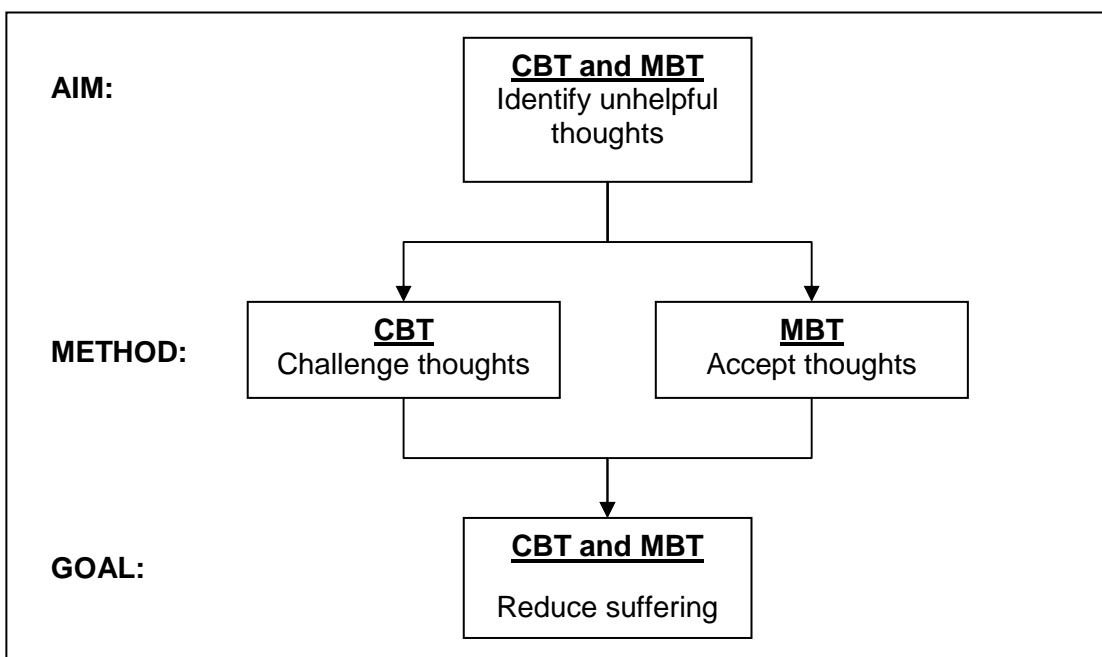
Figure 2.   Simplified process diagram for CBT and MBT approaches to unwanted thoughts in depression

In summary, MBT are thought to be effective through changing the relationship one has with unwanted thoughts. By not trying to change our inner experiences and by adopting a 'non-doing' or 'non-striving' mode through the practice of mindfulness, the purpose is to accept what is there rather than struggle to change it. By, for example, not 'buying' unwanted thoughts, it is reasoned that the thoughts will have a reduced

affective impact. Therefore by changing the relationship with inner experience it is likely that affective symptomatology will have a reduced effect. It is recognised that there is an inherent paradox in this approach: by letting go of one's attempts to control one's internal world - the control agenda – it actually gives more control of one's life. Similarly by adopting an accepting stance towards inner experience the influence of unwanted inner experience will be less.

1.7   The growth and development of mindfulness as a therapy

In the West, the 1960s and 70s saw considerable interest in meditation practise and the benefits this could bring to enhance otherwise healthy lifestyles. The link between mindfulness and meditation was explored in more detail below (See 1.7.1). The application of this practise to mental health can be seen to gain momentum through reporting in the late 1970s and early 1980s (Boorstein, 1983; D. Brown, Forte, Rich, & Epstein, 1982; D. P. Brown & Engler, 1980; Deatherage, 1975; Kabat-Zinn, 1982; Kabat-Zinn, et al., 1985; Kutz, Borysenko, & Benson, 1985). However it has only been in the last decade that this interest has achieved a critical mass, as shown in Figure 3. This graphs the number of articles retrieved from the psycINFO® database containing the word 'mindfulness' - in either the title or the abstract – by year of publication. Admittedly these data are crude as they will contain unrelated articles as the word mindfulness was sometimes used in different contexts; and it also does not give a sense of the content of the articles. However it does give some sense of the growth of interest in mindfulness as a therapy as this is a database for psychological therapies.

One of the most recognisable names associated with the development of a formalised *mindfulness* practice and technique for health problems – and so arguably the founding-father of MBT - is Jon Kabat-Zinn. In 1979 Kabat-Zinn founded the Stress Reduction Clinic at the University of Massachusetts Medical School. He also founded at the same University the Center for Mindfulness in Medicine, Health Care and Society. He began teaching Mindfulness-Based Stress Reduction (MBSR) at the inception of the Clinic in 1979 and the initial focus of MBSR was for the relief of chronic pain and stress-related disorders (Kabat-Zinn, 1982; Kabat-Zinn, et al., 1985).

Kabat-Zinn has a longstanding interest in the application of mindfulness practice to a wide audience and has believed that benefit can be conferred to society as a whole by encouraging 'healthy' individuals to practise mindfulness regularly. For example his Center for Mindfulness teaches the course to business and health care professionals.

As mindfulness gained momentum, it coincided with an important paradigm shift within psychology. In the 1950s the dominant paradigm within the field was behaviourism. This began to give way to cognitive models in the 1960s and 1970s. MBT is considered a development within the cognitive-behavioural tradition and a form of CBT. However it brings a different approach which Hayes described as the 'Third Wave' of behavioural and cognitive therapy (Steven C. Hayes, 2004): following on from behaviourism, the first wave; and cognitivism, the second wave.

The growth in mindfulness therefore coincided with the development of Cognitive-Behavioural Therapy (CBT) and the two have become inextricably linked. For this reason my focus was on the use of mindfulness within the cognitive-behavioural tradition. There have however been intriguing suggestions that mindfulness may be an underlying factor for all forms of psychotherapy (Jeffrey R. Martin, 1997; Jeffery R. Martin, 2002). This theory suggests that all therapies work by helping us to become more aware of our mental processes. It has also been suggested that rather than an entirely new endeavour in Western traditions, mindfulness can be understood with reference to the larger body of literature on emotional regulation (Hofmann & Asmundson, 2008). Mindfulness is a form of emotional regulation (Blackledge & Hayes, 2001; A. M. Hayes & Feldman, 2004) and so it is possible that reference to the literature on emotional regulation may help develop it. However what distinguishes MBT is that within the modern field of clinical psychology it is the first *therapeutic* use of techniques that address inner experience in this particular way.
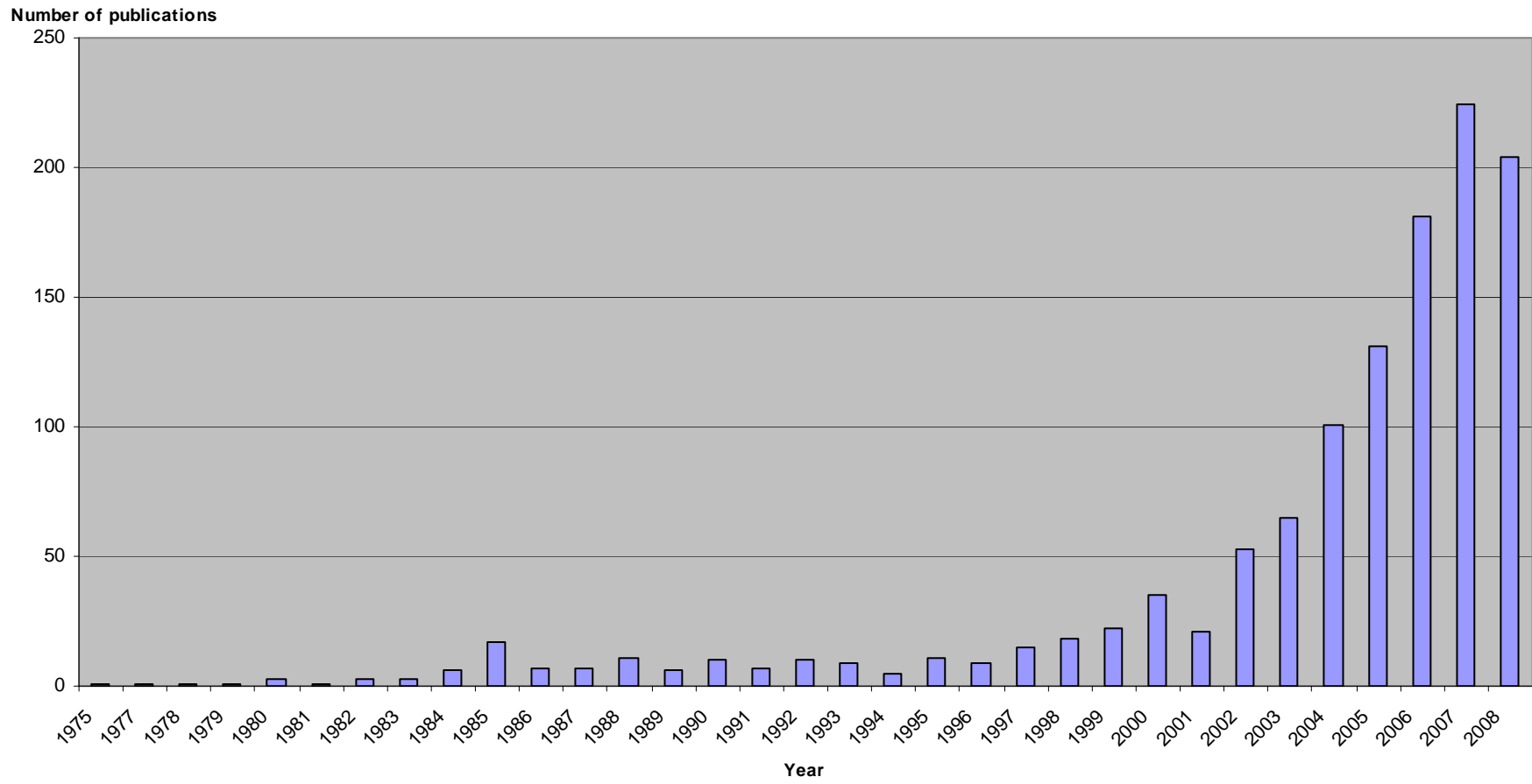
Figure 3.   Crude number of 'mindfulness' publications, by year 1975-2008

1.7.1 The link between meditation and mindfulness

Meditation is a tradition with a written history spanning over 2500 years with possibly the earliest descriptions of meditation techniques in Buddhist texts (Gombrich, 1988). In the modern era meditation is indeed most widely linked with the Buddhist tradition although it plays some part in most Eastern as well as Western religions (Delmonte, 1995; Flood, 2004; West, 1987).

Within the Buddhist tradition, mindfulness is just one of a number of techniques used within meditation. Other techniques include concentration, tranquillity and insight practices. Indeed some researchers categorise different forms of meditation practice as Concentration Meditation (CM) and Mindful Meditation (MM) (Lazar, 2005). Examples of these include focusing on the breath (CM) or paying attention to the input from one or more of the senses (MM), such as listening and experiencing the sounds come into and drift out of our awareness. I question whether this is a useful distinction as a number of MBT will employ a focus on the breath, which technically would be more accurately described as a concentrative meditation technique (J. C. Smith, 2004). For example, within MBCT a number of techniques are presented, some of which could be considered concentration meditation (focusing on the breath), others mindfulness meditation (focusing on sounds). However with some, they can be practised mindfully or with a concentrative focus (for example, walking meditation). Participants of the group programme are positively encouraged to use the techniques they find useful or see as appropriate for them.

1.8 Current evidence for mindfulness-based therapies

There is a growing body of evidence that mindfulness is an effective therapeutic intervention (Baer & Krietemeyer, 2006); however systematic reviews and meta-analyses are limited in number and scope. I will summarise systematic reviews of MBT for psychological therapies, which were published in the English language, in reverse chronological order. Firstly however, I will consider the purpose of evidence-based clinical practice.

1.8.1 Evidence-Based Clinical Practice

Within the National Health Service (NHS) today, Evidence-Based Clinical Practice (EBCP) is currently the standard for clinical work and supported by the work of the National Institute for Health and Clinical Excellence (NICE). NICE guidelines considerably influence practice (Kendall, Pilling, Whittington, Pettinari, & Burbeck, 2005) and indicate a clear need for methodologically rigorous, comprehensive reviews and meta-analyses of potentially useful therapies.

The importance of summarising evidence to better understand the effectiveness of treatments, and thereby inform further research, is clear. EBCP is the application of the scientific method to healthcare. It was defined as, "*an approach to decision making in which the clinician uses the best evidence available, in consultation with the patient, to decide upon the option which suits that patient best*" (Muir Gray, 2008). It has become the framework in which health care is practised within many countries and a hierarchy of evidence is used to inform decisions. The Centre for Evidence Based Medicine publishes – with regular updates – what is effectively a hierarchy of evidence (CEBM, 2009). NICE also has a similar evidence-grading scheme (see Appendix 6.2). At the top of these hierarchies are systematic reviews of randomised controlled trials (which often include a meta-analysis). Then individual randomised controlled trials, followed by observational studies. At present, clinical trials form the backbone of this system.

There is debate however over whether the clinical trial is an appropriate method for psychological therapies as it was designed for determining the effectiveness of pharmaceuticals. I think it is not possible to use *placebo* controls with trials of psychological therapies, however it is possible to offer other forms of control. Furthermore, it is important to consider other existing evidence to obtain a more comprehensive picture of treatment effectiveness. In psychological research, other designs are frequently used (Barker, Pistrang, & Elliot, 2002) and considered a useful addition to the evidence base. These include single-group designs; non-randomised two-group designs, which may use matched controls; and audits. While audits tend to have a lower external validity, they can inform the judgement as to whether a therapy is useful in everyday practice (high external validity). Reasons for this emphasis are often pragmatic. Funding is often a major factor for psychological studies (not supported by the wealth of pharma). However there may be strong ethical reasons for

not including a control group – such as there not being a position of equipoise in effectiveness between the intervention and the control.

Explanatory trials are studies of the *efficacy* of a treatment and give an idea of whether an intervention can work under 'ideal' conditions. Pragmatic trials, on the other hand, are where *effectiveness* is tested; that is whether the intervention appears to work in routine clinical care (Howard, Moras, Brill, Martinovich, & Lutz, 1996; Kazdin, 2003; Lambert & Ogles, 2003). Explanatory trials tend to optimise internal validity, with less 'noise' in the system, for example by having very strict inclusion criteria and not allowing the use of concomitant medication. They are often conducted in academic centres of excellence where treatment is of a very high standard, and so unfortunately not necessarily representative of practice in the wider world. If an intervention has equivalence with or outperforms another therapy, then it is deemed efficacious.

The generalisability of such trials to wider clinical practice – the effectiveness of an intervention - is limited. This is where pragmatic trials come in, often drawing their sample from a wider population, representative of the variety of the patient population likely to be treated and aiming to inform choices between treatments. In pragmatic trials inclusion criteria for example tend to be more lax, concomitant medication is generally allowed and the control is more often than not a placebo. The intention it to reflect the real world of practice and so the decisions faced by clinicians. Pragmatic trials often compromise internal validity in order to maximise external validity, particularly in community-based studies or audits: the challenge is to maintain a balance between the two. Both types of trials are useful in building the evidence base for a treatment and in practice, the distinction between them is often not clear cut. Clinical trials lie somewhere on a continuum between explanatory and pragmatic trials and so often have characteristics of both.

There is growing interest in using data from clinical practice to inform the decision-making process on best health care (Barkham & Parry, 2008). While there is debate about what constitutes good evidence, it is generally accepted that it is important to summarise evidence to better understand the effectiveness of treatments. This can then be used to guide practice and inform further research.

1.8.2 Published reviews

1.8.2.1 Öst (2008)

The most recent systematic review and meta-analysis which included a MBT was published by Öst (2008). This was a well structured, independent (Öst was not a practitioner of this form of therapy) review of what were described as the 'third wave' of behavioural therapies. Öst included therapies used as a primary treatment for psychiatric disorders and attempted to determine whether these therapies were empirically supported.

An interesting issue raised in Öst's article highlights some confusion in the use of the term 'third wave of behaviour therapies'. This phrase was used by Hayes (2004) to refer to therapies that take a mindfulness-based approach to dealing with unwanted thoughts and emotions. In Öst's article the criterion for third wave behaviour therapies seems to be those which emphasise the therapeutic relationship and acceptance. Therapies considered in Öst's review that overlap with Hayes' definition include Acceptance and Commitment Therapy (ACT) and Dialectical Behaviour Therapy (DBT). The remaining therapies considered do not – and include Cognitive Behavioural Analysis System of Psychotherapy (CBASP), Functional Analytic Psychotherapy (FAP) and Integrative Behavioural Couple Therapy (IBCT). While this debate does not render the article as unimportant to my review its definition does limit its relevance. Furthermore, Öst includes early studies by Zettle and Hayes (1986) and Zettle and Rains (1989) and categorises them as ACT. I think this was incorrect. These studies use technique which became incorporated in ACT and helped develop ACT as a therapy, however they do not include all of the theoretical and technical aspects we now understand as ACT. I think the eleven-year gap between the publication of Zettle and Rains (1989) and the next ACT study included in the review was evidence of this opinion.

The review included a total of 13 RCTs investigating ACT and concluded that there was evidence of a moderate overall effect size of 0.66 when all types of control group were included. This was comparable to Hayes et al.'s (2004) finding of an effect size of 0.68 (see 1.8.2.4) – however this effect size was not reported in the original article. The review also included 13 RCTs investigating DBT, finding a moderate effect size

of 0.58. Sub-group analyses found the effect size was much larger for the studies with waiting list control groups, reported as 0.96 for ACT and 1.30 for DBT.

Öst dealt with the problem of differences in study quality by matching these studies with contemporary CBT trials. He then assessed quality using a specifically developed scale based on one designed for studies of Post-Traumatic Stress Disorder (Tolin, 1999). Overall, Öst found the quality of the third-wave therapies to be somewhat less than for 'matched' trials of CBT.

In conclusion I think this paper raises some interesting issues about MBT and gives a useful overview of the effectiveness of two forms of MBT. However this narrow focus on two minor-component therapies and the inclusion of all trials of all psychiatric disorders together does limit the review.

1.8.2.2   Coelho, Canter & Ernst (2007)

Coelho, Canter & Ernst (2007) recently reviewed the use of MBCT for preventing relapse in depression; the condition for which the therapy was developed. They reported on controlled clinical trials only, which limits the review, and found two randomised and one non-randomised study. A further paper was included providing an additional analysis on one of the randomised studies, which was reported slightly misleadingly as it was presented separately from the other study. A synthesis of these data was not attempted however they concluded that there was evidence of MBCT providing additional benefit to standard care for the treatment of recurrent depression (three or more episodes). They also concluded that it was important to investigate the specific effects of MBCT; that is the contribution of the therapy components rather than for example the therapeutic relationship. The reviewers argued that it was not yet possible to draw firm conclusions from the presented data because the control groups could not control for specific components of the active treatment but only for the attention and expectancy effects towards improvement.

There were no language restrictions to the search, which covered a number of databases. To avoid publication bias the reviewers did not exclude dissertations or unpublished manuscripts. While this strategy ensured high sensitivity it was ultimately redundant: all of the included studies were published in English and in peer-reviewed

journals. By looking at depression only this review provides a narrow focus ensuring comparability of the studies. However this was a limitation at the same time: there was interest in the broader application of this technology and emerging evidence for its benefit in the treatment of anxiety (Evans, et al., 2008; Finucane & Mercer, 2006; K. Yook, et al., 2008), insomnia (Heidenreich, Tuin, Pflug, Michal, & Michalak, 2006) and bipolar disorder (Williams, Russell, & Russell, 2008). Some of these studies pre-date the review and may have been useful to include.

I think it was confusing to include reported outcomes that "indicate a change in mental health (including relapse data) or in symptoms, constructs, or *precursors* of a mental health problem" *(my emphasis in italics)*. Including pre-cursors seemed unusual particularly for a therapy aimed at relapse prevention. It appeared to be a post-hoc criterion (where a way was found to include interesting studies in the review). The "precursors to a mental health problem" they reported was measured by the Autobiographical Memory Test (Williams & Broadbent, 1986), which was used as an outcome in the study that presented a new analysis of a previously published trial. An overgeneralised categorical memory may play a part in depressive thinking however it is not clear whether it is a causal factor and may be a confounder, linked to dependent and independent variables. Overgeneralised memory may be a feature of depressive thinking rather than a cause.

Finally, in summarising the trials they did not report according to the internationally recognised QUOROM guidelines (Quality of Reporting of Meta-analyses; Moher, et al., 1999). These guidelines aim to increase the transparency of meta-analyses. Not all meta-analyses are the same and by detailing the method and rationale sufficiently it allows a better judgement of potential sources of bias introduced into the process. Decisions are made on, for example, which studies to exclude and it would have been useful to know exactly what these decisions were and what numbers were excluded. An example of this lack of reporting by Coelho, Canter & Ernst was failing to disclose the number of identified citations in the initial search.

1.8.2.3 Toneatto & Nguyen (2007)

This review (Toneatto & Nguyen, 2007) summarised the evidence for MBSR in the treatment of anxiety and mood disorders. They reported on controlled studies in the

literature and identified 15 which met their inclusion criteria. While the reviewers looked across clinical populations they focused on anxiety and depression outcomes. They did not attempt a meta-analysis with these data.

Similarly to other reviews, but to a much greater degree, the reporting does not follow QUOROM guidelines. Information was not provided on the search strategy, the search dates, the number of citations found, how many studies met their eligibility criteria or which reviewers undertook which tasks.

They raise an important point regarding adherence to the MBSR programme and provide an interesting summary of variations and adaptations to the original MBSR programme. However, they make no reference whatsoever to MBCT in the main text of the review (it appears only in the table of included studies). Although its developers admit it was derived from MBSR and shares about 80% of the course (Segal, et al., 2002), MBCT has clearly developed into a therapy in its own right. They have included two studies which investigated MBCT (Ma & Teasdale, 2004; Teasdale, et al., 2000). Furthermore, they illustrate the importance of clear, QUOROM-level reporting: assuming their aim was to include MBCT, they have overlooked some important trials in this area, (for example: Williams, Teasdale, Segal, & Soulsby, 2000). Although this study was not a unique population it was included in another review (Coelho, et al., 2007).

They concluded tentatively that there was no evidence for a reliable effect on depression and anxiety, although intriguingly a previous meta-analysis (Grossman, Niemann, Schmidt, & Walach, 2004) did find a reliable effect size for MBSR. Toneatto and Nguyen's review came to a different conclusion as they were more stringent on their inclusion criteria, with only controlled studies considered; and investigated a much narrower band of conditions. They did acknowledge that variation in methodological quality of the studies meant that a strong conclusion could not be drawn. I think consideration of the different clinical populations the studies recruited from would have been useful.

I think their inclusion of published trials only was useful, however in their conclusion they misrepresent the previous reporting of a review also investigating MBSR (Grossman, et al., 2004). Grossman et al.'s review included unpublished trials

however their reference list indicated four of the 10 included controlled trials were published. Toneatto and Nguyen comment that only one in 10 were published studies. This was a minor point, which may be a genuine mistake. However it was used to stress a point about their study and alongside the inclusion of MBCT, it raises questions about the thoroughness of their approach and their knowledge of these therapies.

1.8.2.4 Hayes, Masuda, Bissett, Luoma, & Guerrero (2004)

This descriptive review (Steven C. Hayes, et al., 2004) was of third-wave of *behavioural* therapies, and includes ACT, DBT and Functional Analytic Psychotherapy (FAP). Similarly to the Öst review (Ost, 2008) discussed above, it has linked ACT to therapies that share an emphasis on the therapeutic relationship rather than the mindfulness component; hence the inclusion of FAP. This review was a useful summary of the existing research. However they did not conduct a systematic review and analysis of the literature and I think this leaves open the possibility of bias in the conclusions. The lead author is the originator of ACT and it appears that the authors have not taken a neutral stance in their investigation. In their defence they are clear about the purpose of the article. They appear to be responding to a specific criticism of these being empirically supported treatments (Corrigan, 2001) and as such, they have responded to this criticism by providing evidence of effectiveness. They have focused on the positive gain from these therapies and while this was useful, they have not attempted to provide a critical appraisal of the evidence. I would agree with their conclusion that these forms of therapy show a commitment to empiricism, but I would disagree that this review alone addresses the concern that claims of effectiveness and empirical support were excessive.

The review of the evidence base for ACT included eight RCTs, two other forms of group studies and 14 case studies (mostly with $n = 1$). The review has been very inclusive of conditions, including studies of 'mathematics anxiety' and substance misuse (including smoking cessation); which are possibly unlikely to be the target of intervention for clinical psychologists within a mental health service. Similarly five RCTs of DBT for treating BPD were found, reporting benefit. Two studies of DBT for eating disorders were also found, again reporting benefit.

As stated no synthesis of these data was attempted however they conclude that there was evidence of effectiveness across a wide range of conditions. In this review all forms of study were considered and so a number of case reports were considered as representing a substantial contribution to the evidence base. They do however in the conclusion raise the issue of shortcomings in the evidence, including methodological quality, the depth of evidence within certain conditions and the clear need for more research.

1.8.2.5   Grossman et al. (2004)

Grossman et al.'s (2004) meta-analysis looked specifically at clinical trials of MBSR. It found 64 studies, however only 20 met the criteria specified by the reviewers. These exclusions were mostly made on the basis of quality of reporting (either of the intervention used or of the analysis). They found a similar (medium) effect size for both controlled and uncontrolled studies of approximately 0.5. The reviewers were very clear on the requirements of the studies in terms of the therapy provided, for example the review specifies course length, format. Furthermore, they required studies to operationalise a definition of mindfulness. This review's search strategy included studies up to December 2002 and was therefore over six years old at the time of my study. At a time when publications are growing rapidly it was likely that there have been additional studies to consider. Furthermore they included non-published studies. While this has some value, as these provided the majority of these data (six of the 10 controlled studies were unpublished), it indicated a sub-group analysis to determine whether this was linked to, for example, quality or outcome.

The reviewers included unpublished studies, identified though contact with first authors of published studies. This ensured thoroughness, however it may have led to studies being included which were not peer-reviewed. The review only considered the short-term response to therapy (pre- to post-intervention). They reported not including long-term follow-up data because of the variation in these data (different time points, not consistently reported across studies). This narrowed the focus and left important questions relating to the maintenance of therapeutic benefits unaddressed; thereby limiting the usefulness of the review.

Although not compulsory, the review did not adhere to QUOROM guidelines (Moher, et al., 1999) on the reporting of systematic reviews of clinical trials. Specifically they did not provide data on the number of citations identified by their search strategies and the number of citations from the different sources. This was not a major flaw of the review however it makes the review lose some transparency which is the purpose of the QUOROM guidelines; it makes it difficult for example to replicate or test the methodology of the review. Furthermore the search terms were limited with the search comprising of a total of five keywords or phrases.

My main concern with the analysis was their technique of aggregating across different outcome measures to calculate average effect sizes for the studies. This was useful in simplifying and making sense of a complex data set, however I think they have used far too crude a grouping and the subtleties of these data are lost. The review combined all outcomes for each of the studies into one of two groups: those measuring change in 'physical' health and those measuring 'mental' health.

I think one of the problems with this was that the categorisation could be debated. Examples of this included 'sleep', which may be an important part of depressive symptomatology however I would not consider it a measure of 'mental' health, as the authors did. Some scales, for example the SF-36, have explicit components that measure physical functioning and mental health. Social functioning on the SF-36 may be effected by physical *or* mental health. Also 'affective perception of physical pain' seemed to present considerable ambiguity for me in deciding whether it was 'physical' or 'mental' health (the authors considered it 'mental' health).

It is common practice to group outcomes to clearly defined categories – for example all outcomes measuring 'quality of life' are analysed together (that is the effect sizes are combined). However it was unusual to group disparate scales as the authors have done and I think this does limit the usefulness of the findings. For example, when translating this into clinical practice, stating MBSR has a positive effect on mental health in general is not as useful as knowing whether or not it would have an effect on depressive symptomatology.

1.8.2.6 Baer (2003)

This review (Baer, 2003) looks at MBT more generally; and included MBSR, MBCT, DBT, ACT and Relapse Prevention (RP). RP is a therapy that uses mindfulness techniques specifically for the prevention of relapse in substance abuse. Baer's review provides a useful summary of the area and I think by drawing parallels between therapies that employ mindfulness and combining them in a review, provided a more comprehensive understanding of the technology. One interesting element of this review was the categorisation of the therapies into 'interventions based on mindfulness training' (MBSR, MBCT) and 'interventions incorporating mindfulness training' (DBT, ACT and RP). A therapeutic approach left out was Transcendental Meditation (TM) and other 'concentration-based' approaches. Baer argues that TM primarily employs concentration meditation techniques and so falls outside of the scope of the review. I think the distinction within the more recent MBT is unclear as they do incorporate concentrative techniques. However I do think they fall outside of the scope of such reviews for other reasons: while TM arguably played a considerable part in paving the way and informing MBT, it was no longer of considerable therapeutic interest.

Those interventions that incorporated mindfulness training were required to specifically assess or control for the mindfulness component. As none were found that actually did this, these studies were not incorporated in the review. Of the MBSR and MBCT studies included, Baer reported many methodological flaws with the literature. For example, there was a paucity of controlled research; sample sizes were small; there were few descriptions of therapist training and evaluations of the integrity of the treatments; and a lack of focus on clinical significance. However the reviewed studies did show promising evidence in the treatment of several disorders.

Insufficient information was provided on the methodology of the review, as was common with many other of the reviewers detailed here. The search dates, the number of citations found and the number or details of those not meeting the eligibility criteria were not provided. Baer summarises the studies by clinical population, which was a very useful categorisation. The analysis of these included studies was limited.

Baer reported effect sizes for the studies separately. Baer then used basic meta-analytic technique and averaged these effect sizes, weighted by the sample size. The post-treatment effect sizes ranged from 0.15 to 1.65 and the overall mean effect size was 0.74 (standard deviation (SD) = 0.39). When weighted by sample size the post-treatment effect size was 0.59. Follow-up data were reported less often. The mean effect of the available follow-up effect sizes ranged from 0.08 to 1.35, with a weighted overall mean of 0.59 (SD = 0.41).

I felt that the approach to follow-up data made the meaning difficult to interpret. Multiple follow-up points were averaged within studies before combining and across studies represented a follow-up window of 2-36 months. This was a very broad time period and I think should have been grouped to similar periods, so as to be intuitively more comparable. For example, the clinical significance of a two-month in contrast to a three-year follow-up is very different. A minor technical point relates to the reporting of standard deviations for the pooled effect sizes. I think a more useful (and conventional) statistic would have been the 95% confidence interval.

Baer provided some very useful sub group analyses and concluded that the effect may be greater for Axis I disorders (American Psychiatric Association, 2000) as compared to chronic pain or medical problems. One final point Baer made was that the clinical significance of the estimated effect sizes was difficult to assess because of the poor reporting of data within individual studies (for example, where there was no mention of the pre-treatment score). For this and other reasons, increased methodological rigour in future studies was recommended.

1.8.2.7 Other miscellaneous reviews

A number of other systematic reviews of MBT have been published, however they fall outside of the scope of this review and so have not been summarised here. For example Smith et al.'s paper (2005) systematically reviews the use of MBT as a supportive therapy for cancer care; a physical health condition. Hayes et al. (Steven C. Hayes, et al., 2006) summarise the evidence base for ACT. However this was actually a very broad overview of the ACT literature and does not focus specifically on outcome studies. For example, it includes a number of surveys that administer

ACT specific outcome measures (Batten, Follette, & Aban, 2001; Begotka, Woods, & Wetterneck, 2004).

Wells' paper on MCT (Adrian Wells, 2008) provides a descriptive review of a number of studies claiming effectiveness for his therapy. The Melbourne Academic Mindfulness Interest Group published a useful overview of the area (Allen, Blashki, & Gullone, 2006) however the section on empirical evidence was very short, comprising of a very limited description of select papers. However they conducted a useful overview of potential adverse consequences of meditative practises (See 1.8.4).

Two other interesting reviews investigated meditation techniques and included for example different forms of yoga. This broad scope limits their usefulness here. These were a Cochrane systematic review of meditation therapies for anxiety disorders (Krisanaprakornkit, Krisanaprakornkit, Piyavhatkul, & Laopaiboon, 2006) and a systematic review of meditation techniques for medical illness (Arias, Steinberg, Banga, & Trestman, 2006).

1.8.3   Summary of existing reviews

Published reviews of MBT suggest there is considerable evidence for their effectiveness. However these reviews have either been of only one or two specific forms of the therapy (Coelho, et al., 2007; Grossman, et al., 2004; Steven C. Hayes, et al., 2004; Ost, 2008); or were systematic reviews employing at the most some basic meta-analytic techniques rather than including a rigorous meta-analysis (Baer, 2003; Toneatto & Nguyen, 2007). Furthermore, some of these reviews focus on specific conditions; not necessarily focused on psychological health. These factors limit their scope.

I think these therapies share considerable common ground and form the leading edge of technology within the cognitive-behavioural tradition. There was a clear rationale for considering the therapies together to address the question of whether mindfulness is a useful treatment modality.

1.8.4 The safety of mindfulness-based therapies

Shapiro (1992) authored an interesting early study of adverse mental and physical health events amongst long-term meditators. An adverse event was defined by the practitioner reporting the event and he found a very high prevalence. The criteria meant an adverse event could, for example, be regarded as an increased awareness of negativity within oneself; and it could be argued that this is one of the purposes of mindfulness. Shapiro does recognise this and comments that adverse events can be transformed over time. I think this limits the usefulness of the study. However an important point it made was highlighting the skills of the therapist required in containing and addressing difficulties arising from the practice.

This point was further reinforced by Walsh and Roche (1979): a seminal paper which originated the thinking that meditation was contra-indicated by psychosis. This paper was actually a report of three cases where psychotic episodes occurred after engaging in intensive meditation. Firstly, as the authors state, a *"combination of intensive meditation, fasting, sleep deprivation, a history of schizophrenia, and the discontinuation of maintenance doses of phenothiazines can be hazardous"*. I couldn't agree more; however case reports are uncontrolled and it was unclear which of these components – or which combination of these components - were responsible. Case reports are anecdotal and while they can make associations, definitive conclusions cannot be drawn. Furthermore, the intensive meditation may have been an attempt at self-medication during the early signs of a psychotic episode. What it does clearly show is the need for an appropriately trained practitioner to work therapeutically with a mental health population.

A useful overview of MBT by the Melbourne Group (Allen, et al., 2006) drew attention to some important issues for practitioners. These included the opportunity cost of time invested in practice. For example, the MBCT course (Segal, et al., 2002) encourages significant home practice – 45 minutes a day – while undertaking the course and the expectation is that practise ultimately becomes integrated into daily activity. The Melbourne Group also considered exacerbation of symptomatology and recognised how an increased awareness of internal processes may pose difficult challenges for some individuals. However they concluded that this difficulty is often a stage that is passed through and again highlighted the importance of training from skilled practitioners.

There was an absence of serious concern in the literature about the safety of mindfulness practices and a paucity of adverse event reporting. This suggests an overview of safety was not currently indicated.

1.9 Rationale, aims and objectives

1.9.1 Rationale

MBT are increasingly used within the field of mental health, particularly by clinical psychologists (Didonna, 2009; Germer, Siegel, & Fulton, 2005; Steven C. Hayes & Strosahl, 2005; Mace, 2008; Segal, et al., 2002). Proponents of MBT consider their application to be broad. For example, a recent guide on the practical application of Acceptance and Commitment Therapy (ACT) includes chapters on affective disorders; anxiety disorders; post-traumatic stress disorder; stress more generally; substance abuse and dependence; working with the seriously mentally ill; and working with chronic pain patients (Steven C. Hayes & Strosahl, 2005). Furthermore, MBT seems to be a useful intervention for individual as well as group work (Segal, et al., 2002) and across the lifespan (Didonna, 2009).

The current and growing importance in the field of clinical psychology was further evidenced by the number of journals that have recently devoted or included a substantial element of an issue to a particular form of or all MBT. These include the *Journal of Clinical Psychology* (65(6): 2009); *Behavioural and Cognitive Psychotherapy* (36 (special issue 06): 2008); the *Journal of Rational-Emotive & Cognitive-Behavior Therapy* (23 (2) and 23 (4): 2005; and 24 (1): 2006); *Behavior Therapy* (35(4): 2004); and *Clinical Psychology* (10(2): 2003; 11(3): 2004; and 15(4): 2008).

In today's NHS there is a clear need for professional practice to be empirically supported and organisations such as NICE facilitate this for the NHS. One method of determining whether therapies are effective is through meta-analysis. Reviews summarising specific forms of MBT have been conducted but are limited in scope (Baer, 2003; Coelho, et al., 2007; Grossman, et al., 2004; Steven C. Hayes, et al., 2004; Ost, 2008; Toneatto & Nguyen, 2007). A comprehensive systematic review and meta-analysis of MBT would therefore provide a more rigorous overview of the

effectiveness of MBT. Additionally such a review may help indicate possible moderators of effect to inform future research.

Psychological problems are common, with the point prevalence of any 'neurotic disorder' estimated at 17.3% for working-age adults in Great Britain (ONS, 2000). Current treatment options for anxiety and depression are effective and the benefit of psychological therapy was recognised (McIntosh, et al., 2004; NICE, 2007); however current treatment options are not effective for everyone.

In developing guidelines for the treatment of commonly seen psychological difficulties the focus is on the reduction of symptomatology. Arguably, clients approach mental health services also see symptom reduction as an important indicator. It was hypothesised that MBT can reduce affective symptomatology through changing the relationship with this symptomatology.

1.9.2 Aims

The primary aim of my study was to contribute to ongoing research determining the effectiveness of MBT for the treatment of affective symptomatology. Depending on these results, a secondary aim of the study was to make recommendations for the use of MBT in clinical practice.

1.9.3 Objectives

The objectives of this research were where possible to:

- To undertake a comprehensive search of the literature to identify relevant studies that have employed experimental and non-experimental designs of MBT in treating psychological health conditions;
- To undertake a systematic review of this literature;
- To evaluate the effectiveness of these therapies through the meta-analysis of relevant studies;
- Where indicated, to undertake subgroup analyses of those relevant data to investigate possible moderators of effect;
- To make recommendations on research;
- To make recommendations on practice.

1.10 Chapter summary

In this chapter I have shown how mindfulness-based therapies have been introduced within clinical psychology over the past 30 to 40 years and there development within the cognitive-behavioural tradition. Their origins go back much further, to meditative traditions dating back thousands of years. Taken as a whole, these therapies aim to alter the relationship with difficult inner experience to alleviate the distress these experiences cause. One way of conceptualising these therapies is by grouping them according to how whether mindfulness is a major or a minor component of the therapy. There is a growing evidence base for these therapies however reviews of these therapies have not comprehensively summarised their effectiveness. My aim is to contribute to this evidence base and inform practice, by conducting a meta-analysis of these therapies.

2. Methods

2.1 Chapter outline

Before conducting a meta-analysis, there are a number of essential stages which need to be carefully conducted. These lend methodological rigour to the analysis and support conclusions drawn from it. These stages include a comprehensive search of the literature, so that all relevant studies are included; and the strict application of eligibility criteria. Finally consideration is given to relevant methodological and data issues in conducting a meta-analysis.

2.2 Systematic literature search

Two databases were searched and the details are presented in Table 2. PsycINFO® is an electronic abstract database and possibly the leading source of abstracts in the psychological literature. It is compiled by the American Psychological Association and has over 2.6 million records (APA, 2009), with records dating back to the 1800s. PsycINFO® is updated weekly and includes journal articles (over 2150 titles, which make up the majority of records) as well as book chapters.

MEDLINE® is the leading electronic bibliographic database of articles in the life sciences and biomedical fields. It is compiled by the National Library of Medicine in the United States and has over 16 million records from over 5200 journals (NLM, 2009) with weekly updates. The main coverage is from 1949 onwards.

Table 2. Details of databases searched

Database	Source	Date range of search	Date search conducted
PsycINFO®	Ovid SP	1967-January Week 1 2009	09/01/2009
MEDLINE®	PubMed	1967-January Week 1 2009	10/01/2009

Comprehensive search strategies were developed to be run on these databases services to ensure relevant studies were identified. These search strategies (see 6.3.1) employed searching of text words and index terms. The extent of a text word search depended on the database and could include words or phrases in the title of the article, the abstract, the key concepts supplied by the authors or journal, or

the table of contents. Index terms, such as MeSH terms in Medline, were added when the citation was added to the database.

These strategies comprised of two components: to find 1) relevant therapies, and 2) appropriate study designs. The first component of the search strategies looked at mindfulness-based therapies (MBT) and searched for the names of the therapies as a text word. Examples included, "mindfulness", "MBCT" and "MBSR". These therapies generally did not have terms in the index therefore this component relied on this text word searching. An exception was that "mindfulness" was an indexed term in the psycINFO® database.

The second part of the strategy was used to identify appropriate study designs. Indexing of study designs was more advanced on the databases and this component of the search could employ these indexing systems. However this relies on accurate coding by the database and so this component of the search did not rely entirely on the indexing system and also used text words. For example, "clinical trial" was searched for as an index term and as a text word.

The search strategies then combined these two components. Citations found were retrieved if they contained an appropriate therapy term AND an appropriate methodology term or keyword. The search strategy aimed to find:
- all experimental studies;
- of any type of therapy with a mindfulness component.

The search strategies were developed with reference to that employed by the specialised trial register search of the Cochrane Collaboration Depression, Anxiety and Neurosis Group strategy (Churchill, et al., 2009). All retrieved studies were added to and managed within Endnote® bibliographic database software (version X2).

At this stage of a systematic review sensitivity is more important than specificity. It was more important to cast the net wide and risk finding more studies in the search which are not eligible; that is to have a high false-positive rate. The alternative was less desirable; that is to *not* find studies which would be included in the review and have a high false-negative rate.

Additionally, "snowball" methods (Greenhalgh & Peacock, 2005) were used. These included scanning the reference lists of potential studies and published reviews to identify further references.

2.2.1 Number of citations identified

A total of 687 citations were identified in these searches. 89 of these citations (13% of those identified) were duplicates, where the same article was retrieved from both of the databases, and were removed. Most of these duplicates were identified by the Endnote® software however some were identified by hand. These were typically where the journal title was abbreviated in one citation but written in full in the other.

598 unique citations were identified through the search strategy and were screened. These figures are detailed in the flow diagram below (Figure 4), which also shows the other stages of the study selection procedure.

2.3 Screening

The initial screening involved reading the abstracts – or title only if no abstract was available - of identified citations to determine whether they *could possibly meet* the eligibility criteria. This represented a broad screen of the identified studies and it was anticipated that this would remove a considerable proportion because of the broad search criteria. The aim was to confirm - where possible - that citations were of:

- An experimental study;
- of any type of therapy with a mindfulness component;
- for any type of psychological condition; and
- treating a population commonly seen within mental health services.

Figure 4.   Flow diagram of study selection procedure

Identification	**Citations identified through database searching** **n = 687**
	Number of duplicate citations removed **n = 89**
Screening	**Citations screened** **n = 598**
	Number of citations removed **n = 485**
Eligibility	**Articles assessed for eligibility** **n = 113**
	Articles excluded n = 73 *Diagnosis unclear (n = 3)* *Intervention (n = 3)* *Language (n = 2)* *Outcomes (n = 2)* *Participants (n = 54)* *Publication type (n = 1)* *Study design (n = 6)* *Data inappropriate (n = 1)* *No study reported (n = 1)*
Inclusion	**Included in meta-analysis study pool** **n = 40**

Two researchers (RM and SW) completed an initial screening separately. A form was designed for this purpose which listed the citations (see 6.3.2) and a separate file used which detailed the citation and abstract. The electronic versions of these were used. All studies were given a reference number at this stage to facilitate the process. For studies to be retrieved both researchers had to agree. An attempt to reconcile differences of opinion was made through discussion until a consensus was reached. If a consensus could not be reached then a third researcher (AG) arbitrated. As these data were held electronically they were easily exported into an Excel® spreadsheet to manage the process of reconciliation through the use of the formulas available within Excel.

Categories used in this initial screening were either YES (retrieve and assess for eligibility) or NO (do not retrieve). Additionally, systematic reviews were flagged up to enable a hand search of their reference lists (note that these were indicated as excluded articles).

2.3.1 Number of studies identified in the screening

Of the 598 citations screened, there was an initial agreement between the two reviewers for 547 of these citations (of which, 90 were to be retrieved and 457 excluded). Of the remaining contested 51 citations, consensus was reached at a meeting without resort to a third-party. It was agreed to retrieve 22 further studies from this 51 and exclude 29.

Overall, it was agreed to exclude 485 studies at this stage and advanced 113 to be assessed for eligibility (see Figure 4). The number of studies excluded at this stage was therefore considerable and represented 81% (485 of 598) of the citations entered into the process. This was expected because the initial search strategy was intentionally broad and so for example included many discussion articles which did not feature original study data. The information presented within the title and abstract was limited and if there was any doubt over the content of the article it was retrieved.

2.4   Eligibility

The next stage involved the application of the eligibility criteria to the studies. Copies of all of the 113 articles were retrieved to enable this. These were obtained from either the University of Hertfordshire's Learning Resource Centre (LRC); through the LRC Inter Library Loans facility; or through email contact with the authors.

A form was designed and completed electronically to record the eligibility assessment (see 6.3.3). Two researchers (RM and SW) each conducted this stage and their respective decisions were compared, again using formulas within Excel®. The completed forms allowed the reviewers to detail the reason for exclusion, to allow discussion of differences of opinion and ultimately to detail these reasons (see Figure 4). These were discussed and where a consensus could not be agreed, a third reviewer (JS) reconciled these differences. German language articles were assessed for eligibility by one reviewer (JS).

At this stage linked papers, where two or more papers reporting on the results of one study, were flagged up. This could include, for example, a paper reporting additional follow-up data from an earlier study.

2.4.1   Eligibility criteria

2.4.1.1   Language

The review included studies published in either English or German only as we were limited to the languages members of the project team were proficient in. Where potential studies were identified that were published in other languages, an author was contacted to determine whether an English translation of the article existed.

2.4.1.2   Publication type

Dissertations were not included in the review. To have included dissertations would have required manuscripts to be obtained from authors or other institution's libraries. It was anticipated that this would involve considerable resources and would not necessarily be possible within the time scale of this study. Furthermore it was

anticipated that at least some of these studies were subsequently reported in peer-reviewed journals.

All other types of publication were considered and it was anticipated that the majority of studies would be published in peer-reviewed journal articles and book chapters.

2.4.1.3 Study design

Studies that employed an experimental (interventional) research design were included. The following types of trials were included:

1. Randomised Controlled Trials (RCT)
2. Quasi-Randomised Controlled Trials (QRCT)
3. Non-Randomised Controlled Trials (NRCT)
4. Uncontrolled Trials (UT)

Other non-randomised experimental designs were also considered for inclusion. This included designs described as for example one-group post-test only; one-group pre-test post-test; non-equivalent groups post-test only; and non-equivalent groups pre-test post-test (Barker, et al., 2002).

Other types of studies which included pre-post designs - but perhaps more accurately described as 'audits' - were also included. It was possible to obtain an effect size for relevant outcome variables for these studies and so include them in the analysis. Furthermore and more importantly, they represented effectiveness research as they were all conducted in typical clinical settings. While these studies may compromise their internal validity, they maximise their external validity. They will more accurately reflect usual clinical settings in terms of for example the competence and experience of the therapists or the selection of participants.

Individual case studies and reports or other forms of anecdotal research were excluded. Single case designs were excluded and an example of these designs was described as (non-concurrent) multiple baseline across participants design.

2.4.1.4 Participants

In the present study I was interested in the overarching question, "as a clinical psychologist, could Mindfulness-Based Therapies (MBT) help me in my routine clinical work within a mental health setting?" The focus therefore was on Axis 1 disorders within the DSM-IV framework (American Psychiatric Association, 2000) and therefore personality and other Axis 2 developmental disorders were excluded.

Studies investigating MBT for any psychological condition were therefore included, where a formal diagnosis was explicitly stated and defined using appropriate criteria (which was acceptable at the time of publication of the study). Psychological conditions of any severity and chronicity were considered for inclusion, where the population was a clinical sample.

Studies where the primary diagnosis was a physical condition were excluded even if they investigated psychological outcomes. It was acknowledged that an important area of clinical psychology work was that conducted within physical health settings; however these were considered beyond the scope of this study. Furthermore, as I was approaching it from a mental health perspective my primary interest was in psychological conditions. Within the physical health setting the primary condition will often remain following treatment and it is possible that the mechanism of action – or at the very least the intention behind therapy - may be different.

Studies investigating treatments for substance misuse or withdrawal of any type (for example alcohol, nicotine and banned substances) were not included. Often clinical psychologists tend only to be involved working with these clients within specialist services and if the misuse forms part of a dual diagnosis. Again in these settings the intention behind therapy may be different. Some studies may look at, for example, reducing anxiety symptomatology. However this would again represent a grey area where it could potentially be difficult to clearly differentiate the aims of the research.

Studies involving people with learning disabilities were not included as typically they are seen within more specialist services. There are some studies implementing and assessing the role of mindfulness within LD services, however problems encountered with people with LD may have very different aetiologies and furthermore, the mindfulness techniques may need to be adapted considerably.

In summary these populations raise interesting questions about the applicability of MBT which subsequent research should address. However they fall outside the scope of this review. I was interested in studies for which it was clear that the primary diagnosis was psychological and the work was typical of general mental health services.

2.4.1.5 Sample size

Studies of sample sizes greater than five participants were considered for the review.

2.4.1.6 Intervention

Any type of therapy which included mindfulness was considered for inclusion. Mindfulness-based treatment approaches included more recently developed forms, such as (but not limited to):

- ABBT   Acceptance-Based Behaviour Therapy
- ACT   Acceptance and Commitment Therapy
- BCP   Buddhist Counselling Programme
- DBT   Dialectical Behaviour Therapy
- MAP   Mindful Awareness Practises
- MBCT   Mindfulness-Based Cognitive Therapy
- MB-EAT   Mindfulness-Based Eating Awareness Training
- MBPM   Mindfulness-Based Pain Management
- MBSR   Mindfulness-Based Stress Reduction
- MBRE   Mindfulness-Based Relationship Enhancement
- MBRP   Mindfulness-Based Relapse Prevention
- MBT   Mindfulness-Based Therapy
- MCT   Meta-Cognitive Therapy

2.4.1.7 Control group

If applicable, control or comparison groups typically included active (experimental) treatments; placebo; waiting-list controls; treatment as usual; no-treatment controls; and attention control.

2.4.1.8 Observation period

Studies were included if the duration of treatment – that is the mindfulness intervention - was of one week's duration or longer. Follow-up needed to occur at either the end of treatment or after the treatment had finished.

2.4.1.9 Outcomes

Studies were included with at least one outcome measuring subjective change in symptomatology. All additional outcome measures of any type of objective or subjective parameter were included. All scales used needed to be validated and reliable. It was anticipated that these outcomes would include subjective as well as objective measures such as:

- Disease-specific measures of functioning;
- Treatment-specific measures of outcome;
- Generic measures of functioning (e.g. general health);
- Quality of life measures;
- Physiological markers.

Side effects and the acceptability of treatments (as measured by withdrawals) were not considered.

2.4.2 Number of eligible studies

113 articles were retrieved and assessed for inclusion and a total of 40 studies (35% of those assessed) met the inclusion criteria. These studies formed the 'pool' from which studies were drawn for the meta-analysis. This number represents 6% of those initially identified.

A total of 73 studies were excluded and the reasons were detailed in Figure 4. Most of these studies (74% of the exclusions; n = 54) were excluded because of the sample. Within this category the majority of studies were of DBT where the participants were diagnosed with Borderline Personality Disorder (an Axis 2 condition within DSM-IV).

2.5 Data extraction and coding

When the list of included studies was finalised relevant data were extracted. These tables summarised details of the study and the first table detailed general information and the second outcome data. Only the studies actually used in the analysis are detailed in these tables in the appendix (6.4). The variables used in the analysis were listed below and where relevant, the coding categories or units used in the analysis were provided.

- Study design
    - Randomised Controlled Trial
    - Two groups (not randomised)
    - Single group (pre- and post)
    - Audit
- Duration of therapy and follow-up (weeks)
- Diagnostic category
    - Anxiety
    - Depression
    - Attention Deficit and Hyperactivity Disorder (ADHD)
    - Eating disorder
- Diagnosis (including criteria used)
- Total number of participants, by group
- Mean age of sample
- Percentage of females in study population
- Intervention and whether mindfulness was a major or minor component
    - Major component therapies included:
        - Mindful Awareness Practices (MAP)
        - Mindfulness-Based Cognitive Therapy (MBCT)
        - Mindfulness-Based Eating Awareness Training (MB-EAT)
        - Mindfulness-Based Stress Reduction (MBSR)
    - Minor component therapies included:
        - Acceptance-Based Behaviour Therapy (ABBT)
        - Acceptance and Commitment Therapy (ACT)
        - Buddhist Counselling Programme (BCP)
        - Meta-Cognitive Therapy (MCT)

- Therapy format
  - group
  - individual
- Number and duration (hours) of sessions
- Number of therapists and whether training detailed (yes/no)
- Control group(s)
  - None
  - Waiting List (WL)
  - Treatment as Usual (TAU)
  - maintenance Antidepressant Medication (a ADM)

Outcome measures were recorded separately, which included the time-point of the measurement (pre- or post-treatment; or follow-up). Where given, means and standard deviations (SD) were recorded to allow for effect sizes to be calculated.

2.6 *Methodological and data considerations in meta-analysis*

Meta-analysis is a statistical approach to combining the evidence from a number of studies. Through meta-analysis important research questions can be asked of these data. However meta-analysis is a science in itself and much consideration needs to be given to its method.

2.6.1 Non-randomised designs

It is the subject of some debate as to whether meta-analysis should include non-randomised studies. Differences have been found between meta-analysed randomised and single-group designs (Ioannidis, et al., 2001; Linde, Scholz, Melchart, & Willich, 2002) however these reviews do not consider it inappropriate when interpreted with caution. Often the decision was a pragmatic one: if the majority of the evidence was comprised of single-group designs then this must form the basis of an overview of that evidence.

From the overview of existing systematic reviews (see 1.8.2) it was anticipated that there will be a considerable mixture of independent-group randomised, controlled

trials; and single-group, pre-test post-test designs. If these are to be included, consideration must be given to the statistical methodology.

It is acceptable to combine effect sizes from these different study designs if certain statistical considerations are addressed (Morris & DeShon, 2002). Morris and DeShon (2002) recommend that this includes that common metrics are used for the studies whose effect sizes are combined; so for example the standard deviation of change scores was used for all studies rather than this mixed with the standard deviation of the baseline score. Secondly, the effect sizes should be comparable (homogeneous) in terms of treatment effect. Finally, studies should be weighted to account for differences in precision.

2.6.2 Effect sizes

An effect size is a way of quantifying the effectiveness[2] of an intervention. It can be calculated and expressed in a number of different ways and one fundamental distinction is whether the effect size was standardised or not.

Ideally, the studies included within a meta-analysis will have presented within their statistical analyses the correlation coefficient of the scores between time points such as pre- and post-treatment. Unfortunately reporting of studies was not always comprehensive and this information was often not included in reports. This statistic is required to calculate the standard error which was used to weight the effect sizes. In cases where the correlation coefficient was not presented an assumption was made for its value as there was no agreement on what this value should be (Lipsey & Wilson, 2000). For the purpose of this meta-analysis, this value was assumed to be 0.6; which corresponds to a 'good' correlation.

2.6.2.1 Mean reduction score

For those analyses for which only the intervention group was considered in the effect size, and where the exact same scale was considered together, the mean reduction score – using the raw scores - was used as the effect size. The mean reduction score

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[2] The term 'effectiveness' in this thesis includes the notion of efficacy as well as effectiveness.

is the difference in *raw* data means between two outcome points, typically pre- and post-treatment (Lipsey & Wilson, 2000). As this effect size was employed where the same commonly used outcome measure was used across all of the studies an unstandardised reduction score such as this has a corresponding clinical meaning. An example of this was the Beck Depression Inventory (BDI; A.T. Beck, Steer, Ball, & Ranieri, 1996), which was one of the most widely used measures of depression.

The formula for the mean reduction score:

$$= M_1 - M_2$$

And the calculation for the weight:

$$= 1 / \sqrt{(2((SD_1^2 + SD_2^2) / 2)(1 - 0.60) / N_t)^2}$$

Where:

M_1 and SD_1 the pre-treatment mean and standard deviation

M_2 and SD_2 the post-treatment mean and standard deviation

N_t the number of participants in the treatment group

In those analyses where data for an intervention group and a control group were compared the unstandardised reduction score was also used. For each of the studies, the raw mean difference in the change scores between the intervention and control groups was the effect size.

2.6.2.2 Standardised reduction score

A large number of rating scales exist for measuring outcome. If all studies included in the analysis did not use the same outcome measure they were grouped according to the condition being measured. For example, anxiety scales were grouped together. In this case the different scales were standardised to allow a direct comparison and the effect size used was the standardised reduction score.

The formula for the standardised reduction score, with the same notation as used above, was:

$$= (M_1 - M_2) / \sqrt{((SD_1^2 + SD_2^2) / 2)}$$

And the calculation for the weight:

$$= (\, 2\, N_t\, /\, ((\, 4\, (1 - 0.60)) + (M_1 - M_2)\, /\, \sqrt{((SD_1^2 + SD_2^2) / 2)}^2\,)$$

2.6.2.3   Hedges' g

For the analysis which considered the effect size in the intervention group relative to the control group, a standardised mean difference was calculated, specifically Hedges' *g* statistic (Hedges & Olkin, 1985).

The formula for Hedges' *g* was:

$$g = (M_t - M_c) / S_p$$

Where:

M_t = the mean of the treatment group on a specific measure

M_c = the mean of the comparison group on that measure

S_p = the pooled estimator for the variances

2.6.3   Fixed, random and mixed effects models

Fixed and random effects models are different approaches to the calculation of overall effect in a meta-analysis, which differ in their assumptions about the population(s) the studies are drawn from (Hedges & Vevea, 1998; Hunter & Schmidt, 2000). Fixed effects models assume the studies are all drawn from the same 'super' population – and so there will be a one, fixed effect size. Differences between the effect sizes in the studies are understood to be due to random variation around this fixed effect size. This is contrasted with the random effects model which assumes studies are drawn from different populations and so as a result, the effect of the intervention within these different populations will vary.

There is considerable debate about the appropriate model to use (Hedges & Vevea, 1998; Hunter & Schmidt, 2000). The random effects model generally has less

precision and so the confidence interval around the estimate tends to be wider – which means they are a more conservative test. However its assumptions are more realistic for real-world data. As the random effect model assumes populations will vary there will also be variability between the studies as well as within-study variation. Each study therefore is just a realisation of one sample of the possible distribution of studies. However as it was anticipated that the studies will represent a considerable range of therapies and conditions, it was highly unlikely that a fixed effects model would be appropriate. I think this was a more realistic assumption and do not believe these studies can find one true effect size for these therapies. The random effect model will therefore be used in my analysis.

I am still interested in explaining further some of the amount of variation between studies and will undertake moderator analyses (see 2.6.5). When undertaking a moderator analysis the appropriate model is a mixed effects model. This comprises of a random effects component – which allows for sampling error and random difference between studies – and a fixed effect – which accounts for the variation between groups.

2.6.4 Heterogeneity between studies

The *Q*-Test is a statistical test originally proposed by Cochran (1954) but more commonly associated with DerSimonian and Laird (1986). It is the most commonly used method in meta-analysis for assessing for heterogeneity between included studies. The basic principle is that similar investigations will have drawn subjects from the same population and so will have similar effect sizes. There will be some sampling error however if the variation is greater than that expected by this sampling error then we can assume statistical heterogeneity. This may be for a number of reasons. It is possible that it was a chance finding; it also may be due to the scale used to measure treatment effect or it is a characteristic of the design of the study. The other option is that it was due to a study characteristic *which can be investigated* as a possible moderator variable.

2.6.5 Moderators of effect

A number of theories have been offered as to how MBT work *therapeutically*, such as the work of Brown and Ryan (2003); Shapiro (2006); and Wells (2005). However these theories are not currently empirically supported and it is not clear how MBT is effective (Lazar, 2005).

Detailed sub-group analyses of reported data from experimental research may help inform theory by suggesting possible moderators of effect. For example, certain participants may have a greater response to treatment. Or, participants may have a greater response to certain forms of the therapy which place greater emphasis on certain components. While appropriate analysis of published data would not confirm a mechanism of therapeutic action it may provide evidence of association. This evidence may then inform subsequent research and development of MBT. I detail below some important variables for which it would be useful to consider a moderating effect. The ability to conduct analysis with these variables will depend on available data.

2.6.5.1 *Type of intervention*

There is interest in considering these therapies as having a considerable common ground (Baer, 2003) however it would be interesting to test this hypothesis empirically by contrasting the effectiveness of these therapies. Some of these therapies often have considerable other components and it is possible that these act synergistically with the mindfulness component.

There seems to be considerable variation in the proportion that the mindfulness component makes within these therapies. It has long been suggested that common factors are responsible for the observed effect in psychological therapies (Rosenzweig, 1936) and recent research has supported this view (Wampold, 2001; Wampold, et al., 1997). It would be interesting to determine whether mindfulness was an 'active ingredient'. This could be investigated by determining whether the proportion of the mindfulness component was in any way correlated with outcome. One way of undertaking such an analysis was to consider studies as having mindfulness as either a 'minor' or a 'major' component.

2.6.5.2 Design of study

It was likely that the existing literature consisted of a number of different study designs. These designs are all useful and often emphasise either internal or external validity at the expense of the other. However a comparison of these study designs and the effect sizes they produce would usefully address questions relating to the effectiveness of these therapies in the real world.

2.6.5.3 Gender

Early research has considered the link between gender and frequency of meditation practise (Delmonte, 1984) and found no relationship. However mindfulness has developed considerably and it is unclear whether these approaches are particularly suited to particular demographics within the population. Gender is generally a clearly detailed variable within study reports and so a preliminary investigation could include these details.

2.6.5.4 Therapist training

Baer (Baer, 2006) gives a good overview of the training requirements for the various therapies. In general, while some of the more established therapies have bodies which recommended standards for training, they generally cannot be enforced. This situation may change however as therapies develop and become more widely recognised. Professionally registered therapists however will have general training and practice standards covered by their profession. In the UK for example, the professional standards set out by the British Psychological Society will hopefully ensure an adequate level of expertise before practising.

There are a number of studies which have looked at therapist training and its effect on outcome (Allen, et al., 2006; Ludwig Grepmair, Mitterlehner, Loew, Bachler, et al., 2007; Ludwig Grepmair, Mitterlehner, Loew, & Nickel, 2007; L. Grepmair, Mitterlehner, & Nickel, 2008; Ludwig Grepmair, Mitterlehner, Rother, & Nickel, 2006; Schmidt, 2004; S. L. Shapiro, Brown, & Biegel, 2007; Stanley, et al., 2006). These studies generally find a therapist's personal training and practise as beneficial to clients.

2.6.5.5 Type of psychological condition

Some MBT were designed for specific client groups, for example MBCT was developed for the prevention of relapse in recurrent depression. However more recently there has been interest in a wider application of this therapy to other conditions.

2.6.5.6 Age

There was some evidence from investigations into meditation practice that age may play a factor in maintenance of practice; however it was unclear how. One study found more benefit with older clients (Delmonte, 1980). However these findings were contradicted in another study by the same author (Delmonte, 1986), which found that older subjects showed less improvement and meditated less often. It would be useful to clarify whether there was a link.

2.6.6 Publication bias

Publication bias is a common concern when conducting meta-analysis. This is where it is recognised that the studies available for - and so included within - a meta-analysis are a select sample, chosen on the basis of their positive effect (Dickersin, 1990). Negative (or inconclusive) studies are less likely to be published either because researchers are less likely to write up and submit negative studies, or editors are less likely to accept them for publication. If studies are published on the strength of their findings then it is likely that those not included in a meta-analysis will have different results and this will introduce significant bias to the analysis.

2.6.6.1 Funnel plots

One common method of investigating whether publication bias has been introduced is through the use of a funnel plot (Light & Pilemer, 1984). This plots effect size against a measure of the study sample size. A central line indicates the mean effect size and the pattern the data points make around this line can provide useful information. The assumption is that when the sample size is low there will be less precision and so studies will scatter more widely (and randomly) around the mean. Studies with larger sample sizes will have more precision and so will scatter less

widely around the mean, leading to a distinct funnel-shaped graph (see Figure 5). When the plot is asymmetric (Figure 5) it may indicate the existence of publication bias because insignificant results from small studies were not published.
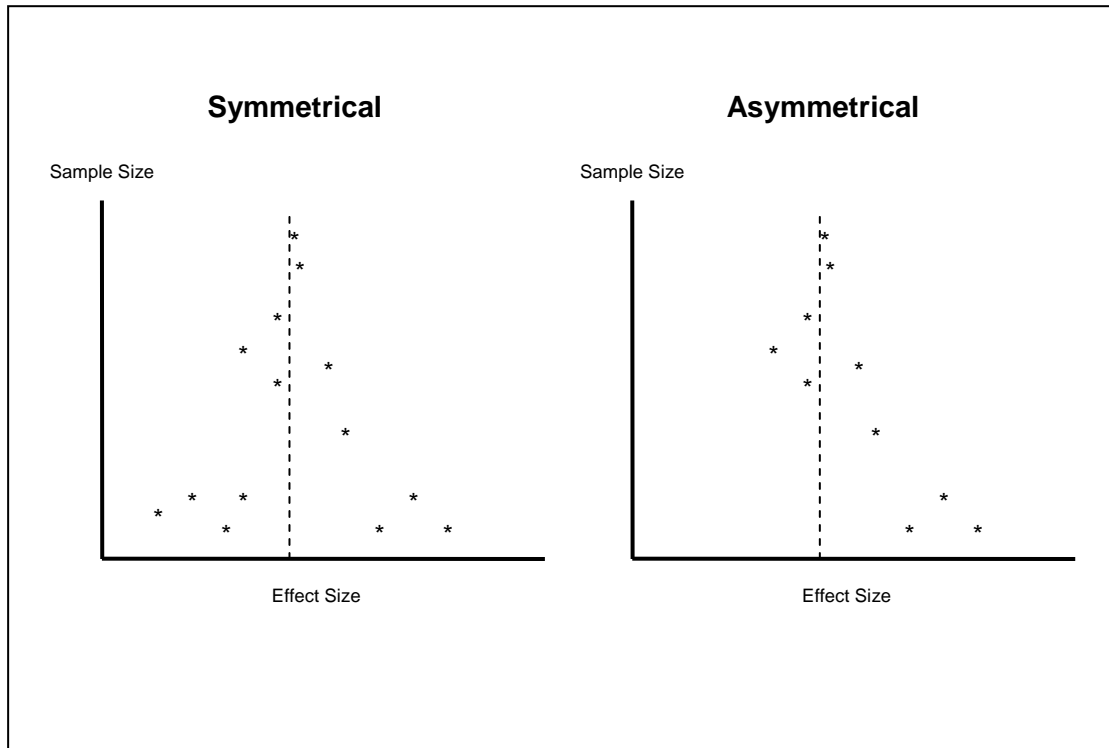
Figure 5.   Funnel plots showing symmetrical and asymmetrical distributions

2.6.6.2   Fail safe N

Unpublished studies may have influenced the effect size and the 'fail safe N' is a calculation to determine how robust the findings are in light of this. Developed by Rosenthal (1979), it provided an estimate of the number of non-significant studies required to render the effect size non-significant (in its original form). One criticism of the original method is that it focuses on *p* values (the number of studies required to render the finding non-significant) rather than the size of the effect. However it is useful if considered carefully and an alternative to Rosenthal's original method was developed by Orwin (1983) and was used in this study, which determines the number of studies required to render the size of the effect negligible rather than zero.

2.7 Data analysis

Where applicable, reporting of the meta-analysis reflected the appropriate guidelines. For example the current QUORUM (Quality of Reporting of Meta-Analyses) guidelines for RCTs (Moher, et al., 1999). Although unpublished at the time of writing, reference was made to the updated PRISMA (Preferred Reporting Items for Systematic reviews and Meta-Analyses) guidelines for all healthcare interventions and study designs. The main data analysis was conducted within SPSS® (SPSS Inc, 15/11/2007). Extensive use was made of SPSS® meta-analysis macros (Lipsey & Wilson, 2000). Some additional analyses were performed in MIX (Bax, Yu, Ikeda, Tsuruta, & Moons, April 2008).

Generally data were presented to two decimal places, however there were some exceptions. Firstly where studies presented data to a lesser degree of precision this was reproduced: for example, to one decimal place. This was to avoid suggesting a higher degree of precision than was reported. Secondly, where a greater degree of precision was a more useful statistic this was reported; for example with probability (p) values. Finally, where the statistical package provided data to a lesser degree of precision this was presented.

The meta-analysis used summary statistics reported in each study. If effect sizes were provided these were not used but recalculated for the analysis. Where possible, data from Intention to Treat (ITT) analysis was used. Pre-treatment was defined as latest available assessment prior to treatment. Post-treatment was defined as those data points closest to the final treatment session.

If means and SD were not presented where possible they were calculated or estimated from available data. For example, estimated from the median and range (Hozo, Djulbegovic, & Hozo, 2005); calculated from the SD and 95% confidence interval; or calculated from Cohen's *d* and the raw difference score.

Effect size and change scores were presented so a positive value indicated an improvement.

2.7.1.1 Forest plots

A forest plot is a distinct form of error-bar chart frequently used in meta-analysis. It is used to group the relative effect sizes of studies as well as the combined effect size into one chart. Typically the effect sizes of the component studies are indicated by a square, the size of which is determined by the relative weight (or contribution the study makes to the overall effect size. The overall effect size is indicated by a diamond. For all of the effect sizes, the confidence interval is represented by an error bar. The line of no effect (zero) is usually added as a reference line. Where applicable, forest plots will be used to illustrate the meta-analyses.

2.8 Chapter summary

There were a number of important stages completed before the meta-analysis was conducted, which together formed a systematic search of the literature. The first stage involved developing and running a comprehensive search strategy to identify possible studies for the analysis. Once identified, citations were screened. This screening stage involved reading the citations and abstracts of these articles. Those reporting studies which were deemed *potentially* suitable for the meta-analysis were retrieved and included in the next stage of the process. This involved reading the full article and applying the eligibility criteria to each of the studies. Eligible articles were included in the pool of studies for the meta-analysis. To enable the analysis, relevant data were extracted from the articles and entered onto the study database. Finally some methodological considerations of the data analysis were presented.

3. Results

3.1 Chapter outline

The results of my analyses are provided in this chapter and are presented in three main sections. These sections in turn consider therapies with mindfulness as a major component; therapies with mindfulness as a minor component; and finally a comparison of these two groups of therapies. The main analyses calculate effect sizes for the combined studies and consider the likelihood of publication bias. Where possible, moderator analyses were conducted to attempt to explain some of the observed heterogeneity between studies.

3.2 The study pool

A total of 40 studies were included within the study pool and once this pool was formed, it was possible to determine the precise analyses conducted. The analyses were therefore guided by the available data and this resulted in three main analyses which are detailed in this chapter and summarised in Figure 6. Until this stage was reached it was not possible to determine whether an analysis of for example follow-up data as well as pre- to post-treatment was possible. Unfortunately very few studies reported follow-up data and for those that did there was considerable variation in the time point. Some studies reported three-month follow-up data whereas others reported one-year. All analyses were based on change from pre- to post-treatment and one study – Miller (1995) - was excluded at this stage as it only reported three-year follow-up data from a previous trial, Kabat-Zinn (1992). It was not possible to combine these data meaningfully with data from other studies.

The other 19 studies which were rendered ineligible for the analyses presented a considerable range of outcome measures. I needed to create reasonably sized groups of studies with a meaningful number to warrant meta-analysis and more detail was provided in each section. It was not possible to include these 19 studies in any of these three analyses. For example condition-specific outcomes only were presented; or objective outcomes across a small number of studies. One notable group of studies excluded at this stage was the three important early studies of Mindfulness-Based Cognitive Therapy. These only presented data on relapse - Teasdale (2000)

and Ma (2004) - or change in a unique outcome related to memory - Williams (2000), which was a further report on data from Teasdale (2000). Unfortunately this meant they could not be combined with other data.

A total of 21 studies were included in the meta-analysis. These were formed into three main groups that looked at studies for which mindfulness was considered a major component of the therapy ($k = 11$); where mindfulness was considered a minor component of the therapy ($k = 8$); and a comparison of these two groups of therapies ($k = 12$). Note that some studies were included in more than one of these three analyses.

3.3   Mindfulness as a major component of therapy

This part of the meta-analysis aimed to answer the question:

> "Are therapies for which mindfulness forms a major component of the intervention effective in treating psychological conditions?"

With reference to the introduction (see 1.5), the distinguishing feature of these mindfulness-based practices was that the mainstay of the therapy was the mindfulness technique and its practise. Interventions within this group included Mindfulness-Based Cognitive Therapy (MBCT); Mindfulness-Based Stress Reduction (MBSR); Mindful Awareness Practices (MAP); and Mindfulness-Based Eating Awareness Training (MB-EAT).

A total of 11 studies were included in this part of the analysis and these studies are detailed in the Appendix (6.4). Of these studies approximately half were single-group pre-post designs ($k = 6$).
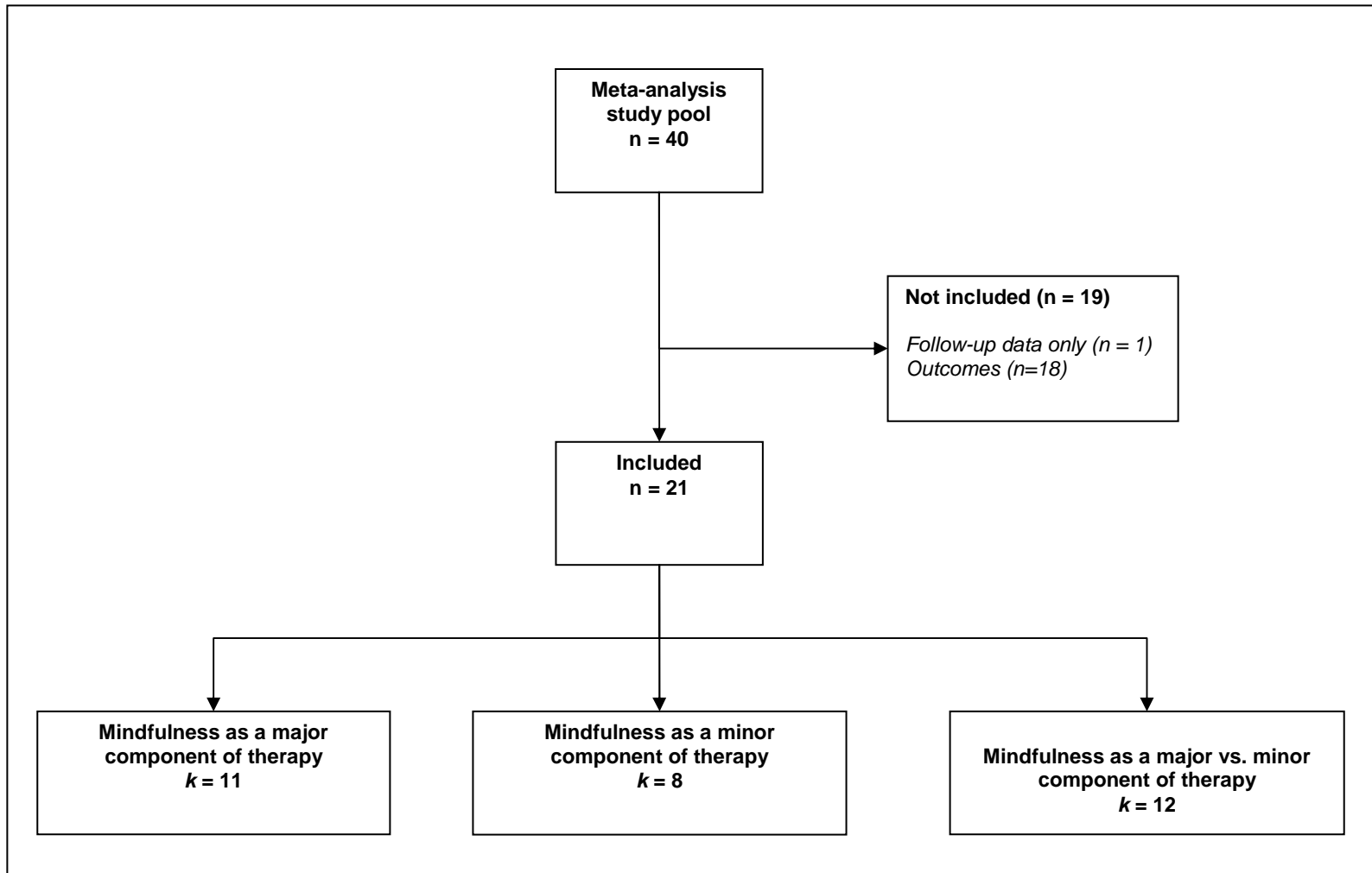
Figure 6.   Flow diagram detailing the number of studies included in the analyses

This analysis had three main parts. The first part included all of these studies and calculated the effect size of a single outcome measure, from pre-treatment to post-treatment. The use of the Beck Depression Inventory (BDI) at these time points determined the studies within this section. This part also investigated possible moderators for the effect size observed and included a number of subgroup analyses. Then the effect size of controlled studies with the same outcome measure only was determined in the third part, and the moderator analysis continued with a comparison of controlled and uncontrolled studies. Finally, the effect size was determined for those studies which provided data on the Beck Anxiety Inventory (BAI) pre-treatment and post-treatment.

3.3.1 Reduction in Beck Depression Inventory (BDI) scores

In this analysis the total number of studies (k) included was 11. The details of these studies relevant to the meta-analysis were summarised in Table 3. The total number of participants (N) was 327 participants were analysed: this figure comprises of the total number of analysed participants across the studies, within the treatment groups. MBCT was the most commonly investigated treatment ($k = 7$); other treatments included MBSR ($k = 2$); MAP ($k = 1$); and MB-EAT ($k = 1$).

Approximately two-thirds of these studies ($k = 7$) treated depression as the index condition; two treated anxiety; and one study treated Attention Deficit and Hyperactivity Disorder (ADHD) and one eating disorder. All of the treatments used in these studies were group interventions. Most of the studies had an initial treatment period of eight sessions over eight weeks ($k = 10$). The exceptions were Kuyken (2008) which had the initial eight-week course followed by four additional sessions over the course of a year; and Kristeller (1999), which was a six-week course. Note that in Kuyken (2008) post-treatment assessment was prior to these additional sessions and so for the purpose of the pre- to post-treatment analysis, the duration of treatment in Kuyken (2008) was equivalent to eight weeks.

Nine of the eleven studies detailed the mean age within their sample. These studies had participants with a mean age that ranged from 38.0 years to 50.2 years. Of the studies that did not report the means Eisendrath (2008) was a brief 'letter to the editor' which reported the age range of the sample only (22-75 years); and similarly

Williams (2008) was a brief report which detailed the inclusion criterion of the range (18-65 years) and that the two groups were comparable with regard to age. Ten of the eleven studies reported the numbers of males and females apart from Williams (2008) which again reported no statistically significant difference in gender numbers between the groups. For the studies reporting gender numbers the percentage of females ranged from approximately half of the sample (54.5%) to the whole sample (100%). Kristeller (1999) purposely recruited females only. Approximately half of the studies ($k = 6$) reported the training undergone by the therapists within the study.

Table 4 summarises the BDI scores within the individual studies. Pre-treatment mean BDI scores ranged from 13.8 with a standard deviation (SD) of 7.9, which was on the borderline for mild depression (range for mild depression was 14-19). This mean was in Evans (2008) – for which anxiety was the index condition for the participants of the study. The maximum pre-treatment BDI score was 35.73 (SD = 8.69) in Finucane (2006). This was in the range for severe depression (for which the range was 29-63).

Since all studies in Table 4 used the BDI, the pre- to post-treatment raw mean difference was used as an effect size representing the average reduction over the course of therapy (that is the mean reduction score = $Mean_{pre} - Mean_{post}$). All of the studies have used the BDI-II apart from Kabat-Zinn (1992), which used the BDI.

Unfortunately only one of these studies - Eisendrath (2008) - reported the value of the pre-post correlation. The pre- post-correlation was however, required for each study in the formula for estimating an overall mean reduction score, where it is used to weight each study (see Lipsey & Wilson, 2002, p. 42). The weights in Table 4 were therefore calculated assuming a modest pre-post correlation of the BDI scores of $r = 0.6$ which allows considerable differences in the individual responses to therapy. All of the studies reported a significant ($p < 0.05$) reduction in the BDI scores.

Table 3. Major component studies included in meta-analysis of the mean reduction in BDI scores, pre- and post-treatment

Lead author (year)	Design	Intervention*	Diagnostic group	Treatment duration, weeks	Treatment group number analysed	Mean age	Percentage female	Therapist training detailed
Eisendrath (2008)	Pre-post single group	MBCT	Depression	8	51	--	74.5	No
Evans (2008)	Pre-post single group	MBCT	Anxiety	8	11	49.0	54.5	Yes
Finucane (2006)	Pre-post single group	MBCT	Depression	8	13	43.0	90.9	Yes
Kabat-Zinn (1992)	Pre-post single group	MBSR	Anxiety	8	22	38.0	72.3	Yes
Kenny (2007)	Audit	MBCT	Depression	8	79	43.3	74.0	No
Kingston (2007)	Pre-post two groups	MBCT	Depression	8	8	41.8	89.5	No
Kristeller (1999)	Pre-post single group	MB-EAT	Eating disorder	6	18	46.5	100.0	No
Kuyken (2008)	RCT	MBCT	Depression	8 (52)	61	49.2	76.4	Yes
Ramel (2004)	Pre-post two groups	MBSR	Depression	8	11	50.2	45.5	Yes
Williams (2008)	RCT	MBCT	Depression	8	28	--	--	Yes
Zylowska (2008)	Pre-post single group	MAP	ADHD	8	25	40.3	62.5	No

* Interventions were: Mindfulness-Based Cognitive Therapy (MBCT); Mindfulness-Based Stress Reduction (MBSR); Mindful Awareness Practices (MAP); and Mindfulness-Based Eating Awareness Training (MB-EAT).

Table 4. Effect sizes for the individual studies, BDI pre- to post-treatment

Lead author (year)	Pre-treatment Mean (SD)	Post- treatment Mean (SD)	Effect size*	SE†	Weight
Eisendrath (2008)	23.95 (10.0)	14.6 (9.28)	9.35	1.21	0.69
Evans (2008)	13.8 (7.9)	8.82 (8.5)	4.98	2.21	0.20
Finucane (2006)	35.73 (8.69)	17.82 (14.59)	17.91	2.98	0.11
Kabat-Zinn (1992)	16.47 (10.97)	10 (9.58)	6.47	1.96	0.26
Kenny (2007)	24.3 (9.8)	13.9 (9.7)	10.40	0.98	1.04
Kingston (2007)	30.33 (7.66)	12.33 (9.72)	18.00	2.77	0.13
Kristeller (1999)	17.8 (12.51)	8 (6.15)	9.80	2.08	0.23
Kuyken (2008)	18.51 (10.91)	13.12 (10.85)	5.39	1.25	0.64
Ramel (2004)	12.54 (10.61)	9.45 (6.73)	3.09	2.40	0.17
Williams (2008)	15.6 (13.84)	8.7 (11.95)	6.90	2.19	0.21
Zylowska (2008)	14.9 (11.1)	7.3 (5.2)	7.60	1.55	0.42

* Mean reduction score
† Standard Error (SE)

The summary statistics are presented within Table 5 and the pre- and post-treatment distributions of the scores are presented graphically in Figure 7. From this the positive-skew to the pre-treatment scores was apparent. However this can still be considered approximately normal as from Table 5 we can determine that the skewness statistic was less than twice the value of the standard error of skewness.

Table 5. Descriptive statistics for the BDI means of the included studies (k = 11, N = 327)

Statistic	Pre-treatment	Post-treatment
Unweighted mean	20.36	11.28
Unweighted SD	7.39	3.31
Median	17.80	10.00
Skewness (SE)	1.10 (0.66)	0.69 (0.66)
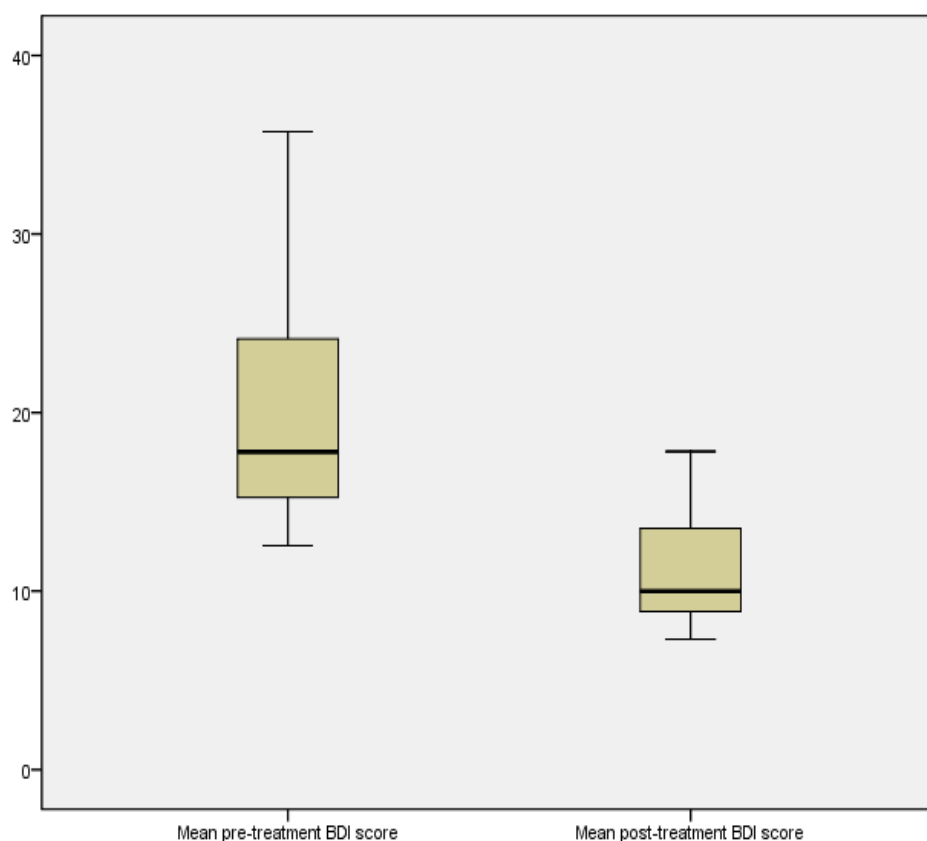
Figure 7.   Box plot, pre- and post-treatment mean BDI scores (k = 11)

Descriptive statistics for the effect sizes are presented in Table 6 and shown graphically in Figure 8.

Table 6. Descriptive statistics for mean reduction score in BDI, pre- and post-treatment

Statistic	Value
Unweighted mean	9.08
Unweighted SD	3.20
Median	7.60
Skewness (SE)	1.08 (0.66)

There are two outliers within this distribution, Finucane (2006) and Kingston (2007). In comparison to the other studies, these two studies treated severely depressed patients with very high pre-treatment depression scores (see Table 3) and thus had potentially the largest scope for a reduction in the depression scores if therapy was effective. MBCT was originally developed as an intervention to prevent relapse in depression however more recently interest has focused on its effectiveness within

actively depressed populations. Table 4 shows the pre-treatment BDI means for these studies to be both over 30 – indicating severe depression – and so a larger change was possible within both of these studies.
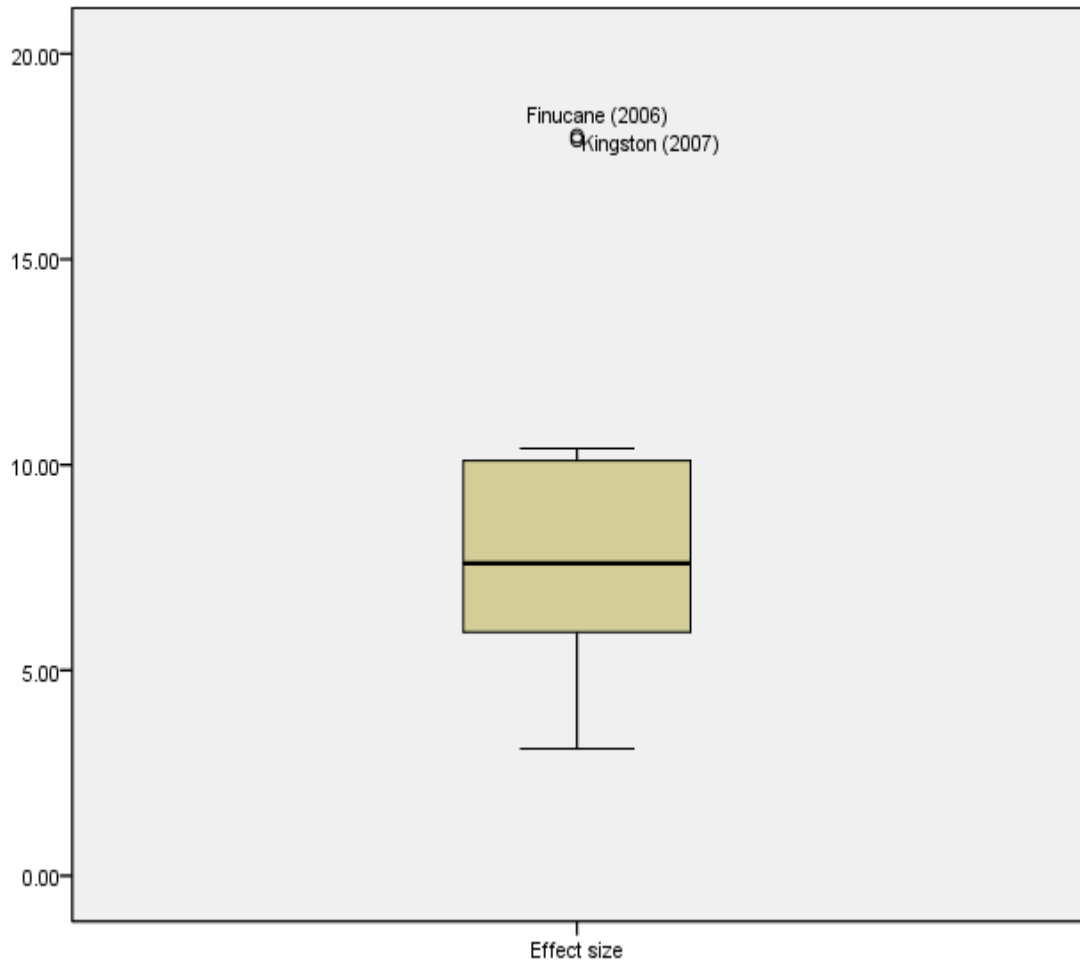
Figure 8.   Box plot of unstandardised reduction score of BDI, pre- to post-treatment

Table 7 provides the mean reduction in score as a percentage of pre-treatment mean score. These reductions are based on the means for all participants within the studies. The reductions range from 25% to 59% of the pre-treatment scores. Spearman's correlation coefficient of the pre-treatment BDI score and the reduction from pre- to post-treatment was highly significant ($\rho = 0.845$; p = 0.001).

Table 7. Percentage reduction pre-treatment to post-treatment in BDI scores, major component therapies

Lead author (year)	Pre-treatment Mean	Percentage reduction
Eisendrath (2008)	23.95	39%
Evans (2008)	13.8	36%
Finucane (2006)	35.73	50%
Kabat-Zinn (1992)	16.47	39%
Kenny (2007)	24.3	43%
Kingston (2007)	30.33	59%
Kristeller (1999)	17.8	55%
Kuyken (2008)	18.51	29%
Ramel (2004)	12.54	25%
Williams (2008)	15.6	44%
Zylowska (2008)	14.9	51%

Figure 9 shows a funnel plot with a symmetrical distribution around the mean effect size. This suggests publication bias was unlikely for this analysis. As a further estimate to determine the effect unpublished studies may have the fail safe N was calculated using Orwin's method (Orwin, 1983). This found that 37 'hidden' studies reporting a zero reduction in the BDI scores were required to bring down the observed effect sizes of an average reduction of 8.73 points to a very small effect size of just 2 points on the BDI. As it was unlikely that such a large number of unpublished studies exist this value would suggest publication bias was unlikely to nullify my result.
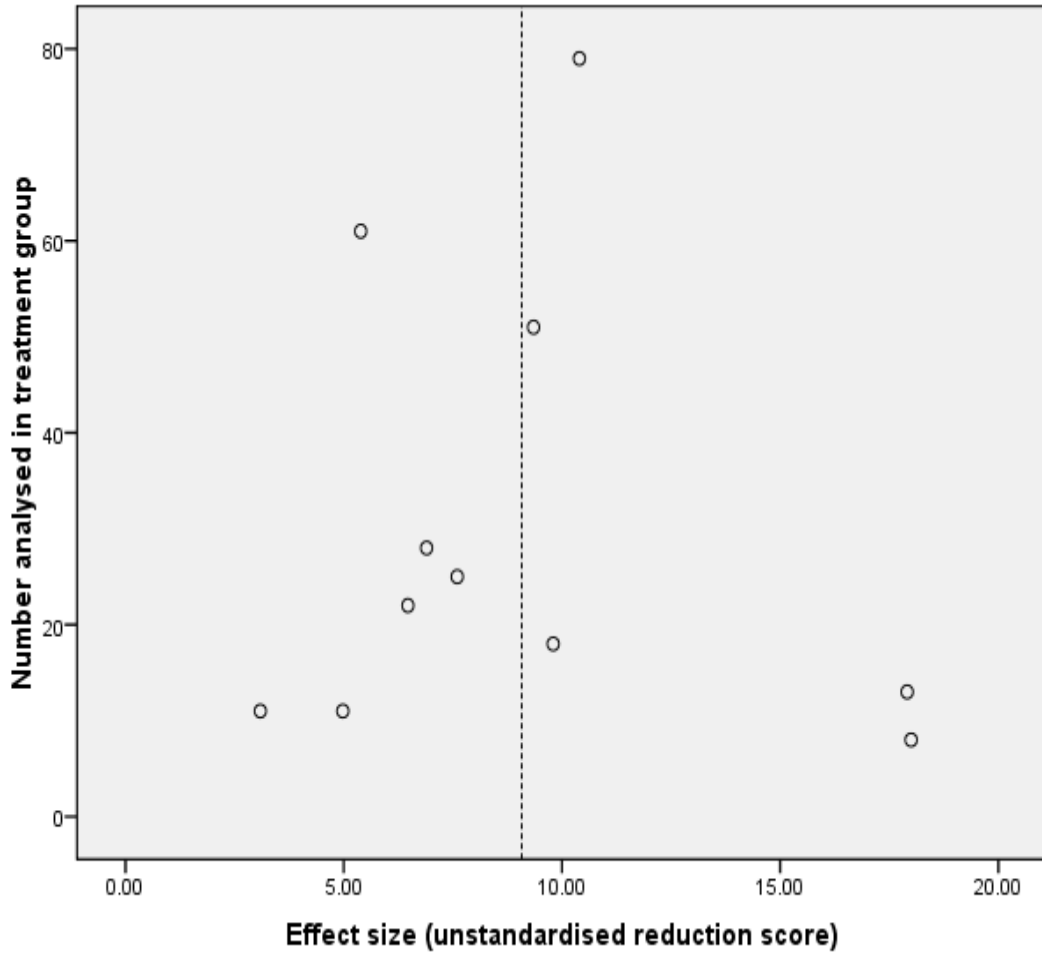
Figure 9.   Funnel plot of unstandardised effect size for BDI, pre- to post-treatment

The meta-analysis for this group of studies was reported in Table 8 and the forest plot presented in Figure 10. The analysis was conducted with a random effects model, which calculated an unstandardised reduction in BDI score of 8.73 (95% CI = LL 6.61, UL 10.86; p < 0.000).

Table 8.   Results of meta-analysis of BDI, pre- and post-treatment

Comparison	k	N	Combined Effect Size	95% CI (LL, UL)*	Combined z score	Combined p value
Major component, depression (BDI)	11	327	8.73	6.61, 10.86	8.05	0.000

* 95% Confidence Interval (Lower Limit, Upper Limit)

Study ID	Year	Mindfulness-based Therapy Mean reduction score / SE		Weight (%)	Histogram of weights	Mean reduction score with 95% CI
Eisendrath (2008)	2008	9.35 / 1.21		11.00%	‖‖	9.35 (6.98 to 11.72)
Evans (2008)	2008	4.98 / 2.21		8.00%	‖	4.98 (0.65 to 9.31)
Finucane (2006)	2006	17.91 / 2.98		7.00%	‖	17.91 (12.07 to 23.75)
Kabat-Zinn (1992)	1992	6.47 / 1.96		9.00%	‖	6.47 (2.63 to 10.31)
Kenny (2007)	2007	10.4 / 0.98		12.00%	‖‖	10.4 (8.48 to 12.32)
Kingston (2007)	2007	18 / 2.77		7.00%	‖	18 (12.57 to 23.43)
Kristeller (1999)	1999	9.8 / 2.08		9.00%	‖	9.8 (5.72 to 13.88)
Kuyken (2008)	2008	5.39 / 1.25		11.00%	‖‖	5.39 (2.94 to 7.84)
Ramel (2004)	2004	3.09 / 2.4		8.00%	‖	3.09 (-1.61 to 7.79)
Williams (2008)	2008	6.9 / 2.19		8.00%	‖	6.9 (2.61 to 11.19)
Zylowska (2008)	2008	7.6 / 1.55		10.00%	‖‖	7.6 (4.56 to 10.64)
META-ANALYSIS:				100%	‖‖‖‖‖‖‖‖‖‖‖‖‖‖‖	8.73 (6.61 to 10.86)

Figure 10. Forest plot of unstandardised mean reduction score in BDI, pre- to post-treatment (k = 11, N = 327)

The test for heterogeneity between the studies (Q-Test) was highly significant ($Q_T = 42.19$, df = 10, p < 0.000), which suggests that the studies were not drawn from the same population. This was suggested by outliers in the distribution of the effect sizes. This finding suggests the combined effect size should be interpreted with caution. The heterogeneity in the effect size distribution between the studies indicated that a moderator analysis was justified to identify factors explaining this variation.

3.3.2 Moderator analyses

A number of moderator analyses were conducted to determine whether they could explain the variation of the effect sizes in the analysis. These analyses were conducted as univariate involving both categorical and continuous moderators (see Table 3). The most promising moderators for which data were available were deemed to be gender; type of therapy; therapist training; and diagnostic group.

The mean age of participants was reported in nine of the studies and was considered as a variable to include in the moderator analysis. However the mean ages covered a very small range (Overall mean = 44.6 years; SD = 4.3; range of means = 38.0-50.2) and this quite limited variation across the studies rendered it unsuitable.

3.3.2.1 Gender

Of the 11 studies, ten reported the numbers of male and female participants (see Table 3). The number of females was converted to a percentage of the overall number of participants within each study. In most studies there was a greater number of female participants (mean proportion = 74.0%; SD = 17.8; range = 45.5-100%). The distribution was shown graphically in Figure 11. The distribution does not have any outliers; all of the data points lie within the inter-quartile range (as represented by the 'whiskers' on the plot).
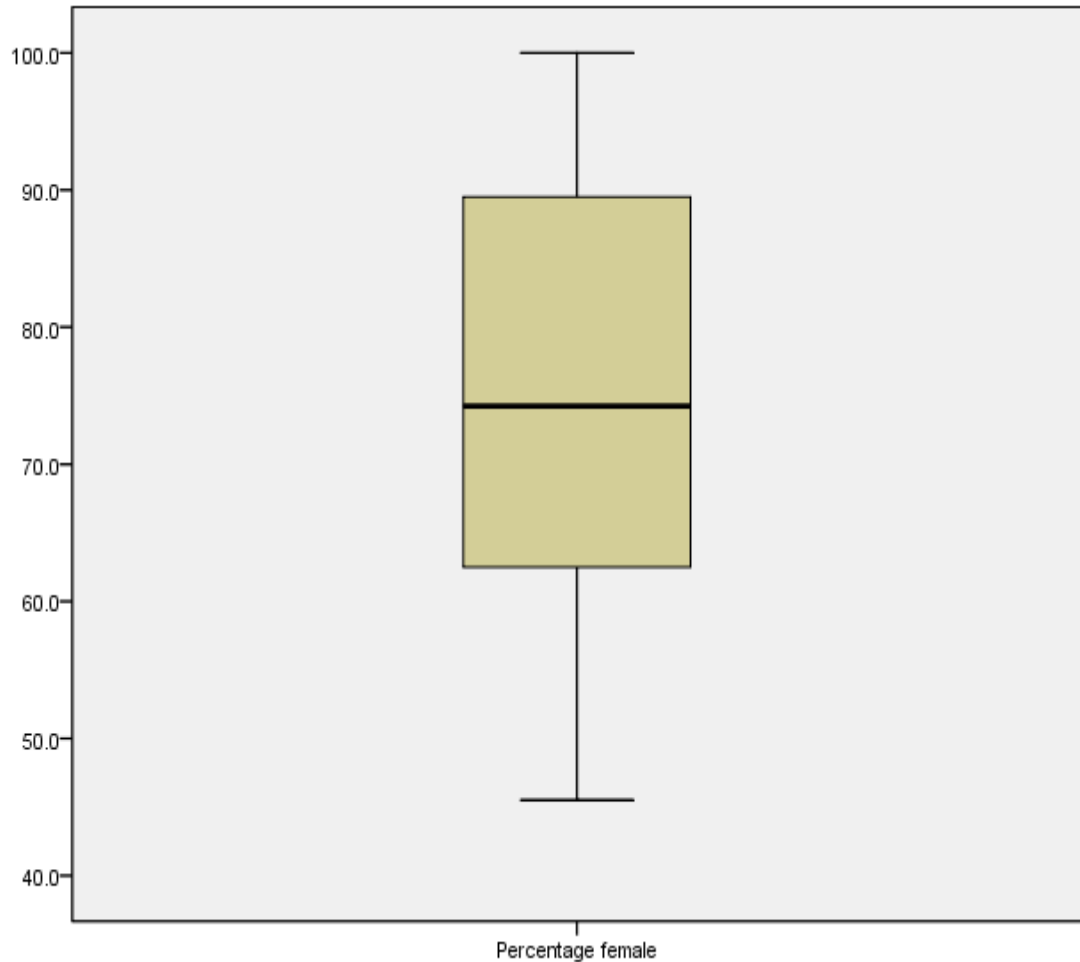
Figure 11. Box plot showing distribution of percentage females within the studies ($k = 9$)

The unstandardised mean change score was plotted against the percentage of females, for each study. The resulting scatterplot shows the linear regression line in Figure 12. The dashed line indicates the line of best fit for the correlation. These data appear to be well distributed along the line of best fit and there do not appear to be any significant outliers in the distribution – this was confirmed with reference to a box plot of the distribution (Figure 11). The scatterplot indicates that there was a positive correlation between reduction in BDI score and proportion of females. The greater the proportion of females taking part in a study, the greater the reported mean reduction in BDI score.
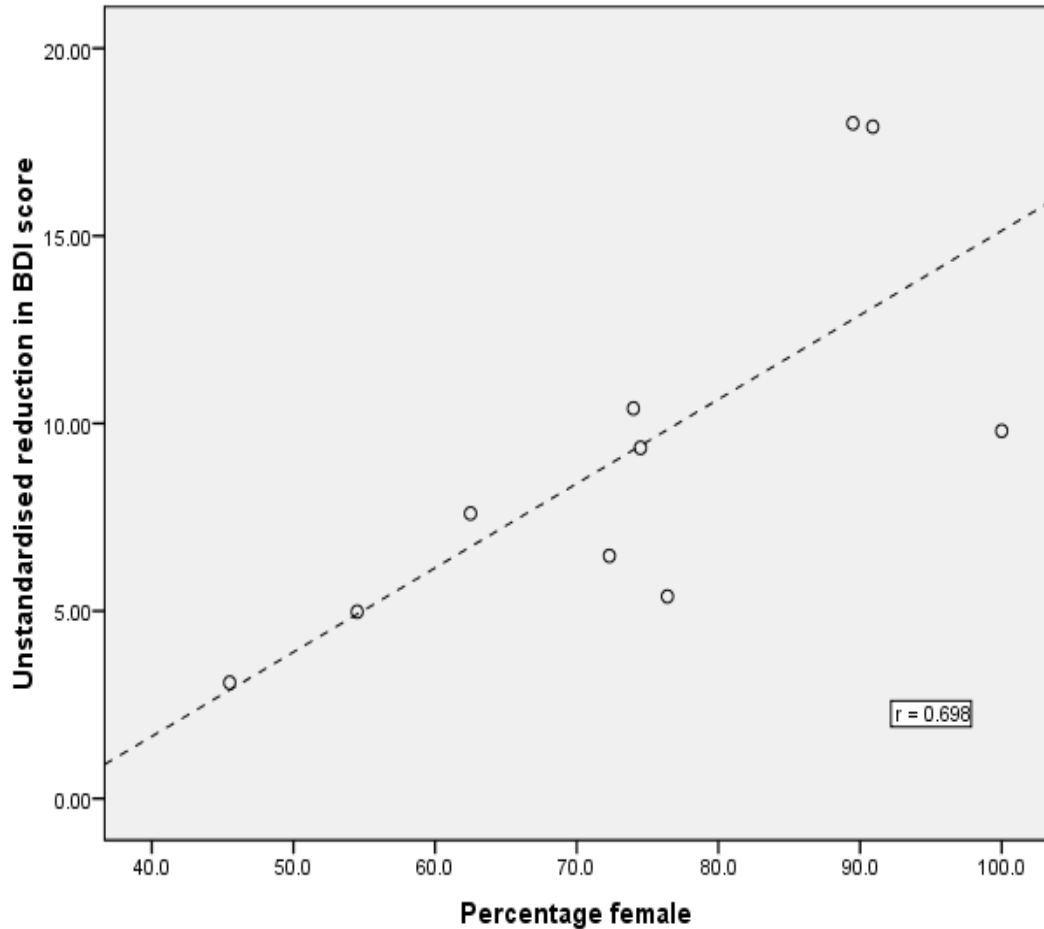
Figure 12. Scatter plot of reduction in BDI scores and female participants (%)

The moderating effect of gender was investigated with a univariate regression which used a mixed effects model. The model was weighted using the inverse of the variance for each study. The effect of gender was statistically significant (B = 0.202; 95% CI LL 0.071, UL 0.333; p = 0.026). In real terms, the unstandardised parameter (B = 0.202) means that for every 5% increase in the proportion of females, there was an one-point reduction in BDI score.

As this was a simple regression the standardised coefficient was equivalent to Pearson's r = 0.698; indicating a marked degree of correlation (Figure 12). More precisely, the regression suggested a considerable amount of the variance was associated with gender. The coefficient of determination was r^2 = 0.487, meaning approximately half (48.7%) of the variation in BDI score was associated with gender.

3.3.2.2 Type of therapy

This analysis determined whether therapy type was an important moderating variable in the observed effect. This analysis comprised two groups, comparing MBCT (n = 7) with other forms of therapy (n = 4; including MBSR, MB-EAT and MAP). The analysis used an inverse variance-weighted mixed effects model.

Table 9 shows the effect sizes for the two groups in the moderator analysis separately. The test for between-group differences was not significant (Q_T = 1.61; df = 1; p = 0.205) and furthermore the 95% CIs overlap suggesting that there was no significant difference between these two therapy groupings and that this grouping of therapies did not explain any of the observed variance in the effect sizes.

Table 9. Effect sizes of therapy groups

Group	k	N	Combined Effect Size	95% CI (LL, UL)	Combined z score	Combined p value
MBCT	7	251	9.82	7.06, 12.57	6.99	0.000
Other	4	76	6.85	3.18, 10.52	3.66	0.000

3.3.2.3 Therapist training

The second analysis determined whether detailing of therapist training (yes/ no) - was an important moderating variable in the observed effect. Reporting on the training undergone for the study was used here as a proxy measure for the quality of training: if studies do not report these details then the assumption was that this was not an important factor when conducting the study. Again, this analysis comprised 11 studies – yes (k = 6) / no (k = 5). The analysis used an inverse variance-weighted one-way random effects ANOVA model.

There was a borderline significant result (Q_T = 3.35; df = 1; p = 0.065) for therapist training. This suggesting that the 'yes' group produced a considerably higher mean reduction (see Table 10).

Table 10. Effect sizes of therapist training

Therapist training detailed?	k	N	Combined Effect Size	95% CI (LL, UL)	Combined z score	Combined p value
Yes	6	135	6.91	4.24, 9.59	5.07	0.000
No	5	192	10.47	7.77, 13.17	7.60	0.000

3.3.2.4 Diagnostic group

This analysis investigated whether diagnostic group (depression vs. other) was an important moderating variable in the observed effect. This analysis comprised 11 studies – depression ($k = 7$) / other ($k = 4$; including anxiety, ADHD and eating disorders). The analysis used an inverse variance-weighted ANOVA model.

Table 11 shows the effect sizes for the two groups in the moderator analysis separately. The test for between-group differences was not significant ($Q_T = 1.01$; df = 1; p = 0.316) which does not suggest diagnostic grouping was an important moderator effect. As the 95% CIs overlap this adds further to there being no significant difference between whether the index diagnosis was depression or not.

Table 11.   Effect sizes of diagnostic groups

Group	k	N	Combined Effect Size	95% CI (LL, UL)	Combined z score	Combined p value
Depression	7	251	9.63	6.81, 12.45	6.69	0.000
Other	4	76	7.24	3.50, 10.97	3.80	0.000

3.3.3 Reduction in BDI scores for controlled studies

Of the 11 studies included in this part of the analysis, a total of four used a control group. A further analysis of these data involved combining these studies to obtain an effect size measure for BDI scores. This was calculated as the unstandardised reduction score and also expressed as Hedges' *g*. The unstandardised reduction score was presented as this gives a sense of the raw change score, as all of the studies used the BDI. Information specific to this analysis are provided in Table 12. The total number of intervention group participants included in the analysis were $N = 108$; and in the control groups $N = 111$. There was considerable variation in the numbers within each study: ranging from Ramel (2004) where the total $N = 22$; up to Kuyken (2008) which had a total $N = 123$. Kuyken (2008) and Williams (2008) were

Randomised Controlled Trials (RCTs); Kingston (2007) used a quasi-randomised design (consecutive referrals were alternately allocated to the two groups) and Ramel (2004) matched the control group on age, gender and baseline BDI score. All studies reported good matching on baseline characteristics; and investigated a form of MBT for depression.

The results of the meta-analysis are shown graphically in Figure 13. The overall effect size was significant. The unstandardised reduction score between intervention and control = 4.86; 95% CI LL 1.18, UL 8.54; $z = 2.59$; $p < 0.01$). Hedges' g was also calculated ($g = 0.4$; 95% CI LL 0.07, UL 0.73; $z = 2.36$; $p = 0.02$). This assumes baseline scores are comparable between intervention and control groups and uses only the post-treatment values between these groups. The analysis was weighted according to derSimonian and Laird's method (DerSimonian & Laird, 1986), which was the default method within the statistical software for random effects models (Bax, et al., April 2008).

The unstandardised reduction score presented for the individual studies in Figure 13 are calculated as the difference in change scores between intervention and control groups. These change scores are presented in Table 12. For example, in Kingston (2007) the change score was 18.00 points on the BDI in the intervention group and 7.09 points within the waiting list control group. The unstandardised reduction score was therefore 10.91 points. There was considerable variation between these unstandardised reduction scores as they ranged from 1.55 to 10.91. The Q-Test for heterogeneity between these studies was not significant ($Q_T = 4.33$; df = 3; $p = 0.231$) and this was anticipated by the amount of variation between the individual effect sizes. This suggested they might not be drawn from different populations, however the power of this test was very low as the number of studies was small ($k = 4$).

Table 12. Summary data for studies included in BDI effect size compared to control ($k = 4$)

Lead author (year)	Design	Intervention					Control				
		Type	N	Pre-treatment BDI score Mean (SD)	Post-treatment BDI score Mean (SD)	Change score	Type*	N	Pre-treatment BDI score Mean (SD)	Post-treatment BDI score Mean (SD)	Change score
Kingston (2007)	Pre-post two groups	MBCT	8	30.33 (7.66)	12.33 (9.72)	18.00	TAU	11	29.18 (9.35)	22.09 (8.99)	7.09
Kuyken (2008)	RCT	MBCT	61	18.51 (10.91)	13.12 (10.85)	5.39	m-ADM	62	20.15 (12.86)	17.47 (12.67)	2.68
Ramel (2004)	Pre-post two groups	MBSR	11	12.54 (10.61)	9.45 (6.73)	3.09	WL	11	13.09 (7.26)	11.55 (7.83)	1.54
Williams (2008)	RCT	MBCT	28	15.6 (13.84)	8.7 (11.95)	6.9	WL	27	12.80 (9.55)	12.26 (11.19)	0.54

* Control group type included: maintenance Anti-Depressant Medication (m-ADM); Waiting List (WL); or Treatment As Usual (TAU)
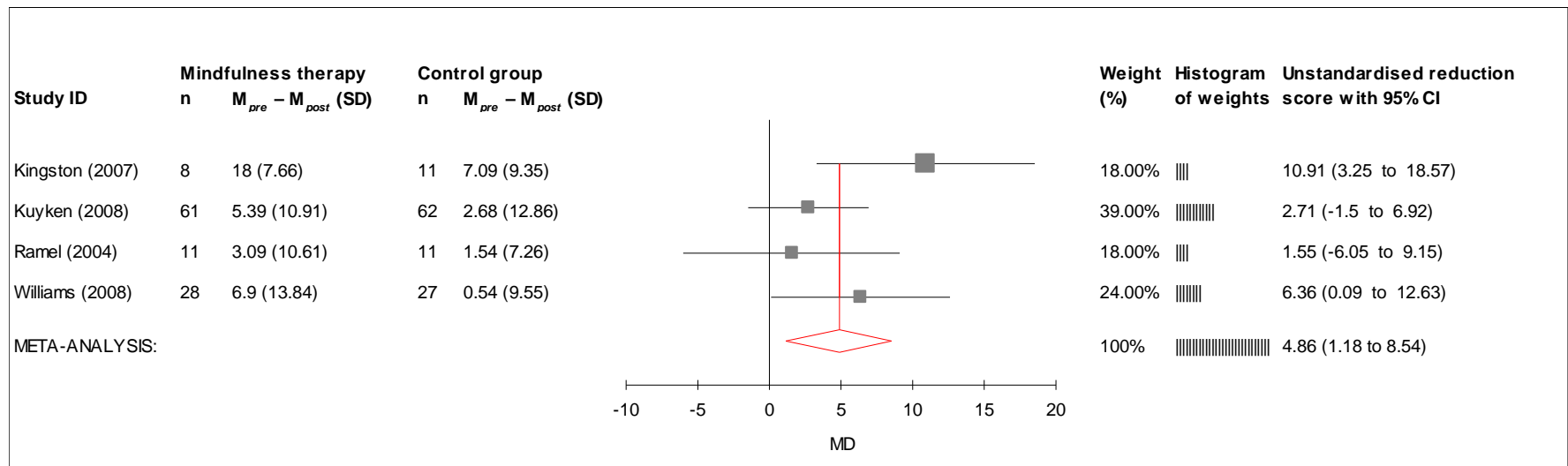
Figure 13. Forest plot of unstandardised effect size of change in BDI score, studies with control groups ($k = 4$)

3.3.3.1 Moderator analysis

This analysis investigated whether study design was a moderating variable in the observed effect. It compared controlled (both randomised and matched; $k = 4$) with uncontrolled studies ($k = 7$). The analysis used an inverse variance-weighted ANOVA model.

Table 13 shows the effect sizes for the two groups in the moderator analysis separately. The effect sizes used were for the intervention group only in the controlled studies to more directly compare with the controlled studies (which accounts for the differences to the effect sizes reported in Section 3.3.3). The test for between-group differences was not significant ($Q_T = 0.32$; df = 1; p = 0.573) which does not suggest controlled designs was an important moderator effect within the intervention groups. As the 95% CIs overlap this adds further to there being no significant difference.

Table 13.   Effect sizes of a control group, major component studies with BDI outcome

Comparison	k	N	Combined Effect Size	95% CI (LL, UL)	Combined z score	Combined p value
Uncontrolled studies	7	219	9.24	6.42, 12.06	6.43	0.000
Controlled studies	4	108	7.86	3.99, 11.74	3.98	0.000

3.3.4   Reduction in Beck Anxiety Inventory (BAI) scores

As well as the effect on depression, I was interested in the effect mindfulness interventions may have on anxiety. The BAI is one of the most commonly used measures of anxiety and a total of $k = 7$ of the studies used it as an outcome; this made it the most appropriate anxiety outcome for meta-analysis. These studies are individually detailed in Table 14 and summarised in Table 15. There was a total of $N = 168$ participants: this figure comprises of the total number of analysed participants across the studies, within the mindfulness treatment groups only (if applicable).

Three of these studies treated depression as the index condition; two treated anxiety; one study treated Attention Deficit and Hyperactivity Disorder (ADHD) and one eating disorder. All of the studies except Kristeller (1999) had an initial treatment period of eight sessions over eight weeks. Kristeller (1999) had a six-week course of treatment.

Table 14.   Studies included in meta-analysis of the average reduction in BAI scores, pre- and post-treatment

Lead author (year)	Design	Intervention	Diagnostic group	Treatment duration, weeks	Treatment group *N* analysed	Pre-treatment Mean (SD)	Post-treatment Mean (SD)
Eisendrath (2008)	Pre-post single group	MBCT	Depression	8	51	13.38 (8.76)	9.78 (6.38)
Evans (2008)	Pre-post single group	MBCT	Anxiety	8	11	19.00(13.70)	8.91 (7.80)
Finucane (2006)	Pre-post single group	MBCT	Depression	8	13	32.00(12.50)	20.54 (17.08)
Kabat-Zinn (1992)	RCT	MBSR	Anxiety	8	22	20.53 (13.24)	9.0 (9.14)
Kristeller (1999)	Pre-post single group	MB-EAT	Eating disorder	6	18	16.3 (13.45)	10.9 (13.02)
Williams (2008)	Pre-post single group	MBCT	Depression	8	28	11.37 (9.64)	9.40 (9.22)
Zylowska (2008)	Pre-post single group	MAP	ADHD	8	25	7.2 (3.1)	4.1 (2.6)

Table 15. Descriptive statistics for the BAI means of the included studies ($k = 7$, $N = 168$)

Statistic	Pre-treatment	Post-treatment
Unweighted mean	17.11	10.38
Unweighted SD	7.99	4.97
Median	16.30	9.40
Skewness (SE)	0.98 (0.79)	1.51 (0.79)

Pre-treatment mean BAI scores ranged from 7.2 (SD = 3.1) in Zylowska (2008)'s study of ADHD, which was on the borderline with the range for mild anxiety (range for mild anxiety 8-15). The maximum pre-treatment BAI score was 32.0 (SD = 12.5) in Finucane (2006). This was in the range for severe anxiety (for which the range was 26-63). Similarly, the lowest post-treatment score (4.1; SD = 2.6) – was within the 'minimal' range for anxiety - was reported in Zylowska (2008); a study which investigated ADHD. The highest post-treatment score was similarly reported in Finucane (2006) and was 20.54 (SD = 17.08); which was within the 'moderate' severity range for anxiety. These are shown graphically in Figure 14.

Treatment duration was comparable across studies: seven of the studies were of eight sessions over eight weeks. The exception was Kristeller (1999) which was six sessions over six weeks. All of the studies were of group interventions.

Since all studies in Table 14 used the BAI, the pre- to post-treatment raw mean difference was used as an effect size representing the average reduction over the course of therapy (that is the mean reduction score = $\text{Mean}_{pre} - \text{Mean}_{post}$). These are presented for the individual studies in Table 16. Again, only one of these studies - Eisendrath (2008) - reported the value of the pre-post correlation. The weights in Table 16 were therefore calculated assuming a modest pre-post correlation of the BAI scores of $r = 0.6$ which allows considerable differences in the individual responses to therapy. All studies reported a significant ($p < 0.05$) reduction in the BAI scores.
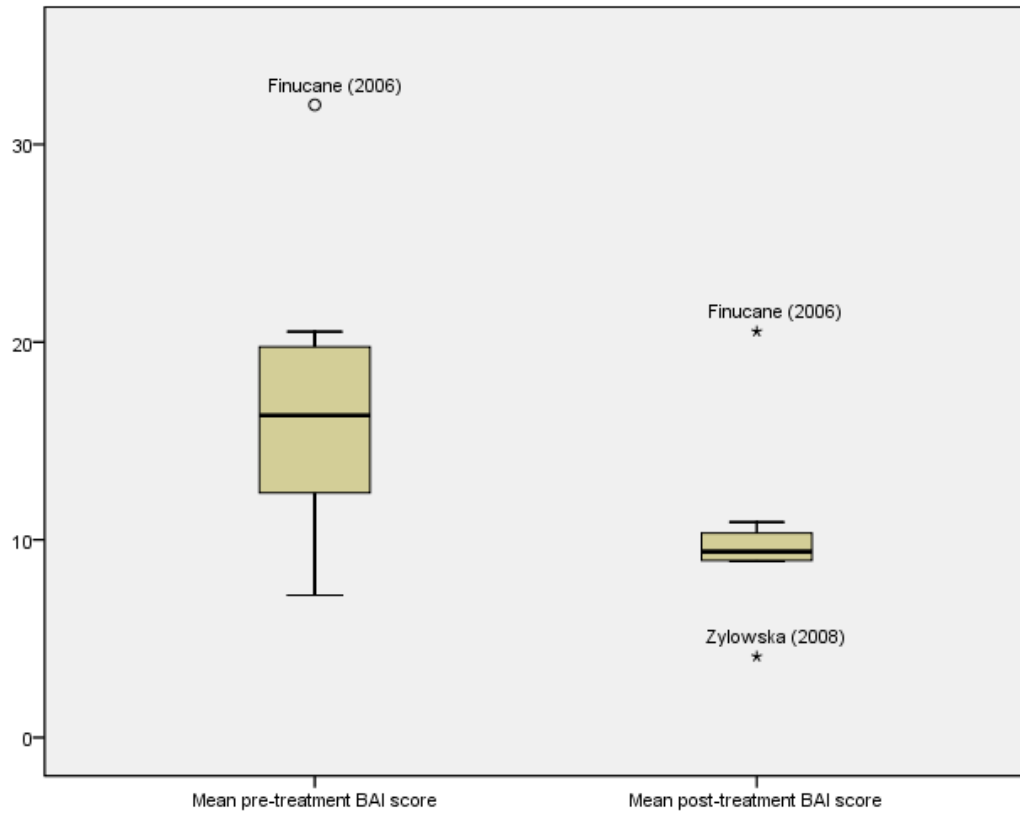
Figure 14. Box plot, pre- and post-treatment mean BAI scores (*k* = 8, *N* = 139)

Table 16. Effect sizes for the individual studies, BAI pre- to post-treatment

Lead author (year)	Mean Reduction Score	SE	Weight
Eisendrath (2008)	3.60	0.96	1.09
Evans (2008)	10.09	3.01	0.11
Finucane (2006)	11.46	3.71	0.07
Kabat-Zinn (1992)	11.53	2.17	0.21
Kristeller (1999)	5.40	2.79	0.13
Williams (2008)	1.97	1.59	0.39
Zylowska (2008)	3.10	0.51	3.82

Descriptive statistics for the overall effect sizes are presented in Table 17 and shown graphically in the form of a box plot in Figure 15.

Table 17.   Descriptive statistics for mean reduction score of BAI, pre-post treatment

Statistic	Value
Unweighted mean	6.74
Unweighted SD	4.17
Median	5.40
Skewness (SE)	0.21 (0.79)
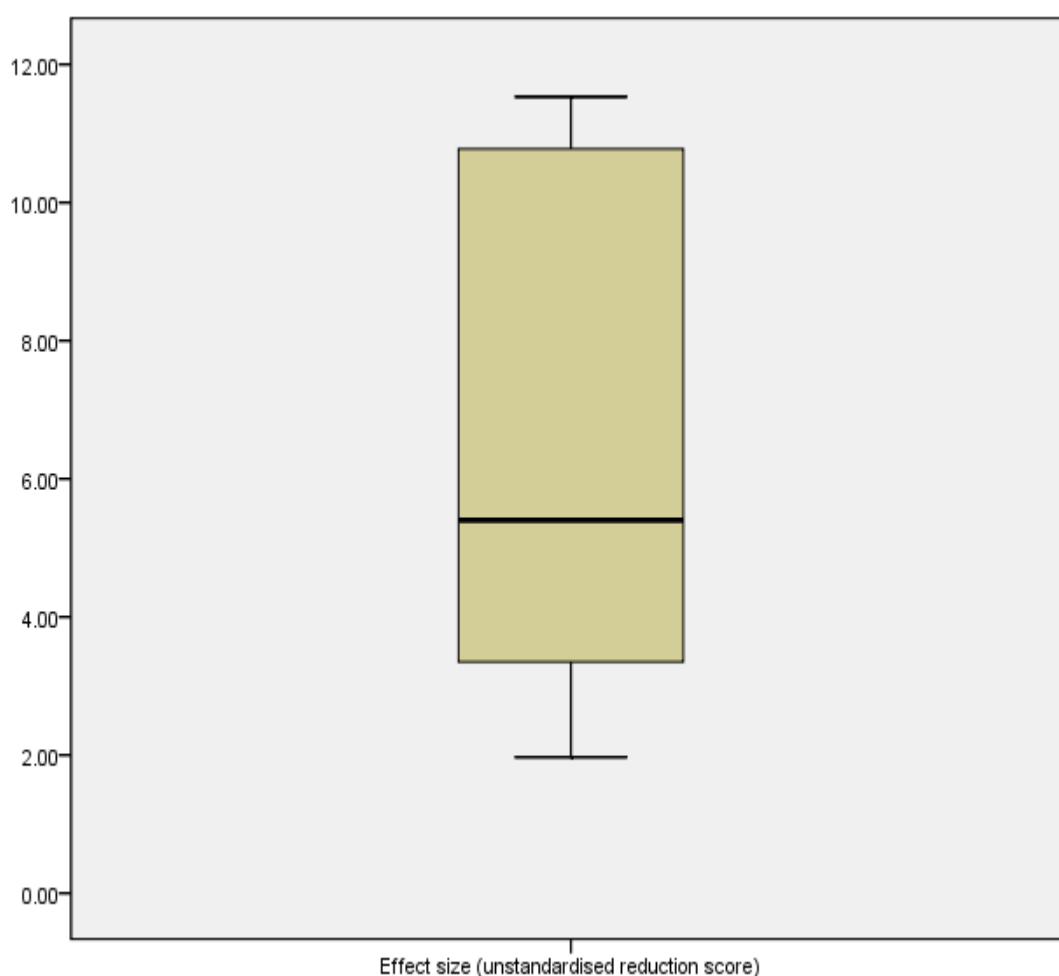
Figure 15. Box plot of unstandardised reduction score of BAI, pre- to post-treatment

Table 18 details the mean reduction in score as a percentage of pre-treatment score for each of the studies included in this analysis. These reductions are based on the

means for all participants within the studies on the BAI. The reductions range from 17% to 56% of the pre-treatment scores. Spearman's correlation coefficient of the pre-treatment BDI score and the reduction from pre- to post-treatment was highly significant ($\rho = 0.929$; p = 0.003).

Table 18. Percentage reduction pre-post treatment in BAI scores, major component therapies

Lead author (year)	Pre-treatment Mean	Percentage reduction
Eisendrath (2008)	13.38	27%
Evans (2008)	19.00	53%
Finucane (2006)	32.00	36%
Kabat-Zinn (1992)	20.53	56%
Kristeller (1999)	16.3	33%
Williams (2008)	11.37	17%
Zylowska (2008)	7.2	43%

As these studies provided data for BDI and BAI outcomes, it was interesting to consider the relationship between the improvements on these two scales. Using Spearman's Rank correlation coefficient the relationship for all of the studies together was not significant ($k = 7$; $\rho = -0.036$; p = 0.939). Interestingly however if the two studies that investigated anxiety conditions are taken out, Evans (2008) and Kabat-Zinn (1992), the correlation was perfect ($k = 5$; $\rho = 1$; p = 0.000).

Figure 16 shows a funnel plot with a symmetrical distribution around the mean effect size. This suggests publication bias was unlikely in these studies. Furthermore the fail safe N calculation found that 13 'hidden' studies reporting a zero reduction in the BAI scores were required to bring down the observed effect sizes of an average reduction of 5.64 points to a very small effect size of just 2 points on the BAI. It was possible such a number of unpublished studies exist and so publication bias may nullify my result however I think this was unlikely as this number represents more studies than those entered into the analysis.
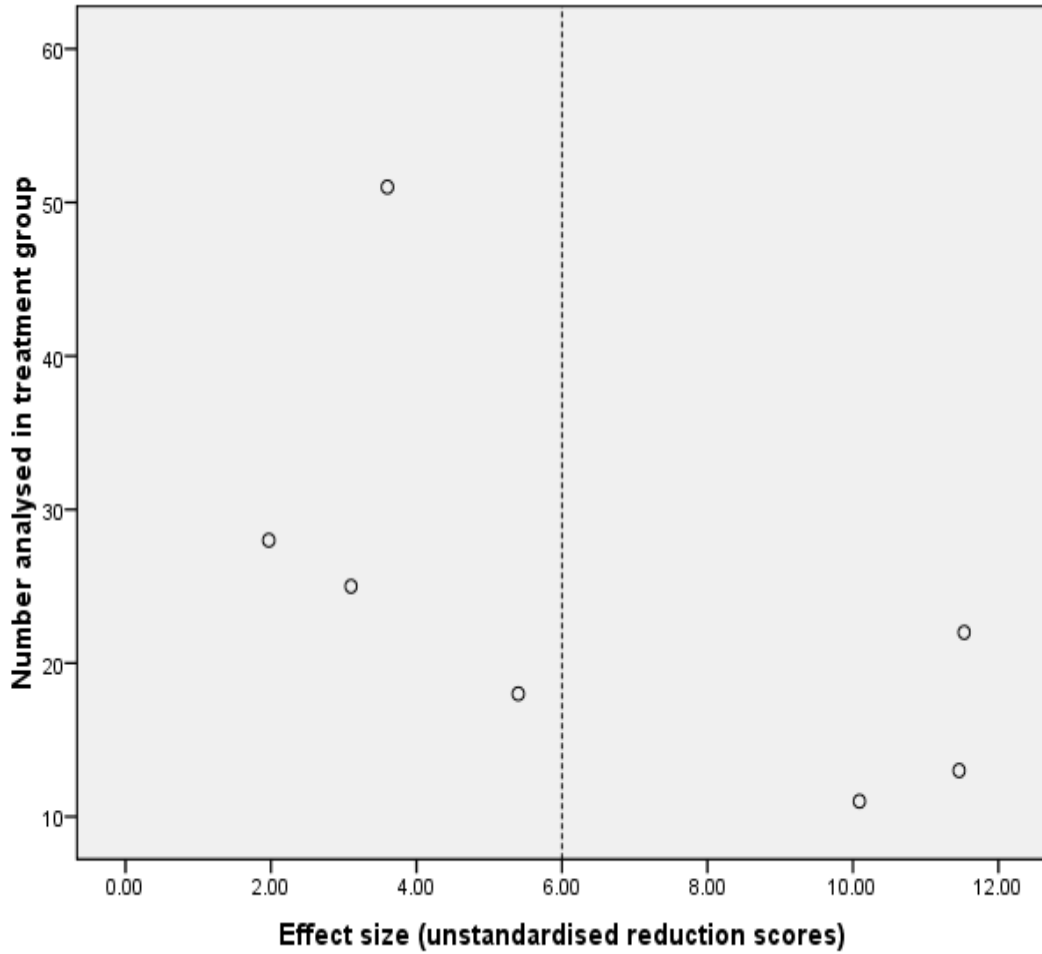
Figure 16. Funnel plot of unstandardised effect size for BAI, pre- to post-treatment

The meta-analysis for this group of studies was reported in Table 19. Using the random effects model, it calculated an unstandardised reduction in BAI score of 5.64 (95% CI = LL 3.29, UL 8.00; p < 0.00).

Table 19.   Results of meta-analysis of BAI, pre- and post-treatment

Comparison	k	N	Combined Effect Size	95% CI (LL, UL)	Combined z score	Combined p value
Major component, BAI reduction	7	168	5.64	3.29, 8.00	4.70	0.000

The test for heterogeneity between the studies (Q-Test) was significant ($Q_T = 24.85$, df = 6, p = 0.000), which suggests that the studies were not drawn from the same population and so the combined effect size should be interpreted with caution. The heterogeneity in the effect size distribution between the studies indicated that a moderator analysis would be justified to identify factors explaining this variation.

However due to the total number of studies being small ($k = 8$) it was considered inappropriate to conduct moderator analyses.

3.4 Mindfulness as a minor component of therapy

This part of the meta-analysis aimed to answer the following question:

> *"Are therapies which include mindfulness as a minor component of the therapy effective in treating anxiety?"*

A total of eight studies were eligible for this part of the analysis and these studies are summarised in Table 20. More detail on the studies was provided in the Appendix (6.4). Most of these studies did not have a control group design ($k = 5$); the others were RCTs ($k = 3$). The analysis included only the mindfulness treatment group arm, where applicable, because of the small number of controlled studies. The eight studies reported on a total of $N = 139$ analysed participants from the intervention groups. All of these studies investigated anxiety conditions and the specific diagnoses included Generalised Anxiety Disorder (GAD; $k = 3$); Social Anxiety Disorder (SAD; $k = 2$); mixed anxiety and depression ($k = 1$); 'anxiety disorders' (not further specified) ($k = 1$); and trichotillomania ($k = 1$).

Across the studies, the total number of hours of treatment ranged from six hours to 20 hours; occurring over between four and sixteen weeks. A number of different treatments were investigated within the studies. Therapies included in this analysis were Acceptance and Commitment Therapy (ACT; $k = 4$); Acceptance-Based Behaviour Therapy (ABBT; $k = 2$); a Buddhist Counselling Programme (BCP; $k = 1$); and Metacognitive Therapy (MCT; $k = 1$).

The mean age of participants was provided in seven of the studies and these ranged from 31.0 years to 42.4 years. All of the studies provided the gender of the participants and the proportion of females ranged from 50% to 89.3%. Seven of the studies detailed the training of the therapist(s) providing the treatment in the study.

Table 20. Studies included in meta-analysis of therapies with mindfulness as a minor component, pre- and post-treatment (k = 8, N = 139)

Lead author (year)	Design	Intervention*	Diagnosis	N	Treatment duration, weeks	Treatment duration, hours	Mean age	Percentage female	Therapist training detailed
Dalrymple (2007)	Pre-post single group	ACT	SAD	16	12	12	31.0	52.8	Yes
Forman (2007)	RCT	ACT	Anxiety and depression	37	15	15	27.8	80.2	Yes
Ossman (2006)	Pre-post single group	ACT	SAD	12	10	20	42.4	50.0	Yes
Roemer (2007)	Pre-post single group	ABBT	GAD	16	16	18	36.4	56.3	Yes
Roemer (2008)	RCT	ABBT	GAD	15	16	18	33.6	71.0	Yes
Rungreangkulkij (2008)	Pre-post single group	BCP	Anxiety disorder	21	4	6	42.1	81.0	Yes
Wells (2006)	Audit	MCT	GAD	10	3-12†	--		60.0	
Woods (2006)	RCT	ACT‡	Trichotillomania	12	12	--	35.0	89.3	Yes

* Interventions were: Acceptance and Commitment Therapy (ACT); Acceptance-Based Behaviour Therapy (ABBT); Buddhist Counselling Programme (BCP); and Metacognitive Therapy (MCT).
† Wells (2006) was an audit of treatment: as such there was no pre-specified treatment duration.
‡ The treatment also included habit reversal training (for trichotillomania).

The outcomes used in this analysis are detailed in Table 21, along with pre- and post-treatment means and weighted standardised reduction score. The weighting used for this analysis was the inverse of the variance. As a number of different scales are used in this analysis it was appropriate to use a standardised reduction score for the effect size. All of these outcomes were the self-rated current anxiety state (subjective measures).

Table 21.   Outcome measures used in the analysis of mindfulness-based therapies for treating anxiety (*k* = 8, *N* = 139)

Lead author (year)	Anxiety outcome measure*	Pre-treatment Mean (SD)	Post-treatment Mean (SD)	Weighted standardised reduction score	Weight
Dalrymple (2007)	SPAI-sp	130.81 (31.26)	97.48 (32.05)	1.05	11.81
Forman (2007)	BAI	13.42 (10.2)	10.32 (9.57)	0.31	43.57
Ossman (2006)	SPAI-sp	138.25 (24.4)	118 (24.4)	0.83	10.49
Roemer (2007)	DASS-Anxiety	16 (9.44)	4.88 (6.06)	1.40	8.98
Roemer (2008)	DASS-Anxiety	12.53 (1.95)	5.52 (1.35)	4.18	1.57
Rungreangkulkij (2008)	STAI-St	61.29 (8.56)	48.57 (11.2)	1.28	13.01
Wells (2006)	BAI	21 (9.67)	3.4 (4.12)	2.37	2.77
Woods (2006)	PAI-Anxiety	63.8 (12.4)	58.3 (9.7)	0.49	13.01

* Outcomes included: Social Phobia and Anxiety Inventory – social phobia subscale (SPAI-sp); Beck Anxiety Inventory (BAI); Depression and Anxiety Stress Scales—Anxiety subscale (DASS-Anxiety); State–Trait Anxiety Inventory – State measure (STAI-St); and the Personality Assessment Inventory-Anxiety subscale (PAI-Anxiety).

The summary statistics for the standardised reduction scores for the studies used in this analysis are presented in Table 22. The studies covered a wide range of effect sizes (0.31 – 4.18) and their distribution was shown graphically in Figure 17. There was one outlier in the distribution, Roemer (2008). As well as reporting a considerable reduction in raw score, the study also reported very small SDs around these values, which also contributed to the very large effect size. This outlier significantly skews the distribution, as the skewness statistic was over twice its standard error.

Table 22. Descriptive statistics for the standardised mean differences in anxiety scores, pre-post treatment (*k* = 8)

Statistic	Value
Unweighted mean	1.49
Unweighted SD	1.26
Median	1.16
Skewness (SE)	1.65 (0.75)
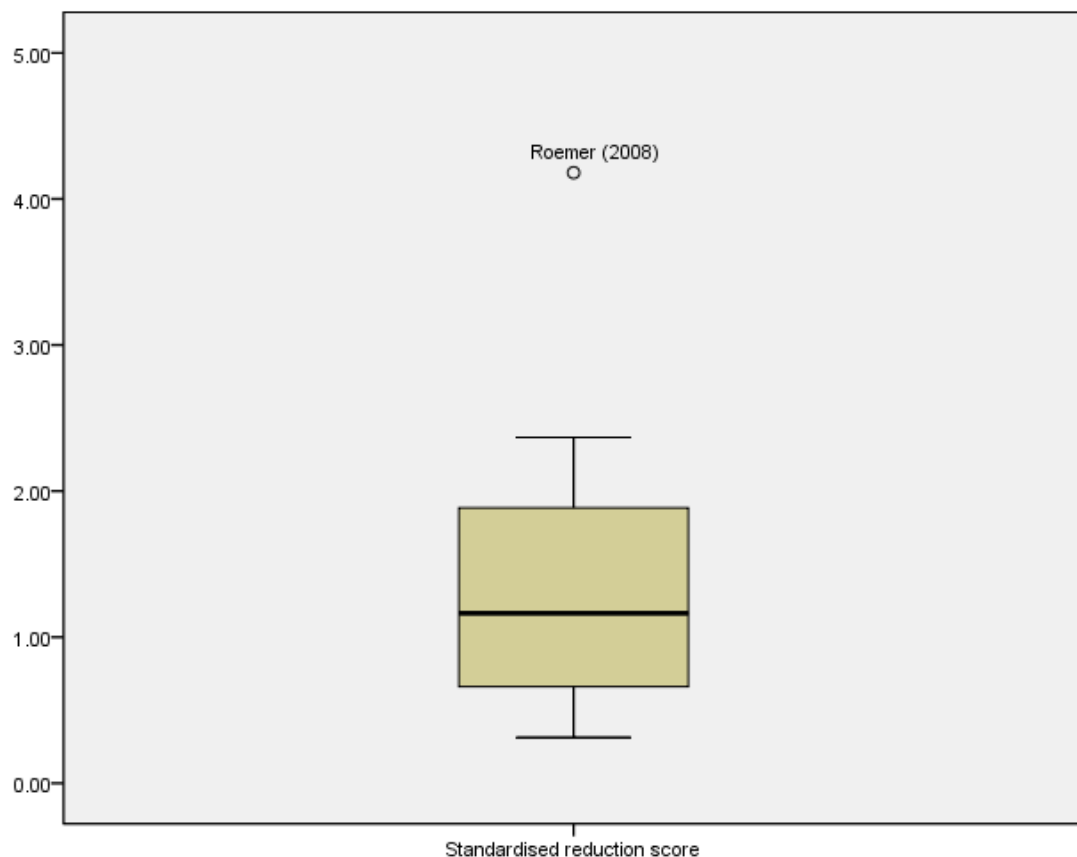
Figure 17. Box plot of standardised mean differences across studies in analysis of mindfulness-based therapies for treating anxiety

The meta-analysis for this group of studies was reported in Table 23. Using a random effects model, it calculated a statistically significant standardised reduction score of 1.24 (95% CI = LL 0.70, UL 1.77; p < 0.000).

Table 23. Results of meta-analysis of standardised mean differences of anxiety scores, pre- and post-treatment

Comparison	k	N	Effect size	95% CI (LL, UL)	Combined z score	Combined p value
Minor component studies, anxiety outcomes	8	139	1.24	0.81, 2.10	4.56	0.000

The test for heterogeneity between the studies (Q-Test) was highly significant ($Q_T = 43.29$, df = 7, p < 0.000), which suggests that the studies were not drawn from the same population and so the combined effect size should be interpreted with caution.

Table 24 provides the mean reduction in score as a percentage of pre-treatment mean score. These reductions are based on the means for all participants within the studies. The reductions cover a very wide range: from 9% to 84% of the pre-treatment scores. Spearman's correlation coefficient of the pre-treatment score and the standardised effect size was not significant ($\rho = -0.464$; p = 0.294).

Table 24. Percentage reduction pre-post treatment in BAI scores, minor component therapies

Lead author (year)	Anxiety outcome measure	Pre-treatment Mean	Percentage reduction
Dalrymple (2007)	SPAI-sp	130.81	25%
Forman (2007)	BAI	13.42	23%
Ossman (2006)	SPAI-sp	138.25	15%
Roemer (2007)	DASS-Anxiety	16.0	70%
Roemer (2008)	DASS-Anxiety	12.53	56%
Rungreangkulkij (2008)	STAI-St	61.29	21%
Wells (2006)	BAI	21.0	84%
Woods (2006)	PAI-Anxiety	63.8	9%
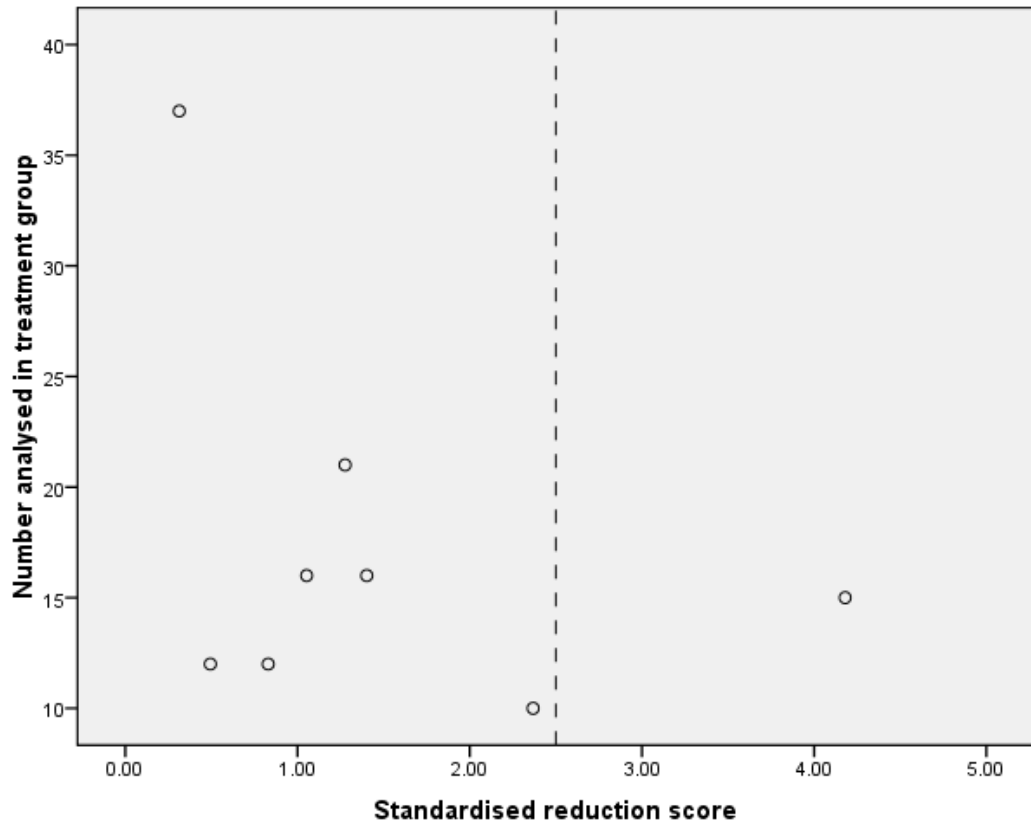
Figure 18. Funnel plot of standardised mean difference for anxiety scores, pre- to post-treatment

The funnel plot (Figure 18) of the distribution of the standardised effect sizes has an outlier to the top-left of the graph; other than this the distribution around the mean effect size appears symmetrical. As the outlier was to the left rather than the right of the graph (reporting a lower than expected effect size) it suggests publication bias was unlikely for this analysis. However the number of studies in the plot was small ($k = 8$). The fail safe N calculation found that 25 'hidden' studies reporting a zero reduction in the BAI scores were required to bring down the observed effect size of 1.24 to a very small effect size of 0.2. As it was unlikely that such a large number of unpublished studies exist this value would suggest publication bias was unlikely to nullify my result.

3.5 *Mindfulness as a major vs. minor component of therapy*

This part of the meta-analysis aimed to answer the following question:

> *"In the treatment of anxiety, are there any differences in the effectiveness of studies for which mindfulness comprises a major component compared with those for which mindfulness comprises a minor component?"*

Anxiety was chosen as the condition under investigation because it provided the largest number of therapies within the two groups. This analysis was conducted to determine whether the 'amount' of mindfulness was associated with therapeutic gain in anxiety scores. This was conceived as essentially a dose-response analysis; studies were categorised as having mindfulness as a minor component of the therapy or as a major component (see Table 25). To minimise the amount of data previously presented, only variables pertinent to these analyses are detailed. Yook (2008) and Koszycki (2007) were not included in previous analyses however more detail on these two studies can be found in the Appendix (6.4).

Table 25. **Major and minor component therapies included in meta-analysis of the mean reduction in anxiety scores, pre- and post-treatment**

Lead author (year)	Design	Diagnosis	N	Intervention	Mindfulness component	Anxiety outcome measure*
Dalrymple (2007)	Pre-post single group	SAD	16	ACT	Minor	SPAI-sp
Forman (2007)	RCT	Anxiety and depression	37	ACT	Minor	BAI
Ossman (2006)	Pre-post single group	SAD	12	ACT	Minor	SPAI-sp
Roemer (2007)	Pre-post single group	GAD	16	ABBT	Minor	DASS-Anxiety
Roemer (2008)	RCT	GAD	15	ABBT	Minor	DASS-Anxiety
Rungreangkulkij (2008)	Pre-post single group	Anxiety disorder	21	BCP	Minor	STAI-St
Wells (2006)	Audit	GAD	10	MCT	Minor	BAI
Woods (2006)	RCT	Trichotillomania	12	ACT	Minor	PAI-Anxiety
Yook (2008)	Pre-post single group	GAD	19	MBCT	Major	HRS-A
Kabat-Zinn (1992)	Pre-post single group	GAD	22	MBSR	Major	BAI
Koszycki (2007)	RCT	SAD	26	MBSR	Major	LSAS-fear
Evans (2008)	Pre-post single group	GAD	11	MBCT	Major	BAI

* Anxiety outcomes not previously mentioned include: Hamilton Rating Scale for Anxiety (HRS-A); and the Liebowitz Social Anxiety Scale-Fear (LSAS-fear).

In this analysis there were a total of 12 studies. Eight of these had mindfulness as a minor component and four had mindfulness as a major component. For those with mindfulness as a minor component, the total $N = 139$; for mindfulness as a major component, $N = 78$.

Table 26 shows the descriptive statistics for these two groups of therapies. The distributions are shown graphically in Figure 19.

Table 26. Descriptive statistics for standardised mean differences in anxiety scores, overall for major and minor component mindfulness therapies

Statistic	Minor component ($k = 8$)	Major component ($k = 4$)
Unweighted mean	1.49	1.62
Unweighted SD	1.26	1.14
Median	1.16	1.17
Skewness (SE)	1.65 (0.75)	1.72 (1.01)

For studies with mindfulness as a major component, from Figure 19 we can determine there was a positive skew to the distribution. Although the skew was quite large, it would still be considered approximately normal.
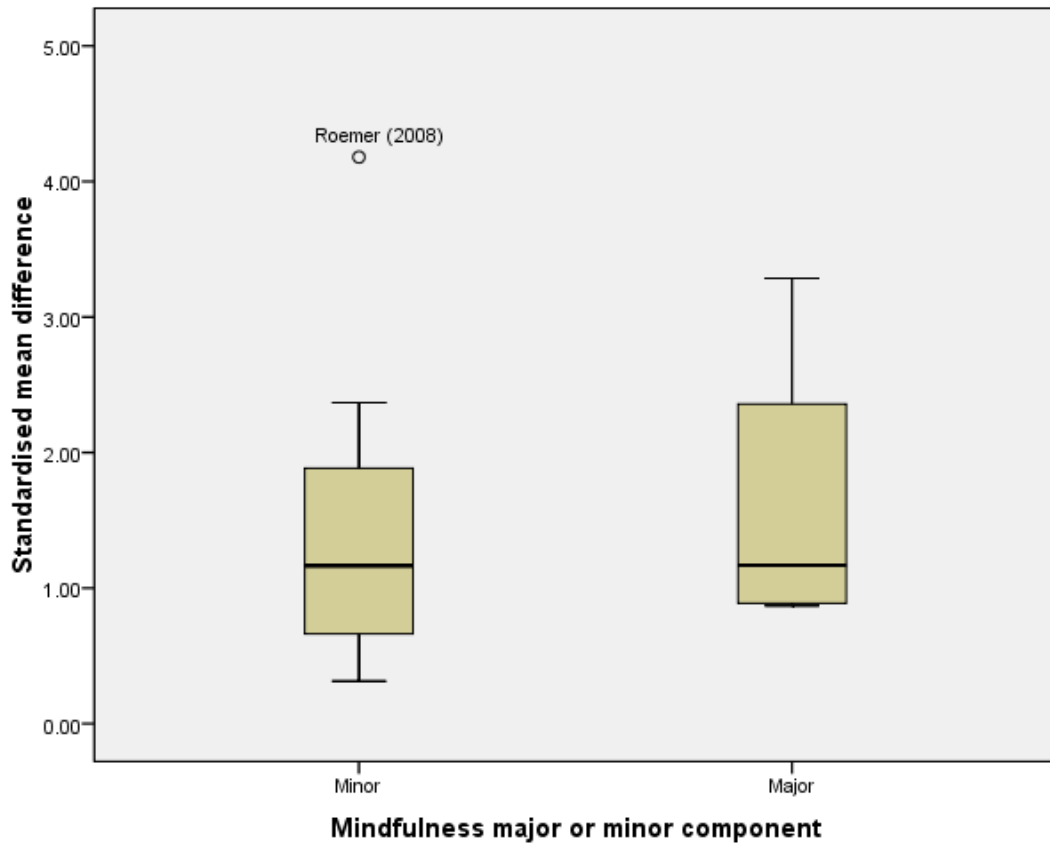
Figure 19. Box plot of standardised mean differences for studies with mindfulness as a minor and a major component of the therapy separately

The results of the meta-analysis are displayed in Table 27. These analyses used random effects models. Both analyses found a statistically significant effect of the therapies. Interestingly however, the major-minor comparison did not reveal a noticeable difference between these two groups. The test for between-group differences was not significant ($Q_T = 0.27$; df = 1; p = 0.605) and furthermore there was considerable overlap between their respective 95% CI suggesting that there was no significant difference between these therapies when grouped in this way.

Table 27. Meta-analysis of effect on anxiety of studies with mindfulness as a minor component, major component

Comparison	*k*	*N*	Effect size	95% CI (LL, UL)	Combined z score	Combined p value
Minor component, anxiety	8	139	1.24	0.71, 1.77	4.56	0.000
Major component, anxiety	4	78	1.48	0.74, 2.22	3.91	0.000

3.6 Chapter summary

I found evidence of effectiveness for the mindfulness-based therapies included in my analyses. These therapies appeared to be effective in reducing affective symptomatology across a number of Axis 1 conditions. It appeared unlikely that publication bias affected these results and some of the moderator analyses conducted returned significant findings. However all of these results need to be considered objectively and discussed further.

4. Discussion

4.1   Chapter outline

In this final chapter I consider my findings and discuss them in detail, with reference to the theoretical model of mindfulness within a therapeutic setting. I discuss the strengths and limitations of my research. Finally, I consider what the implications of my findings are for research and for practice.

4.2   The effectiveness of Mindfulness-Based Therapies

Overall, I found evidence for the effectiveness of Mindfulness-Based Therapies (MBT) in reducing levels of depression or anxiety mainly in patients diagnosed with depressive or anxiety disorders. Other diagnostic groups - that was eating disorders and Attention Deficit and Hyperactivity Disorder – did also benefit from MBT. I will discuss these findings based on the separate analyses I conducted.

The effect sizes discussed here have been estimated using random or mixed effects models. It is important to reiterate the difference in interpreting effect size parameters for the fixed as opposed to the random effects model (see also 2.6.3). A basic fixed effects model rests on the theoretical assumption that there is just one true effect – or 'fixed' effect - for a given intervention, however weak or strong this effect size may be. The observed variation in the effect sizes between studies is explained by a fixed effects model as being the result of the sampling error associated with each study. If the variation of the effect sizes is considerable, however it is likely that there are hidden *systematic* differences between the studies that enhance or hinder the effectiveness of the intervention. The difference may be for example the intensity or quality of the treatment. In the case of hidden systematic difference, a moderator analysis is required to uncover those important conditions which alter the degree of effectiveness for that treatment.

Within the context of psychotherapy research, the idea of a single true effect parameter, which is constant across a range of intervention studies, appears unrealistic. This is because both the therapeutic settings as well as the psychological conditions of the individual clients tend to vary considerably, even within the same diagnostic group. This variation can be regarded as 'natural' or 'unique' and is

generated by the interaction of the individuals involved – the clients (participants), therapists and researchers - in a specific context each with specific intentions and needs. A random effects model takes into account this 'natural randomness' between the studies. Rather than assuming just one true effect for an intervention, a random effects model replaces the notion of a single fixed effect with the idea of a distribution of effect parameters. Consequently a random effects model differentiates between two types of random error: sampling error within each study; and natural randomness between the studies.

The estimated average effect size of a random effects model is therefore a characteristic of the distribution of the effect sizes in all studies assumed to exist which employ a specific intervention. Since the random effects model regards the studies included in the meta-analysis as a sample of these studies, it enables the researcher to generalise the findings to the population of studies (Hedges & Vevea, 1998; Lipsey & Wilson, 2000). By contrast, a fixed effects model can never generalise the validity of its findings beyond those studies that were actually included in the analysis.

4.2.1   Mindfulness as a major component of therapy

The first aim of the meta-analysis was to establish whether therapies for which mindfulness formed a major component of the intervention were effective in treating various mental health problems. Therapies defined as having mindfulness as a major component were Mindfulness-Based Cognitive Therapy (MBCT); Mindfulness-Based Stress Reduction (MBSR); Mindful Awareness Practices (MAP); and Mindfulness-Based Eating Awareness Training (MB-EAT). Table 28 provides a summary of the main findings from this meta-analysis. This section included a total of 11 studies.

Table 28.   Main results of the major component meta-analyses

Comparison	k	N	Combined effect size	95% CI	p value
Depression (BDI)	11	327	8.73	6.61, 10.86	0.000
Moderator analyses					
Gender (% female)	11	327	*0.202**	*0.071, 0.333*	*0.026*
Type of therapy (MBCT)	7	251	*9.82*	*7.06, 12.57*	*0.000*
(other)	4	76	*6.85*	*3.18, 10.52*	*0.000*
Therapist training (detailed)	6	135	*6.91*	*4.24, 9.59*	*0.000*
(not detailed)	5	192	*10.47*	*7.77, 13.17*	*0.000*
Diagnostic group (depression)	7	251	*9.63*	*6.81, 12.45*	*0.000*
(other)	4	76	*7.24*	*3.50, 10.97*	*0.000*
Controlled studies	4	219	4.86	1.18, 8.54	0.01
Uncontrolled studies	7	219	*9.24*	*6.42, 12.06*	*0.000*
Controlled studies	4	108	*7.86*	*3.99, 11.74*	*0.000*
Anxiety (BAI)	7	168	5.64	3.29, 8.00	0.000

* This was the unstandardised parameter; all other effect sizes are unstandardised reduction scores

4.2.1.1 Pre-post treatment reduction in depression scores

This analysis found that for depression as an outcome there was a significant average pre-post reduction of 8.73 points on the BDI (Table 28) corresponding to a standardised effect size of 0.84 using the average standard deviation (SD) of the 11 pre-treatment SD (see Table 5) as the standardising variable. This suggests that therapies with mindfulness as a major component of the therapy are effective in treating current *active* depression, as measured by the Beck Depression Inventory (BDI).

It was important however to also consider whether this effect size was clinically significant. On the Beck Depression Inventory (BDI), clinical significance is normally indicated by a change in score which crosses the cut-off point for 'caseness' – that is minimal to mild depression. For the BDI-II, 0-13 represents minimal depression; 14-19 mild depression; 20–28 moderate depression; and 29–63 severe depression. Therefore a score that moves from a value of 14 or over to a value of 13 or less would be considered a clinically significant reduction.

The problem was that the studies varied considerably in their pre-treatment mean score and furthermore, as it was the mean score it cannot convey clinically significant benefit for the individual participants. For example, Kingston (2007) reported the largest drop and the treatment group's mean BDI score was in the 'severe' range. Ramel (2004) reported the smallest drop, however the starting mean BDI score in the treatment group was within the range that indicated 'minimal' depression (note that the diagnosis for Ramel (2004) was for a lifetime mood disorder and participants were participants did not need to be actively depressed). In analysing these data I found a link between the pre-treatment score and the reduction in depression score (ρ = 0.845; p = 0.001): the greater the severity the greater the reduction. This positive correlation was to be expected as it may be related to the famous 'regression toward the mean' phenomenon (Casella & Berger, 2001); where extreme scores at the end of the distribution of a variable have a greater potential for a change back towards the centre of the distribution.

4.2.1.2 *Moderators of effectiveness*

The result of the *Q*-test for homogeneity of the effects sizes suggested that the studies differed to the extent that moderator analyses were indicated. Several promising moderators were considered for a univariate mixed effects model analysis (Viechtbauer, 2005, 2007). However it is important to remember that the overall number of studies in the group was small (k = 11) and so the estimates of the random variance component may not be fully reliable.

Gender was coded in each of the studies as the percentage of females and 10 studies were included in this analysis. The relationship between effect size and gender was statistically significant and a marked degree of correlation was found (r = 0.698). The greater the proportion of females in the studies the greater the reduction in BDI score and this accounted for almost half of the variation in these scores. It is important though to not misinterpret this finding as simple gender effects as this would amount to an ecological fallacy (Robinson, 1950). Few studies reported information on gender differences so this correlation involves two aggregated variables (that is the effect size and the number of females). Whilst it tentatively suggests differential effectiveness of MBT for gender, this would have to be confirmed in a new meta-analysis involving effect sizes for both genders.

Another important point to note in this context is that moderator analyses provide evidence of an association: it is not possible to draw conclusions about causality. It is possible that the variables in the moderator analyses are confounders, which are linked to both outcome and a third variable. For example, all of these studies investigated the intervention in a group format. It is possible that women may respond better to these therapies than men in a group rather than an individual format.

Type of therapy was considered as a moderator by grouping MBCT ($k = 7$) and the other therapies separately. This attempted to determine whether therapy type accounted for some variation in the score. This finding was not significant and so it was not yet possible to conclude that MBCT was superior in its effectiveness compared to other major component therapies.

A further moderator analysis considered whether reporting of therapist training could account for a significant proportion of the variance in the BDI score. About half of these studies reported therapist training ($k = 6$). The analysis found a trend towards significance ($p = 0.0649$). Interestingly, this suggested that the effect size was greater by 3.5 points on the BDI in those studies which *did not* report training. However this could be explained by the higher mean pre-treatment score - of 3.5 points – across the studies not reporting training compared to those reporting training. This could again be because more severe participants were treated who had a much larger potential for a reduction in their symptoms.

Within this analysis I used quality of reporting as a proxy for the quality of training. I reasoned that if consideration was paid to the degree of training then it would be reported. However other factors (for example, article length) may also account for the lack of reporting. Using reporting as a proxy is not unusual within research. The most well-known examples are quality rating scales, which judge the methodological rigour of studies by how well they are reported (Jadad, et al., 1996).

The final moderator analysis looked at diagnostic group and compared depression ($k = 7$) with other mental health conditions. I did find a slightly higher effect size for the depression studies, although this was not statistically significant. Although not significant, I think the finding of a mean difference of 2.4 points on the BDI was actually quite interesting. It suggests that a reduction in depression scores was

obtained irrespective of the actual index condition. So for example, MTB was equally effective in reducing depression score for clients with ADHD as it was for those consulting with depression. It was recognised that this analysis was limited because the diagnostic grouping was crude; however the limited number of studies precluded more specific grouping.

4.2.1.3   Studies with a control group

Four of the studies included in the analysis of reduction in BDI score were controlled. All of these studies were of depression, although there were some differences in the form of the control group. I was interested in a meta-analysis of this subgroup because controlled studies tend to apply more stringent inclusion and exclusion criteria. This was done in order to generate homogenous groups of participants to strengthen the internal validity when comparing them on the outcome measures.

In this meta-analysis, which looked at the differences in the average change from pre- to post-treatment between the therapy and control group, and a significant reduction of 4.86 points in BDI score was found in favour of the therapy group. This was smaller than for the controlled and uncontrolled studies combined, which looked at the reduction pre- to post-treatment within the therapy groups only. The reason for the smaller amount of reduction was because there was also improvement in each control group. This was particularly the case in the two control groups that received other types of treatment; but far less in the two WL control groups (see Table 12). Furthermore, the lower limit of the 95% CI was 1.61 which represents a minimal change of questionable clinical significance.

What this finding suggests is that the effect size for a psychological intervention tends to be smaller when it is calculated relative to the amount of improvement within a control group, rather than as a simple pre-post reduction score within the intervention group only. Unfortunately the small number of studies within this subgroup precluded further investigation. A useful further analysis, for example, could have looked at comparing waiting list controls with 'active' controls to establish the degree of spontaneous remission or expectancy effects. It is however questionable whether there is much real difference. A waiting list control for psychological therapy may well be receiving other forms of care concurrently and so may improve somewhat anyway.

4.2.1.4   Reduction in anxiety scores

The Beck Anxiety Inventory (BAI) is one of the most widely used measures of anxiety symptomatology and was used as an outcome in seven of the studies. I found that using this as an outcome, MBT did reduce anxiety symptoms (Table 28). The effect size was small - 5.64 points on the BAI - but significant in the random effects model. This corresponds to a standardised effect size of 0.53 using the average SD of the seven pre-treatment SD (see Table 15) as the standardising variable.

These studies included three depression and four other diagnostic groups (see Table 12). As I investigated BAI reduction in these studies it was perhaps unsurprising that the reduction was less than that seen with a measure of depression. Perhaps this should particularly be the case as for example MBCT was specifically designed to target depression and the cognitive component of the therapy therefore addressed depression. However it was interesting that there was a significant reduction in these scores and this supplements the findings in the subsequent analysis discussed below.

Furthermore, there appeared to be a strong general improvement in anxiety and depression scores when considering those studies which did not have anxiety as the index condition ($k = 5$; $\rho = 1$; p = 0.000). When all studies were considered together, there did not appear to be any relationship between improvement in anxiety and improvement in depression scores ($k = 7$; $\rho = -0.036$; p = 0.939).

4.2.2   Mindfulness as a minor component of therapy

The aim of the meta-analysis was to establish the effectiveness of therapies considered to have mindfulness as a minor component. These 'minor component' studies included Acceptance and Commitment Therapy (ACT); Acceptance-Based Behaviour Therapy (ABBT); a Buddhist Counselling Programme (BCP); and Metacognitive Therapy (MCT). A total of eight studies were included in this analysis.

In this analysis most of the studies were single-group designs ($k = 5$) however there were three Randomised Controlled Trials (RCT). I therefore conducted a meta-analysis of the amount of reduction in anxiety levels from pre- to post-treatment in the intervention groups only. A total of 139 participants were included. Only four of the

studies used the BAI, so a standardised mean reduction score (Morris & DeShon, 2002) was used for the effect size in order to compare studies with different anxiety outcome measures. All of the outcomes were subjective measures of anxiety symptomatology.

I found a significant effect for these studies in the random effects model with a standardised mean difference of 1.24 (95% CI = 0.81, 2.10). This suggests a considerable reduction - well above 1 standard deviation - indicating a very strong effect. However the significant test for heterogeneity was significant suggests this should be interpreted with caution. Further moderator analyses were indicated by this finding however as the number of studies within the original pool was small ($k = 8$), they were not conducted.

4.2.3 Mindfulness as a major vs. minor component of therapy

The final analysis used the studies included in the analysis of treatment for anxiety disorders. It compared the studies with mindfulness as a minor component ($k = 8$) with those defined as having mindfulness as a major component ($k = 4$). Again it used the anxiety outcomes and so the standardised mean difference for the effect size. For both groups there was a statistically significant effect and interestingly, it found very similar effect sizes for these two groups of studies. For those with mindfulness as a minor component the effect size was 1.24 (95% CI 0.71, 1.77); and as a major component 1.48 (95% CI 0.74, 2.22). Although the effect size for the major component group was slightly larger, unsurprisingly the confidence interval was wider as there were fewer studies in this group and there was considerable overlap between these two confidence intervals. However for both groups the lower value of the confidence interval indicated a medium to large effect size.

I think what was interesting about this finding was that the amount of mindfulness did not seem to make a difference to the size of the effect – there did not appear to be a 'dose-response' relationship. These therapies were equally effective irrespective of the amount of mindfulness involved. However it was difficult to definitively state exactly how much mindfulness was included in the 'minor' component group; or indeed how much was actually practised by the participants within the 'major' component studies. Although it was easier to know how much was taught within the

major component group as these studies employed a group format with clearly defined treatment manuals.

With the minor component studies, the amount of mindfulness teaching involved was often stated for the purpose of the research. With ACT *in practice*, it is often a clinical decision. It will depend on the individual's presentation and more or less emphasis will be placed on it accordingly – or the intervention will be tailored to the individual (Steven C. Hayes, Strosahl, & Wilson, 1999). Again however, it was difficult to state how much it was actually practised as 'homework'. There is a considerable requirement placed on home practice with MBCT; typically up to 45 minutes a day, five days a week during the duration of the course (Segal, et al., 2002). Ideally, measures of mindfulness would help establish the extent to which improvement in clinical symptomatology is coupled with improved mindfulness skills.

In this analysis I pooled anxiety conditions. Of the 12 studies, six included participants with a diagnosis of Generalised Anxiety Disorder, two of which may have had an accompanying diagnosis of panic disorder. Of the remaining five studies, three included participants with a diagnosis of Social Anxiety Disorder; one anxiety and depression; one included any anxiety disorder; and one trichotillomania. I think if the number of studies were greater it would have been useful to conduct a moderator analysis to determine whether some anxiety conditions were more responsive to these approaches. However the numbers prevented this particularly as these diagnoses were more similar than in the moderator analysis for the major component studies (reported in Section 4.2.1.2 above).

4.2.4   Summary of main findings

My findings suggest that MBT are an effective treatment for reducing current, active anxiety and depression. In the introduction I discussed the basic model of mindfulness, which suggests mindfulness works by changing the relationship with inner experience. If thoughts are not 'bought' – if there is less attachment to our thoughts and less belief in them as true - then the model suggests they will have less impact on affect. Negative thoughts are an important component in the development and maintenance of depression and anxiety. If the relationship between thought and emotion is changed – and becoming aware of automatic negative thought processes

would be the first step - a corollary would be that the experience of symptomatology will be less painful. There is an important paradox at work within mindfulness practise. By changing or letting go of the desire to reduce symptomatology and by developing a more accepting attitude towards one's suffering, it paradoxically reduces it.   By striving less and letting go of the desire to get rid of the symptoms, they are reduced (Steven C. Hayes, et al., 1999; Segal, et al., 2002).

4.3   Limitations of my findings

4.3.1   Studies not included in these analyses

I think the most serious limitation of my meta-analysis was that I was not able to code and include all of the relevant studies I identified. Almost half of the studies (n = 19) entered into the meta-analysis study pool were not included in any of the analyses. These exclusions were made because the scope of this project meant it was not possible to conduct a comprehensive analysis of all of the studies. Therefore decisions were made on which data to include. The pool included studies investigating a number of different conditions, using a number of different outcomes at different follow-up points. The decisions were pragmatic based on what *meaningful* analyses could reasonably be conducted, with the largest number of studies.

Not all forms of therapies considered to include a mindfulness component were included. In some cases this was because retrieved studies did not fulfil the criteria – and were therefore excluded in the eligibility screen - however this was not always the case. The most notable was Dialectical Behaviour Therapy (DBT), which actually accounted for the greatest number of effectiveness studies retrieved and assessed for eligibility. The majority of these investigated Borderline Personality Disorder rather than Axis 1 disorders, and so were not eligible. However there has been some recent interest in applying DBT to a number of other conditions. These include depression (Goldstein, Axelson, Birmaher, & Brent, 2007; Harley, Sprich, Safren, Jacobo, & Fava, 2008; Lynch, et al., 2007); ADHD and Oppositional Defiant Disorder (Hesslinger, et al., 2002; Nelson-Gray, et al., 2006; Philipsen, et al., 2007)]; and binge eating disorder (Telch, Agras, & Linehan, 2001). Unfortunately these studies did not fit into any of the analyses conducted.

4.3.2   Study coding

It was necessary to group together in my analysis disparate conditions; for example anxiety, depression, eating disorder and ADHD. This introduces heterogeneity and was a decision informed by the questions asked of these data. This type of grouping has precedent in the meta-analytic literature and is acceptable if it is done appropriately with a clear rationale. For example, a landmark meta-analysis published in a leading medical journal (Linde, et al., 1997) pooled all studies of homeopathy – irrespective of condition – into the main analysis. The paper investigated whether there was any evidence for homeopathy over and above placebo. It was therefore appropriate as it was done with reference to the hypothesis.

It was acknowledged that this may have resulted in a loss of some meaning, however I think it was a more pragmatic approach to these data; especially considering the limited number of studies. The DSM-IV (American Psychiatric Association, 2000) categories are a form of short-hand for clinicians and in summarising the evidence provide a useful structure. In the real-world of practise however it is unlikely that DSM-IV criteria will be applied to every client and furthermore, there exists controversy over the use of diagnoses. It is more likely that the question asked will be something like, "will this therapy reduce anxiety or depression in my client?" and this question may be asked irrespective of the index condition.

4.3.3   Possible sources of bias

One possible source of bias I may have introduced into my research was selection bias. This form of bias is where included studies were unrepresentative of the current practice of mindfulness therapies. I think this was unlikely. My search strategy was comprehensive and I included articles in the English or German languages. The databases I searched – psycINFO® and Medline® - were the most comprehensive databases available within the fields of psychology and medicine respectively. Furthermore I excluded a large proportion of studies at the screening stage, which suggests the search strategies were comprehensive.

It was difficult to comment precisely on what may have been missed but I think it unlikely there were a considerable number of studies excluded which would affect the results. Although I did not include grey literature such as dissertations in my analysis I

think it was unlikely that these would represent a radically different approach to the use of these therapies or the findings of these studies. However publication bias was a possibility – studies which may add to the literature even though they find a negative result may not be considered as they are not as interesting. The analyses of publication bias I conducted failed to show any indication of a systematic exclusion of studies.

I did not include studies which described the intervention as meditation. There were also examples in the literature investigating 'mindful exercise' (Chow & Tsang, 2007; Tsang, Chan, & Cheung, 2008); which could include yoga (Schure, Christopher, & Christopher, 2008). Again, this omission reflected the question asked. I was interested in modern, psychology-led applications of mindfulness which are typically informed by existing psychological theory and practice.

4.4   The strengths of my analysis

One of the strengths of this study was that it used meta-analytic methods to determine the effectiveness of therapies, a well-recognised method of summarising evidence which directly informs therapeutic guidelines.

By using anxiety and depression outcomes – and where possible, the same outcomes at the same time points – my results were clinically relevant and I think this was an important strength of this research. To be meaningful, meta-analysis requires an aggregation of data from a number of studies and as these studies were not identical, it inevitably required working with what was available. I think this was particularly relevant in determining which outcomes to focus on and which to combine. Most of the studies presented several outcome measures. Using them all would either have prevented a succinct overview of these data, as I would have had to report many comparisons; or it would have required aggregation of a number of disparate measures. I think it was inappropriate to reduce all outcome measures from one study into a single effect size; or even two effect sizes, with one for mental health outcomes and one for physical health outcomes. I think this loses too much detail and does not answer clinically relevant questions asked of these data. For example, a clinical psychologist is more likely to ask, "Will this therapy improve my client's

mood?" rather than, "Will this therapy improve my client's mental health?" Furthermore it may give a misleading impression of these therapies. As an example the therapies may have been very effective with depression outcomes but not with ADHD outcomes; by pooling these together it would have given a false impression of the effectiveness of both.

A further strength of this study was its independence from any one particular therapeutic modality. Studies of mindfulness-based therapies are most likely to be conducted by practitioners of the therapies. For example, Williams and Teasdale (MBCT), Hayes (ACT), Linehan (DBT) and Wells (MCT) are all originators and leading researchers for their respective forms of therapy. This may lead to confirmation bias in the interpretation of the results to suit pre-conceived ideas about the therapy.

4.5   What this adds to the literature

4.5.1   Implications for research

I found evidence for the effectiveness of MTB however due to the small number of current studies there is a need for further research. The small number of studies influenced coding and categorisation and it would be useful for more research to allow a meta-analysis of for example, specific types of anxiety disorder.

There is considerable interest in operationalising of mindfulness as a concept to allow direct measurement. A number of recently developed measures attempt to assess factors unique to the method of treatment by quantifying what is purported to be the target of the practice of mindfulness; although the difficulties inherent in these attempts is recognised (Grossman, 2008). However these have proliferated, reflecting variation in definition (see 1.3.1). At the time of writing I was aware of over 10 measures of either mindfulness or constructs related to the concept of mindfulness. The earliest developed scales are beginning to find their way into the effectiveness literature, such as the Mindful Attention Awareness Scale (K. W. Brown & Ryan, 2003); and the Freiburg Mindfulness Inventory (FMI; Walach, Buchheld, Buttenmuller, Kleinknecht, & Schmidt, 2006).

When considering the effectiveness of therapy it is important to consider whether treatments have a lasting effect. Currently the lack of adequate follow-up limits the usefulness of these data on the effectiveness of MBT. For example, one study (Miller, et al., 1995) which was considered for inclusion presented the three-year follow-up data from an earlier trial (Kabat-Zinn, et al., 1992). This study would have been a very useful addition to the analysis however there were no other studies with a similar length of follow-up to appropriately combine with these data. One scale developed for assessing the quality of psychological therapies (Yates, Morley, Eccleston, & Williams, 2005) considers treatment follow-up of less than six months as 'inadequate'. I think studies with follow-up less than this period do still provide useful data however are limited. I think there are problems inherent in longer trials and so pragmatically it is not always possible to undertake them. One of the most relevant factors is that attrition usually increases with longer follow-up so there are considerable resource implications for running longer studies. Furthermore longer follow-up increases the chance of the use of other therapies or life changes which may affect outcome scores and be difficult to control for between groups.

A limitation of my findings stems from the number of uncontrolled studies in the literature. I aimed to include both controlled and uncontrolled studies to ensure a more comprehensive consideration of the effectiveness of MBT. However the number of controlled studies was disappointingly small. Controlled studies can provide a more robust estimate of effectiveness.

Furthermore, the types of control used in the studies are also relevant. Waiting list (WL) or treatment as usual (TAU) controls are the most common encountered within these MBT studies. Within a TAU comparator there may for example be considerable variation in what participants receive and in a WL control attempts to get better may be wither consciously or unconsciously deferred by participants. Furthermore MTB is a novel therapeutic approach which is generating considerable interest in the scientific literature (Figure 3). This will undoubtedly be reflected in the media (University of Iowa Health Care). It is possible that this may create expectancy effects influence either the therapist or the participants' engagement and inflate outcomes when there is no control group.

My research highlighted a number of issues relating to the quality of reporting. Useful moderator analyses were prevented by lack of description within the studies. For example, more description on the training of therapists, particularly with regard to their own practise, would be useful. More detail provided and discussion in the analysis would also be useful. The meta-analysis reported here highlights the difficulty in determining a mechanism of therapeutic action. The current literature fails to consistently report on variables which may mediate the effect and thus help determine a mechanism of therapeutic action. This lack of reporting has been noted elsewhere (Singh, Lancioni, Wahler, Winton, & Singh, 2008). I think a consensus statement providing guidelines on the reporting of mindfulness research would be a useful addition to the literature.

A useful controlled addition which is a good example of a more sophisticated use of controls will be Abbot et al. (2007, 2009; in preparation). This study had three arms comparing mindfulness training, CBT, and waiting list control groups and furthermore, a long follow-up period. This may answer questions regarding the effectiveness as compared to other forms of therapy – as well as the specific effects of mindfulness – and of its lasting benefit. The study investigated Generalised Anxiety Disorder and randomised approximately 100 participants and followed the participants up over 19 months. Unfortunately these data could not be included in my research. Although the study was summarised in an earlier book chapter (Abbott, 2007) at present not enough information has been published to allow meta-analysis. I contacted Maree Abbott who provided some useful additional data (Abbott, 2009), which unfortunately was still not enough information to allow inclusion in this meta-analysis. Abbott's maternity leave prevented her providing a more detailed summary and the forthcoming publication is eagerly anticipated (Abbott, et al., in preparation).

Dismantling studies could also potentially provide useful information about the relative benefit of the mindfulness component of therapy. All of the therapies detailed here have other components and while my crude categorisation of minor component vs. major component did not show any difference in effect between the two, it only included a small number of studies in each group. The more recent forms of MBT have a considerable component of 'other' psychological therapy. It is not clear whether mindfulness and these other components have a synergistic effect and the ideal balance between these components, if indeed there is an ideal. It is unclear

whether a more individualised treatment programme is the best option. It is notoriously difficult however to tease apart specific and non-specific effects of therapies (Fisher, McCarney, Hasford, & Vickers, 2006). For example, presenting a formulation brings an expectation of expertise. This expectation may then produce or influence an effect.

A recently reported study (Carmody & Baer, 2009) suggests research is becoming more sophisticated and answering questions about for example how best to implement these therapies. This study looked at the relationship between effect size and the number of class contact hours and found no significant difference. This has implications not just for resources but I think raises important questions about home practice, which have been investigated before. Home-practice has been monitored (Gross, et al., 2004) and when the aim is to undertake 45 minutes a day, five days a week it is a considerable commitment (Allen, et al., 2006) which is not always followed. Other studies have asked about the skills practised in DBT (Lindenboim, et al., 2007) and adherence to practice is an important variable for longer-term follow-up. An early study which looked at the continuing meditation practise for individuals referred for relaxation training found over half had stopped their practise (Delmonte, 1988). However this study was small (n = 38), focused only on one type of condition (relaxation training; it is possible that once the desired result was achieved participants gave up the practice); and looked at meditation, which requires considerable commitment in terms of time. Modern MBT practices tend to try and integrate practice more into the 'modern lifestyle', for example the 'three-minute breathing space' and 'mindful walking' (Segal, et al., 2002).

I think there is a considerable need for additional qualitative research to determine to provide useful insight into clients' experiences of these therapies. There are some good examples already in the literature (Carroll, Lange, Liehr, Raines, & Marcus, 2008; Fonteyn & Bauer-Wu, 2005; Higginson & Mansell, 2008; Hodgetts, Wright, & Gough, 2007; A. Smith, Graham, & Senthinathan, 2007). A good example which shows how this type of research can add value is Fonteyn (2005). This study used qualitative methodology to amend the mindfulness techniques and then subsequently tested them using quantitative methodology. I think these studies provide useful evidence into the acceptability and popularity of MBT; however they could also inform the development of these therapies.

I think another important consideration for future research is the use of outcomes with client-generated variables. This would be more in keeping with the idea of getting in touch with values, which is explicit in the ACT model and arguably implicit in the other forms of MBT.

There has also been a very recent and interesting look at how similar our Western concept of mindfulness is to the Eastern concept (Christopher, Christopher, & Charoensuk, 2009) – and found that there may be important differences in the relative conceptualisations. Revisiting the idea of the origins of mindfulness, I think this may highlight that there is still much to research from the ancient practices but also much to learn about how these can be augmented with modern Western psychological technologies.

4.5.2 Implications for practice

In considering the implications for practice, I will recap on what was known about these therapies from other published reviews. I outlined some of the issues with these reviews in my introduction (see 1.8.2); here I will summarise the findings as they were reported.

Öst (2008) found in his meta-analysis of that ACT and DBT were effective therapies for psychiatric disorders, with effect sizes of 0.66 and 0.58 respectively. Coelho, Canter & Ernst (2007) did not attempt to combine these data however reported there was evidence that MBCT providing additional benefit to standard care for the treatment of recurrent depression (three or more episodes). Toneatto & Nguyen (2007) also did not attempt to meta-analyse these data and reported there was no evidence supporting MBSR (and MBCT) for the treatment of active anxiety and mood disorders. A descriptive review of ACT and DBT by Hayes et al. (2004) reported evidence of effect over a wider range of conditions. Grossman et al. (2004) reviewed MBSR across all studies of psychological as well as physical health conditions. The meta-analysis found evidence of effectiveness and reported an effect size of 0.54 for mental health outcomes. Finally Baer (2003) reported for MBCT and MBSR an overall weighted effect size of 0.59; and in the subgroup analyses an effect size of 0.86 for depression measures and 0.70 for anxiety measures. The findings of these reviews, along with the findings of my review, were summarised in Table 29 below.

Where applicable the most relevant results in those meta-analyses conducted are presented and for clarity, studies identified by lead author and year.

Table 29.   Summary findings of reviews of Mindfulness-Based Therapies

Study	Included therapies	Conditions	Meta-analysis	Effect size*	Evidence of effectiveness?
McCarney (2009)	MAP MBCT MBSR MB-EAT ABBT ACT BCP MCT	(Axis 1) psychological disorders	✓	0.84† 0.53 1.24	✓
Öst (2008)	ACT DBT	Psychiatric disorders	✓	0.66‡ 0.58	✓
Coelho (2007)	MBCT	Recurrent depression	✗	--	✓
Toneatto (2007)	MBCT MBSR	Anxiety and mood disorders	✗	--	✗
Hayes (2004)	ACT DBT	All clinical and non-clinical populations	✗	--	✓
Grossman (2004)	MBSR	All health problems	✓	0.54§	✓
Baer (2003)	MBCT MBSR	All health problems	✓	0.86† 0.70	✓

* All reported effect sizes are standardised mean differences
† Effect size presented separately for measures of depression (top line) and measures of anxiety (bottom line)
‡ Effect size presented separately for ACT studies (top value) and DBT studies (bottom value)
§ All 'mental health' outcomes

My research suggests that based on current evidence, mindfulness-based therapies have a role in reducing current distress from anxiety and depression in psychological conditions.

In treating depression, the early application of mindfulness-based therapies was MBCT in the prevention of relapse (Segal, et al., 2002). Early clinical trials of MBCT (Ma & Teasdale, 2004; Teasdale, et al., 2000) reported a significant reduction in relapse rates for individuals with three or more episodes of depression. Limited support for this was reported in Coelho et al.'s (2007) review however Toneatto and Nguyen (2007) found no evidence of effectiveness in treating mood disorders in general from the controlled research.

My findings therefore represent an important addition to the effectiveness literature for depression. Recent research has shown an interest in MBCT for treating active depression and my findings support the application of MBT with mindfulness as a major component in this condition. Furthermore, there was some evidence supporting this from controlled studies.

Some of the earliest research into MBT – specifically MBSR - was in its application for treating anxiety. Only the Toneatto and Nguyen (2007) specifically looked at anxiety disorders and this did not find any evidence of effectiveness within the controlled literature. I did not look separately at the controlled literature however I did include these study designs. I found evidence of effectiveness for both minor and major component MBT in reducing anxiety symptomatology. This also represented an important addition to the literature.

I have concentrated on outcomes measuring symptomatology as this was the focus within these studies. Using studies of symptomatology for outcome in MBT represents an interesting issue. Theoretically, the intended aim of increased mindfulness is not to target symptomatology directly rather it is to change the relationship with that symptomatology. We may still have negative thoughts, however they have less influence. This suggests it will not necessarily reduce symptomatology scores however paradoxically an increased mindfulness *is* likely to reduce the symptomatology. Within the theory there is the idea of 'primary' (or 'clean') pain and 'secondary' (or 'dirty') pain (Segal, et al., 2002). There is the initial event which we cannot change (for example, we feel sad because we are ignored by someone – which is primary pain). However it is our response to this difficulty that mindfulness targets (for example, the wish to avoid feeling sad – the secondary pain). Therefore reducing this distress is likely to reduce overall distress as commonly used measures of symptomatology are not sophisticated enough to distinguish between these two types of distress. This was supported by my research.

With outcome on the BDI, I did find that the effect size was less – although not significantly - when the intervention group was compared to a control group. It was unsurprising that the *relative* reduction in scores was less and this was to be expected, particularly with 'active' control groups, as one would hope that they will

also be producing a clinical effect. It did however find that MTB were more effective than the controls used.

The importance of these findings can be seen in relation to currently existing guidelines and the amount of detail presented on MBT. For example, a recent collaborative publication between the Royal College of Psychiatrists and Royal College of General Practitioners (2008) provided guidance on the use of psychological therapies in psychiatry and primary care. It gave a very limited mention for MBCT as a therapy which has "*proven valuable in the prevention of further episodes of illness*" (p.14: Royal College of Psychiatrists, 2008) – the illness they believe it may prevent was unspecified.

Similarly reference to MBT is limited within NICE guidelines. There is no mention of MBT in the current anxiety guidelines (McIntosh, et al., 2004), however these are due for an update. In their amended depression guidelines focus on the preventative aspect of 'Mindfulness-based CBT' [*sic*], stating it, "*should be considered for people who are currently well but have experienced three or more previous episodes of depression, because this may significantly reduce the likelihood of future relapse*" (p.37: NICE, 2007). Interestingly, it is referred to as mindfulness-based CBT. Although MBCT is considered a form of CBT I think this title is potentially misleading. It represents a strong association with CBT and may suggest a broader evidence base because of this (Adams, 2008). I think it is important to clearly delineate MBT as a group of therapies as the approach they take is somewhat different to 'traditional' CBT.

Some modern CBT techniques do not focus nearly as much on challenging thoughts (Butler, Fennell, & Hackman, 2008). A principal difference between traditional CBT and MBT appears to be that CBT spends more energy evaluating the validity of thoughts by asking if they are true and encouraging individuals to see things from a different perspective by challenging thoughts. In contrast, MBT places far more emphasis on changing the relationship to thoughts. It promotes meta-cognitive skills, where a perspective is gained so that it is possible to stand back from thoughts and tolerate them rather than challenge them directly. From this meta-cognitive perspective it is easier to see thoughts as mental phenomena, just thoughts, rather than as facts.

Both these approaches get to a similar position, which is that thoughts are not facts and do not have to be accepted as true. The extent to which people in CBT actually learn this as a meta-belief is still unclear. For example, someone may learn that their thoughts about their anxiety are not accurate. The chest pain is not necessarily an indicator of having a heart attack; or people are not as likely to reject me as I thought. However they might not get the general rule that thoughts are mental events and not facts. Another difference is that CBT holds that we have thinking 'errors', whereas mindfulness is more concerned with workability or taking 'skilful action', and not getting tied up into labelling thoughts in this way. Steven Hayes argues that CBT starts from an assumption of healthy normality, so depression is dysfunctional and different from the norm (Steven C. Hayes, 2008). MBT starts from the assumption that suffering is a part of the human condition, which arguably normalises rather than pathologises suffering.

Ultimately it was difficult to say whether the mindfulness component was responsible for the effectiveness found within this review. However what was clear was that the therapy 'package' of MBT may provide benefit. When conducting research of this nature it is important that it translates into a real benefit in practice. I think the conclusion of my research is that some clients can gain from this form of therapy and I aim to publish my findings to inform and develop practice.

4.6 Conclusion

Mindfulness-based therapies are an important, current technology within the cognitive-behavioural tradition. They have their roots within meditative practice dating back thousands of years and their current growth and development within the field of clinical psychology are considerable. These therapies are based on changing the relationship with difficult inner experience and the theory suggests this may play an important role in reducing the impact of affective symptomatology. There is a growing evidence base for these therapies however reviews have not comprehensively summarised their effectiveness using meta-analytic techniques. My aim was to contribute to this evidence base and inform practice, by conducting a meta-analysis of these therapies.

I considered these therapies within two main groups: those with mindfulness as a major component and those with mindfulness as a minor component. My analysis looked at these groups separately and found evidence of effectiveness in reducing active depression and anxiety. There were a number of methodological limitations to the research. I was not able to include all of the studies I identified and most of the included studies were uncontrolled. However these findings represent an important addition to the literature which has implications for subsequent research and practice.

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6. Appendix

6.1 Abbreviations and acronyms

6.1.1 Project team

AG	Andrew Grey
JS	Jörg Schulz
RM	Robert McCarney (author)
SW	Susannah Walker

6.1.2 Therapies, outcome measures, statistics and others

95% CI	95% Confidence Interval
AAQ-II	Acceptance and Action Questionnaire - version 2
ABBT	Acceptance-Based Behaviour Therapy
ACT	Acceptance and Commitment Therapy
ADHD	Attention Deficit and Hyperactivity Disorder
BCP	Buddhist Counselling Programme
BDI (-II)	Beck Depression Inventory (version II)
BMI	Body Mass Index
CAMS-R	Cognitive and Affective Mindfulness Scale-Revised
CAS	Cognitive Attentional Syndrome
CBASP	Cognitive Behavioural Analysis System of Psychotherapy
CBGT	Cognitive Behavioural Group Therapy
CBT	Cognitive Behavioural Therapy
CM	Concentration Meditation
CT	Cognitive Therapy
DF	Degrees of Freedom
DBT	Dialectical Behaviour Therapy
EBCP	Evidence-Based Clinical Practice
EQ	Experiences Questionnaire
ES	Effect Size
FAP	Functional Analytic Psychotherapy
FFMQ	Five Facet Mindfulness Questionnaire
FMI	Freiburg Mindfulness Inventory

GAD	Generalised Anxiety Disorder
HDRS	Hamilton Depression Rating Scale
H/O	History of
IBCT	Integrative Behavioural Couple Therapy
k	Number of studies (included in meta-analysis)
KIMS	Kentucky Inventory of Mindfulness Skills
LL	Lower Limit (to 95% confidence interval)
MAAS	Mindful Attention Awareness Scale
m-ADM	maintenance Anti-Depressant Medication
MAP	Mindful Awareness Practices
MBCT	Mindfulness-Based Cognitive Therapy
MB-EAT	Mindfulness-Based Eating Awareness Training
MBPM	Mindfulness-Based Pain Management
MBSR	Mindfulness-Based Stress Reduction
MBRE	Mindfulness-Based Relationship Enhancement
MBRP	Mindfulness-Based Relapse Prevention
MBT	Mindfulness-Based Therapies
MCT	Meta-Cognitive Therapy
MDD	Major Depressive Disorder
MM	Mindful Meditation
MR	Mental Retardation
N	Combined number of participants (included in meta-analysis)
n	Number (of citations)
NHS	National Health Service
NICE	National Institute for Health and Clinical Excellence
NRCT	Non-Randomised Controlled Trial
ODD	Oppositional Defiant Disorder
p	Probability value
PD	Panic Disorder
PDD	Pervasive Developmental Disorder
PMS	Philadelphia Mindfulness Scale
PRISMA	Preferred Reporting Items for Systematic reviews and Meta-Analyses
RCT	Randomised Controlled Trial
RDD	Recurrent Depressive Disorder
Q_T	Q-Test for homogeneity

QRCT	Quasi-Randomised Controlled Trial
QUORUM	QUality Of Reporting of Meta-Analyses
SAD	Social Anxiety Disorder
SMD	Standardised Mean Difference
S-REF	Self-Regulatory Executive Function
SMQ	Southampton Mindfulness Questionnaire
TAU	Treatment As Usual
TMS	Toronto Mindfulness Scale
UL	Upper Limit (to 95% confidence interval)
UT	Uncontrolled Trial
WASI	Wechsler Abbreviated Scale of Intelligence
WL	Waiting List

6.2 NICE Hierarchy of evidence and recommendation grading scheme

Level	Type of evidence	Grade	Evidence
I	Evidence obtained from a single randomised controlled trial or a meta-analysis of randomised controlled trials	A	At least one randomised controlled trial as part of a body of literature of overall good quality and consistency addressing the specific recommendation (evidence level I) without extrapolation.
IIa	Evidence obtained from at least one well-designed controlled study without randomisation	B	Well-conducted clinical studies but no randomised clinical trials on the topic of recommendation (evidence levels II or III); or extrapolated from level I evidence.
IIb	Evidence obtained from at least one other well-designed quasi-experimental study		
III	Evidence obtained from well-designed non-experimental descriptive studies, such as comparative studies, correlation studies and case studies		
IV	Evidence obtained from expert committee reports or opinions and/or clinical experiences of respected authorities	C	Expert committee reports or opinions and/or clinical experiences of respected authorities (evidence level IV). This grading indicates that directly applicable clinical studies of good quality are absent or not readily available.
		GPP	Recommended good practice based on the clinical experience of the GDG.
NICE	Evidence from NICE clinical guideline or technology appraisal	NICE	
Adapted from Eccles M, Mason J (2001). How to develop cost-conscious guidelines. Health Technology Assessment 5 (16)			
NHS Executive. Clinical Guidelines: Using Clinical Guidelines to Improve Patient Care Within the NHS. London: 1996.			

6.3 Methodology

6.3.1 Search strategies

6.3.1.1 psycINFO

1. mindfulness.mp. or exp Mindfulness/
2. MBCT.mp. [mp=title, abstract, heading word, table of contents, key concepts]
3. MBSR.mp. [mp=title, abstract, heading word, table of contents, key concepts]
4. DBT.mp. [mp=title, abstract, heading word, table of contents, key concepts]
5. dialectical behavio*.mp. [mp=title, abstract, heading word, table of contents, key concepts]
6. MB-EAT.mp. [mp=title, abstract, heading word, table of contents, key concepts]
7. MBRE.mp. [mp=title, abstract, heading word, table of contents, key concepts]
8. commitment therap*.mp. [mp=title, abstract, heading word, table of contents, key concepts]
9. 1 or 2 or 3 or 4 or 5 or 6 or 7 or 8
10. ((singl$ or doubl$ or trebl$ or tripl$) adj25 (blind$ or dummy or mask$)).mp. [mp=title, abstract, heading word, table of contents, key concepts]
11. placebo$.mp. [mp=title, abstract, heading word, table of contents, key concepts]
12. random$.mp. [mp=title, abstract, heading word, table of contents, key concepts]
13. crossover.mp. [mp=title, abstract, heading word, table of contents, key concepts]
14. assign$.mp. [mp=title, abstract, heading word, table of contents, key concepts]
15. allocat$.mp. [mp=title, abstract, heading word, table of contents, key concepts]
16. ((clin$ or control$ or compar$ or evaluat$ or prospectiv$) adj25 (trial$ or studi$ or study)).mp. [mp=title, abstract, heading word, table of contents, key concepts]
17. exp Placebo/
18. exp Treatment Effectiveness Evaluation/
19. exp Mental Health Program Evaluation/
20. exp Experimental Design/
21. versus.id.
22. vs.id.
23. case report.mp. or exp Case Report/
24. case series.mp.
25. interrupted time-series.mp.
26. case stud$.mp.
27. (equivalent adj group$).mp. [mp=title, abstract, heading word, table of contents, key concepts]
28. (test adj retest).mp. [mp=title, abstract, heading word, table of contents, key concepts]
29. exp Clinical Trials/
30. (clinical adj trial$).mp. [mp=title, abstract, heading word, table of contents, key concepts]
31. 10 or 11 or 12 or 13 or 14 or 15 or 16 or 17 or 18 or 19 or 20 or 21 or 22 or 23 or 24 or 25 or 26 or 27 or 28 or 29 or 30
32. 9 and 31

6.3.1.2 Medline

1. "randomized-controlled-trial" [Publication Type]
2. "controlled clinical trial" [Publication Type]
3. randomized controlled trials as topic [MeSH Terms]
4. "random allocation" [MeSH Terms]
5. "double blind method" [MeSH Terms]
6. "single blind method" [MeSH Terms]
7. "clinical trial" [Publication Type]
8. clinical trials as topic [MeSH Terms]
9. "clin*" [Title/Abstract] AND "trial*" [Title/Abstract]
10. "sing*"
11. "doubl*"
12. "tripl*"
13. "treb*" AND "blind*"
14. "mask*"
15. "dummy*"
16. "placebos" [MeSH Terms]
17. "placebo*" [Title/Abstract]
18. "random*" [Title/Abstract]
19. "research design" [MeSH Terms]
20. comparative stud*
21. evaluation studies as topic [MeSH Terms]
22. "follow up studies" [MeSH Terms]
23. "prospective studies" [MeSH Terms]
24. "control*" [Title/Abstract]
25. "prospectiv*" [Title/Abstract]
26. "volunteer*" [Title/Abstract]
27. "crossover"
28. "case report" [Title/Abstract]
29. "case stud*" [Title/Abstract]
30. "equivalent group*" [Title/Abstract]
31. "test retest" [Title/Abstract]
32. "interrupted time series" [Title/Abstract]
33. (#1) OR (#2) OR (#3) OR (#4) OR (#5) OR (#6) OR (#7) OR (#8) OR (#9) OR (#10) OR (#11) OR (#12) OR (#13) OR (#14) OR (#15) OR (#16) OR (#17) OR (#18) OR (#19) OR (#20) OR (#21) OR (#22) OR (#23) OR (#24) OR (#25) OR (#26) OR (#27) OR (#28) OR (#29) OR (#30) OR (#31) OR (#32)
34. "mindfulness" [Title/Abstract]
35. "MBCT" [Title/Abstract]
36. "MBSR" [Title/Abstract]
37. "DBT" [Title/Abstract]
38. "dialectical behavio*" [Title/Abstract]
39. "MB-EAT" [Title/Abstract]
40. "MBRE" [Title/Abstract]
41. "commitment therap*" [Title/Abstract]
42. (#34) OR (#35) OR (#36) OR (#37) OR (#38) OR (#39) OR (#40) OR (#41)
43. (#33) AND (#42)

6.3.2 Screening form

Mindfulness-based therapies for psychological conditions   <u>Screening form</u>

<div align="right">Reviewer: RM / SW</div>

Citation		**Retrieve?**
1.	Abbott, M., Enhancing outcomes in the treatment of generalised anxiety disorder. [References], in Innovations and advances in cognitive behaviour therapy, D.A. Einstein, Editor. 2007, Australian Academic Press: Bowen Hills, QLD, Australia. p. 157-170.	
2.	Ackroff, K., F. Lucas, and A. Sclafani, Flavor preference conditioning as a function of fat source. Physiology & Behavior, 2005. 85(4): p. 448-460.	
3.	Adelman, E.M., Mind-body intelligence: a new perspective integrating Eastern and Western healing traditions. Holist Nurs Pract, 2006. 20(3): p. 147-51.	
4.	Alexander, C.N., et al., Transcendental Meditation, mindfulness, and longevity: An experimental study with the elderly. Journal of Personality and Social Psychology, 1989. 57(6): p. 950-964.	
5.	Alexander, J.L., et al., Collaborative depression care, screening, diagnosis and specificity of depression treatments in the primary care setting. Expert Rev Neurother, 2007. 7(11 Suppl): p. S59-80.	
6.	Alterman, A.I., et al., Pilot trial of effectiveness of mindfulness meditation for substance abuse patients. Journal of Substance Use, 2004. 9(6): p. 259-268.	
7.	Anderson, N.D., et al., Mindfulness-based stress reduction and attentional control. Clinical Psychology & Psychotherapy, 2007. 14(6): p. 449-463.	
8.	Arch, J.J. and M.G. Craske, Mechanisms of mindfulness: emotion regulation following a focused breathing induction. Behav Res Ther, 2006. 44(12): p. 1849-58.	
9.	Asberg, M., et al., [Mindfulness meditation--an old fashion method for stress relief]. Lakartidningen, 2006. 103(42): p. 3174-7.	
10.	Astin, J.A., Stress reduction through mindfulness meditation: Effects on psychological symptomatology, sense of control, and spiritual experiences. Psychotherapy and Psychosomatics, 1997. 66(2): p. 97-106.	
11.	Astin, J.A., et al., The efficacy of mindfulness meditation plus Qigong movement therapy in the treatment of fibromyalgia: a randomized controlled trial. J Rheumatol, 2003. 30(10): p. 2257-62.	
12.	…	

6.3.3 Inclusion form

<u>Mindfulness-based therapies for psychological conditions</u>   <u>Inclusion form</u>

Reviewer: RM   Date: <u>06/02/09 -</u>

Criteria:

Criterion 1 (language):   English or German *(OR contact author to determine whether English translation)*

Criterion 2 (design):   Any type of **experimental** design (including non-randomised)

Criterion 3 (participants):   Any **psychological** condition typically treated in MH services where a formal diagnosis explicitly stated using appropriate criteria (and a **clinical** sample)

Criterion 4 (sample size):   **2+** participants

Criterion 5 (intervention):   **Any type of mindfulness practice**

Criterion 6 (observation period):   **Treatment period 1 week or more** with follow-up at end of/ post treatment

Criterion 7 (outcome measure):   Any type of **validated** outcome

INCLUDE: **only if trial meets all criteria** *(place 'Y' or 'N' in boxes 1-7 and 'include' column)*

Lead author (yr)	Source	1	2	3	4	5	6	7	INCLUDE?
Abbott (2007)	Innovations and advances in cognitive behaviour therapy								
Alterman (2004)	J of Substance Use								
Bach (2002)	J of Consulting and Clinical Psychology								
Bohus (2004)	Behaviour Research and Therapy								
Bohus (2000)	Behaviour Research and Therapy								
Bowen (2006)	Psychology of Addictive Behaviors								
Bowen (2007)	Addictive Behaviors								
Bradley (2003)	J of Traumatic Stress								
Brassington (2006)	Australasian Psychiatry								
Carmody (2008)	J Psychosom Res								
Chadwick (2005)	Behavioural and Cognitive Psychotherapy								
Chen (2008)	International J of Eating Disorders								
Clarkin (2007)	American J of Psychiatry								
Comtois (2007)	Cognitive and Behavioral Practice								
Dalrymple (2007)	Behav Modif								
Dams (2007)	Psychotherapie Psychosomatik Medizinische Psychologie								
Eisendrath (2008)	Psychotherapy and Psychosomatics								
Evans (2008)	J of Anxiety Disorders								

6.4 Included studies

Table 30. Details of the studies included in the analyses

	Methods		Participants						Intervention			
Study	**Design[3]**	**Duration of therapy, follow-up**	**Diagnosis (criteria used)**	**Inclusion/ exclusion criteria**	**Total N (treatment, control) Completers (treatment, control)**	**Age Mean (SD)**	**Sex (M/F)**	**Ethnicity**	**Therapy type[4]**	**Number and duration of sessions**	**Number of therapists, training**	**Control[5]**
Dalrymple (2007)	1 group repeated measures	12 weeks, 3month follow-up	SAD (DSM-IV)	Excluded if diagnosis: primary not SAD; MR or a PDD; psychiatric disorder due to medical condition; substance dependence (<6 months); suicidal; medical contraindicating treatment; previous therapy for SAD.	19 enrolled, 16 completed post-treatment assessment, 12 follow-up	31 (10)	52.8%F	63.9% Caucasian	ACT, individual	12 weekly 1hr	Doctoral students, 3-hr protocol workshop, weekly supervision	N/A
Eisendrath (2008)	Pre-post single group	2 months	Active treatment resistant, major depression (DSM-IV)	Failure to remit with 2 previous at least 2 anti depressant treatments	55 enrolled, 51 completers	Range 22-74yrs	38F (74.5%)	82% white	MBCT Group	8 x 2 hour, weekly	Psychiatrist and co-therapist	N/A
Evans (2008)	Pre-post single group	8 weeks	GAD (DSM-IV)	18-80 yrs; medically Stable. Exclude if co-morbid depression; substance abuse/dependence; psychosis. Current or homicidal ideation and dissociative states.	12, 11	49 (Range 36-72yrs)	5 M/ 6 F	?	MBCT Group	8 weeks of 2 hours	1, completed internship in MBSR	N/A

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[3] Coded as either randomised or non-randomised; then study type (e.g. RCT; audit; one-group pre-test post-test)

[4] Coded as either ACT; DBT; MBCT; MBSR; MBT; MCT; MB-EAT; MBPM; MBRE; MBRP; or detailed if other

[5] Controls coded as either TAU (treatment as usual); WL (waiting list); AC (attention control); or detailed if active control or medication

	Methods			Participants					Intervention			
Study	Design[3]	Duration of therapy, follow-up	Diagnosis (criteria used)	Inclusion/ exclusion criteria	Total N *(treatment, control)* Completers *(treatment, control)*	Age Mean (SD)	Sex (M/F)	Ethnicity	Therapy type[4]	Number and duration of sessions	Number of therapists, training	Control[5]
Finucane (2006)	Pre-post single group	8 weeks Treatment. 3 months until post	Depression Or Depression and Anxiety (ICD-10)	Include if: 18–65yrs; H/O RDD or Depression and Anxiety; current symptoms>2wks; 2 previous episodes; BDI>14. Exclude if: organic brain disease. Current substance misuse; psychosis or mania; Personality Disorder; suicidal.	13	43 (Range 23-46yrs)	10 F	?	MBCT Group	8 weeks	One. Meditation experience, completed MBCT course, further training as MBCT instructor	N/A
Forman (2007)	RCT	Varied: Mean 15.27 sessions (CT), 15.60 (ACT)	Anxiety and Depression (DSM-IV)	Excluded if diagnosis of a serious psychiatric illness; or presented in crisis.	101 enrolled (56 ACT, 45 CT), 63 completers (37 ACT, 26 CT)	27.8 (7.25)	80.2% F	64.4% white, 12.9% black, 10.9% Asian, 3.0% Latino	ACT, individual	At least an hour weekly	23 therapy-naïve student therapists.	CT
Kabat-Zinn (1992)	1 group	5 months	GAD or PD w/ or w/out PD (DSM-III)	Excluded if other primary psychiatric diagnosis, psychosis, endocrine disorder, significant current alcohol or substance abuse.	22	38	17 F	?	MBSR Group	8 weekly 2 hour session + 7.5 hour "retreat"	1 per group	N/A
Kenny (2007)	Audit	8 weeks	Depression (DSM-IV)	Currently depressed with 3+ episodes depression or chronic >1 year at clinical interview, Not abusing substances.	50, 49 completers (46 supplied data)	43.3 (9.7)	74% F		MBCT	Eight 2-h classes were held + 1 hour yoga or related	First author (MK), psychiatrist, cognitive therapist	N/A
Kingston (2007)	Non-RCT	3 months	Recurrent MDD (DSM-IV)	>3 previous episodes, BDI 13-45. Exclude if substance dependence, psychosis, bipolar disorder	28 enrolled, 22 completed with 19 providing data (8, 11)	41.8 (range 20-62)	17 F	?	MBCT Group	8 weekly, 2 hr sessions	2	TAU

	Methods			**Participants**					**Intervention**			
Study	Design[3]	Duration of therapy, follow-up	Diagnosis (criteria used)	Inclusion/ exclusion criteria	Total N *(treatment, control)* Completers *(treatment, control)*	Age Mean (SD)	Sex (M/F)	Ethnicity	Therapy type[4]	Number and duration of sessions	Number of therapists, training	Control[5]
Koszycki (2007)	RCT	8 weeks	SAD (DSM-IV)	Exclude if HDRS>14; other axis 1 other than dysthymia, depression, panic disorder, agoraphobia, GAD, specific phobia and somatisation disorder; lifetime psychosis or bipolar; substance abuse <12 mnths.	58 enrolled, 53 completed (26 Treatment, 27 Control)	38.9 (15.7)	25M/ 28F	?	MBSR Group	8 2.5 hr sessions, one all day retreat (27.5 total)	1, with experience	CBGT
Kristeller (1999)	1 group pre-post	6 week	Binge Eating Disorder	BMI>27	18	46.5 (10.5)	All F	17 white	MB-EAT	7 sessions		N/A
Kuyken (2008)	RCT	15 months	Recurrent Depression (DSM-IV)	3+ episodes; 18+ doses ADM<6 mnths; current full/ partial remission. Exclude if substance dependence; organic brain damage; psychosis; Bipolar; Anti-Social/ Self-injurious behaviour; concurrent psychotherapy	123 enrolled, all analysed (61 treat., 62 ctrl)	48.95 (10.55)	47F	98% white		8 weekly 2 hr sessions; 4 follow up sessions over year.	2, trained by Teasdale, experienced	m-ADM
Ossman (2006)	Single-group pre-post	10 weeks	Social anxiety (DSM-IV)	Excluded if active substance dependence	22 enrolled, 12 completed	42.4	11, 11	Not stated	ACT, group	10 sessions, 2hours each	Two, graduate students (formal training, weekly supervision)	N/A

	Methods				Participants					Intervention			
Study	Design[3]	Duration of therapy, follow-up	Diagnosis (criteria used)	Inclusion/ exclusion criteria	Total N *(treatment, control)* Completers *(treatment, control)*	Age Mean (SD)	Sex (M/F)	Ethnicity	Therapy type[4]	Number and duration of sessions	Number of therapists, training	Control[5]
Ramel (2004)	2 group Pre-post	8 weeks	Lifetime diagnostic criteria for a mood disorder	Exclude if psychosis, substance dependence, substantial cognitive impairment	Matched sample of 11completers and 11	50.87 (8.87) T	35% F	100% white	MBSR group	8 weekly 2 hr classes + homework	psychiatric nurse specialist (PEC), who has participated in a professional training program under the direction of Kabat-Zinn, + a doctoral student	WL Data not included as added on afterwards.
Roemer (2007)	Single-group pre-post	4 months treatment, 3 months FU	GAD (DSM-IV)	18+yrs. Exclude if suicidal intent; bipolar; substance dependence; psychosis.	16	36.44 (12.34)	9 F	15 White, 1 Latino	ABBT, individual	16 (4 90-minute, 12 60-minute)	Authors, doctoral students, one postdoctoral therapist.	N/A
Roemer (2008)	RCT	16-32 weeks	GAD (DSM-IV)	18+yrs. Exclude if suicidal intent; bipolar; substance dependence; psychosis.	31 all analysed (15, 16)	33.59 (11.74)	9M/ 22F	27 white, 2 Latino, 1 black, 1 Asian	ABBT, individual	16 (4 90-minute, 12 60-minute)	Six doctoral students	WL
Rungreangkulkij (2008)	Single group pre-post	1 month treatment, 2 month FU	'Anxiety disorder'	Not stated (inpatients)	21	42.1 (range 22-56)	4, 17	Thai	BCP, individual	2 60-90 min sessions, 1 month apart (4 sessions)	One, Buddhist belief system	N/A
Wells (2006)	Open trial (audit)	Varied, 12-month FU	GAD (DSM-IV)	None stated	10	Range 25-76yrs	4M/ 6F	Not stated	MCT, individual	3-12 sessions	Not stated	N/A
Williams (2008)	RCT	8 weeks	Unipolar or Bipolar disorder, in remission	Aged 18-65, ≥1 episode major depression with suicide ideation. No manic episodes <6 mnths	68 (33, 35) 55 (28, 27)	Not given (no difference reported)	Not given (no difference reported)	Not given	MBCT group	8, 2hr sessions. 1 full-day	Two with recognised expertise	WL

	Methods				Participants					Intervention			
Study	Design[3]	Duration of therapy, follow-up	Diagnosis (criteria used)	Inclusion/ exclusion criteria	Total N *(treatment, control)* Completers *(treatment, control)*	Age Mean (SD)	Sex (M/F)	Ethnicity	Therapy type[4]	Number and duration of sessions	Number of therapists, training	Control[5]
Woods (2006)	RCT	12 weeks, 3 months	Trichotillomania (DSM-IV)	>85 on WASI; no physical conditions preventing participation; no mental health conditions requiring immediate attention; no psychotherapy for TTM. Stable psychotropics (>4weeks)	28 enrolled (14/14), 25 completers (12/13)	35 (10.2)	3, 25	96.4% Caucasian, 3.6% African American	ACT and habit reversal training, individual	10 sessions, duration not stated	One, trained by originator of ACT (Hayes)	WL
Yook (2008)	Pre post design	8 weeks	GAD, or PD with or without agoraphobia (DSM-IV)	Stable symptoms>2 months, not helped by medication. Exclude: substance misuse; comorbid psychiatric; medical problems.	19	41.1 (6.3)	11/8	?	MBCT Group	8 sessions of 2hrs each (16 hours)	?	N/A
Zylowska (2008)	Pre-post	8 weeks	ADHD (DSM-IV)	15 yrs+. Exclude if: substance dependence <6 months; psychosis; Bipolar; MR; personality disorder; conduct disorder; suicidal/ self-injurious.	24 adults & 8 adolescents, completers: 18 adults, 7 adolescents	48.5 (10.9) for adults, 15.6 (1.1) for adoles.	12 M/ 20 F	Not given	MAP, Group	8 weekly 2.5 hrs	experienced mindfulness instructor	N/A

6.5 Excluded citations

Table 31. Ineligible citations

Citation	Reason excluded
Abbott, M., Enhancing outcomes in the treatment of generalised anxiety disorder. [References], in Innovations and advances in cognitive behaviour therapy, D.A. Einstein, Editor. 2007, Australian Academic Press: Bowen Hills, QLD, Australia. p. 157-170.	Data inappropriate
Alterman, A.I., et al., Pilot trial of effectiveness of mindfulness meditation for substance abuse patients. Journal of Substance Use, 2004. 9(6): p. 259-268.	Participants
Barnhofer, T., et al., Effects of meditation on frontal alpha -asymmetry in previously suicidal individuals. Neuroreport: For Rapid Communication of Neuroscience Research, 2007. 18(7): p. 709-712.	Outcomes
Bohus, M., et al., Effectiveness of inpatient dialectical behavioral therapy for borderline personality disorder: A controlled trial. Behaviour Research and Therapy, 2004. 42(5): p. 487-499.	Participants
Bohus, M., et al., Evaluation of inpatient Dialectical-Behavioral Therapy for Borderline Personality Disorder--A prospective study. Behaviour Research and Therapy, 2000. 38(9): p. 875-887.	Participants
Bowen, S., et al., Mindfulness meditation and substance use in an incarcerated population. Psychology of Addictive Behaviors, 2006. 20(3): p. 343-347.	Participants
Bowen, S., et al., The role of thought suppression in the relationship between mindfulness mediation and alcohol use. Addictive Behaviors, 2007. 32(10): p. 2324-2328.	Participants
Bradley, R.G. and D.R. Follingstad, Group Therapy for Incarcerated Women Who Experienced Interpersonal Violence: A Pilot Study. Journal of Traumatic Stress, 2003. 16(4): p. 337-340.	Intervention
Brassington, J. and R. Krawitz, Australasian dialectical behaviour therapy pilot outcome study: Effectiveness, utility and feasibility. Australasian Psychiatry, 2006. 14(3): p. 313-319.	Participants
Brotto, L.A., R. Basson, and M. Luria, A mindfulness-based group psychoeducational intervention targeting sexual arousal disorder in women. Journal of Sexual Medicine, 2008. 5(7): p. 1646-1659.	Participants
Carmody, J., et al., Mindfulness, spirituality, and health-related symptoms. J Psychosom Res, 2008. 64(4): p. 393-403.	Participants
Carson, J.W., et al., Mindfulness-based relationship enhancement. Behavior Therapy, 2004. 35(3): p. 471-494.	Participants
Carson, J.W., et al., Self-expansion as a mediator of relationship improvements in a mindfulness intervention. Journal of Marital & Family Therapy, 2007. 33(4): p. 517-528.	Participants
Chen, E.Y., et al., Dialectical behavior therapy for clients with binge-eating disorder or bulimia nervosa and borderline personality disorder. International Journal of Eating Disorders, 2008. 41(6): p. 505-512.	Participants
Clarkin, J.F., et al., Evaluating three treatments for borderline personality disorder: A multiwave study. American Journal of Psychiatry, 2007. 164(6): p. 922-928.	Participants
Comtois, K.A., et al., Effectiveness of dialectical behavior therapy in a community mental health center. Cognitive and Behavioral Practice, 2007. 14(4): p. 406-414.	Participants
Dams, A., et al., Skilltraining and the post-treatment efficacy of dialectic behavior therapy six month after discharge of the hospital. Psychotherapie Psychosomatik Medizinische Psychologie, 2007. 57(1): p. 19-24.	Participants
Evans, K., et al., Manual-assisted cognitive-behaviour therapy (MACT): A randomized controlled trial of a brief intervention with bibliotherapy in the treatment of recurrent deliberate self-harm. Psychological Medicine, 1999. 29(1): p. 19-25.	Intervention
Fassbinder, E., et al., Effectiveness of dialectical behavior therapy for patients with borderline personality disorder in the long-term course--A 30-month-follow-up after inpatient treatment. Psychotherapie Psychosomatik Medizinische Psychologie, 2007. 57(3-4): p. 161-169.	Participants
Fisher, P.L. and A. Wells, Metacognitive therapy for obsessive-compulsive disorder: A case series. Journal of Behavior Therapy and Experimental Psychiatry, 2008. 39(2): p. 117-132.	Study design

Citation	Reason excluded
Fleischhaker, C., et al., Suicidal, parasuicidal and self injurious behavior in patients with borderline symptoms. First data of a pilot study on dialectical-behavioral therapy for adolescents (DBT-A). Kindheit und Entwicklung, 2005. 14(2): p. 112-127.	Participants
Harned, M.S., et al., Treating co-occurring Axis I disorders in recurrently suicidal women with borderline personality disorder: A 2-year randomized trial of dialectical behavior therapy versus community treatment by experts. Journal of Consulting and Clinical Psychology, 2008. 76(6): p. 1068-1075.	Participants
Hoschel, K., Dialectic Behavioral Therapy for Borderline Personality Disorder in Standard Medical Care - the Saarbrucken Treatment program. Verhaltenstherapie, 2006. 16(1): p. 17-24.	Participants
Jain, S., et al., A Randomized Controlled Trial of Mindfulness Meditation Versus Relaxation Training: Effects on Distress, Positive States of Mind, Rumination, and Distraction. Annals of Behavioral Medicine, 2007. 33(1): p. 11-21.	Participants
Katz, L.Y., et al., Feasibility of dialectical behavior therapy for suicidal adolescent inpatients. Journal of the American Academy of Child & Adolescent Psychiatry, 2004. 43(3): p. 276-282.	Participants
Koons, C.R., et al., Efficacy of dialectical behavior therapy in women veterans with borderline personality disorder. Behavior Therapy, 2001. 32(2): p. 371-390.	Participants
Kroger, C., et al., Effectiveness of dialectical behaviour therapy for borderline personality disorder in an inpatient setting. Behaviour Research and Therapy, 2006. 44(8): p. 1211-1217.	Participants
Lanius, R.A. and I. Tuhan, Letters to the editor: Stage-Oriented Trauma Treatment Using Dialectical Behaviour Therapy. The Canadian Journal of Psychiatry / La Revue canadienne de psychiatrie, 2003. 48(2): p. 126-127.	Participants
Lappalainen, R., et al., The impact of CBT and ACT models using psychology trainee therapists: A preliminary controlled effectiveness trial. Behavior Modification, 2007. 31(4): p. 488-511.	Participants
Levitt, J.T. and M. Karekla, Integrating Acceptance and Mindfulness with Cognitive Behavioral Treatment for Panic Disorder. [References], in Acceptance and mindfulness-based approaches to anxiety: Conceptualization and treatment, S.M. Orsillo and L. Roemer, Editors. 2005, Springer Science + Business Media: New York, NY. p. 165-188.	Publication type
Levy, K.N., et al., Change in attachment patterns and reflective function in a randomized control trial of transference-focused psychotherapy for borderline personality disorder. Journal of Consulting and Clinical Psychology, 2006. 74(6): p. 1027-1040.	Participants
Linehan, M.M., et al., Two-Year Randomized Controlled Trial and Follow-up of Dialectical Behavior Therapy vs Therapy by Experts for Suicidal Behaviors and Borderline Personality Disorder. Archives of General Psychiatry, 2006. 63(7): p. 757-766.	Participants
Linehan, M.M., et al., Dialectal behavior therapy versus comprehensive validation therapy plus 12-step for the treatment of opioid dependent women meeting criteria for borderline personality disorder. Drug and Alcohol Dependence, 2002. 67(1): p. 13-26.	Participants
Linehan, M.M., H.L. Heard, and H.E. Armstrong, Naturalistic follow-up of a behavioral treatment for chronically parasuicidal borderline patients. Archives of General Psychiatry, 1993. 50(12): p. 971-974.	Participants
Linehan, M.M., et al., Dialectical behavior therapy for patients with borderline personality disorder and drug-dependence. The American Journal on Addictions, 1999. 8(4): p. 279-292.	Participants
Linehan, M.M., et al., Interpersonal outcome of cognitive behavioral treatment for chronically suicidal borderline patients. American Journal of Psychiatry, 1994. 151(12): p. 1771-1776.	Participants
Lynch, T.R., et al., Treatment of older adults with co-morbid personality disorder and depression: A dialectical behavior therapy approach. International Journal of Geriatric Psychiatry, 2007. 22(2): p. 131-143.	Participants
Martin-Asuero, A. and G.G. de la Banda, The advantages of being present: Developing mindfulness to reduce distress. International Journal of Clinical and Health Psychology, 2007. 7(2): p. 369-384.	Language
McQuillan, A., et al., Intensive Dialectical Behavior Therapy for Outpatients With Borderline Personality Disorder Who Are in Crisis. Psychiatric Services, 2005. 56(2): p. 193-197.	Participants

Citation	Reason excluded
Miller, A.L., et al., Analysis of behavioral skills utilized by suicidal adolescents receiving dialectical behavior therapy. Cognitive and Behavioral Practice, 2000. 7(2): p. 183-187.	Participants
Moritz, S., et al., A home study-based spirituality education program decreases emotional distress and increases quality of life--a randomized, controlled trial. Altern Ther Health Med, 2006. 12(6): p. 26-35.	Participants
Murrell, A.R., L.W. Coyne, and K.G. Wilson, ACT with Children, Adolescents, and Their Parents, in A practical guide to acceptance and commitment therapy, S.C. Hayes and K.D. Strosahl, Editors. 2005, Springer Science + Business Media: New York, NY. p. 249-273.	No study reported
Nee, C. and S. Farman, Female prisoners with borderline personality disorder: Some promising treatment developments. Criminal Behaviour and Mental Health, 2005. 15(1): p. 2-16.	Participants
Nyklicek, I. and K.F. Kuijpers, Effects of mindfulness-based stress reduction intervention on psychological well-being and quality of life: is increased mindfulness indeed the mechanism? Ann Behav Med, 2008. 35(3): p. 331-40.	Participants
Ostafin, B.D., et al., Intensive Mindfulness Training and the Reduction of Psychological Distress: A Preliminary Study. Cognitive and Behavioral Practice, 2006. 13(3): p. 191-197.	Diagnosis unclear
Prendergast, N. and J. McCausland, Dialectic behaviour therapy: A 12-month collaborative program in a local community setting. Behaviour Change, 2007. 24(1): p. 25-35.	Participants
Price, C.J., et al., Mindful awareness in body-oriented therapy for female veterans with post-traumatic stress disorder taking prescription analgesics for chronic pain: a feasibility study. Altern Ther Health Med, 2007. 13(6): p. 32-40.	Participants
Rathus, J.H. and A.L. Miller, Dialectical Behavior Therapy adapted for suicidal adolescents. Suicide and Life-Threatening Behavior, 2002. 32(2): p. 146-157.	Participants
Ree, M.J. and M.A. Craigie, Outcomes following mindfulness-based cognitive therapy in a heterogeneous sample of adult outpatients. Behaviour Change, 2007. 24(2): p. 70-86.	Diagnosis unclear
Rizvi, S.L. and M.M. Linehan, The treatment of maladaptive shame in borderline personality disorder: A pilot study of "opposite action.". Cognitive and Behavioral Practice, 2005. 12(4): p. 437-447.	Participants
Safer, D.L., C.F. Telch, and W. Agras, Dialectical behavior therapy for bulimia nervosa. American Journal of Psychiatry, 2001. 158(4): p. 632-634.	Diagnosis unclear
Salbach, H., et al., Dialectical behavior therapy for adolescents with anorexia and bulimia nervosa (DBT-AN/BN) -- a pilot study. Praxis der Kinderpsychologie und Kinderpsychiatrie, 2007. 56(2): p. 91-108.	Participants
Sambrook, S., N. Abba, and P. Chadwick, Evaluation of DBT emotional coping skills groups for people with parasuicidal behaviours. Behavioural and Cognitive Psychotherapy, 2007. 35(2): p. 241-244.	Participants
Semple, R.J., E.F. Reid, and L. Miller, Treating anxiety with mindfulness: An open trial of mindfulness training for anxious children. Journal of Cognitive Psychotherapy, 2005. 19(4): p. 379-392.	Participants
Simons, M., S. Schneider, and B. Herpertz-Dahlmann, Metacognitive Therapy versus Exposure and Response Prevention for Pediatric Obsessive-Compulsive Disorder. Psychotherapy and Psychosomatics, 2006. 75(4): p. 257-264.	Study design
Simpson, E.B., et al., Combined Dialectical Behavior Therapy and Fluoxetine in the Treatment of Borderline Personality Disorder. Journal of Clinical Psychiatry, 2004. 65(3): p. 379-385.	Participants
Solanto, M.V., et al., Development of a new psychosocial treatment for adult ADHD. Journal of Attention Disorders, 2008. 11(6): p. 728-736.	Intervention
Soler, J., et al., Double-Blind, Placebo-Controlled Study of Dialectical Behavior Therapy Plus Olanzapine for Borderline Personality Disorder. American Journal of Psychiatry, 2005. 162(6): p. 1221-1224.	Participants
Stanley, B., et al., Brief Dialectical Behavior Therapy (DBT-B) for suicidal behavior and non-suicidal self injury. Archives of Suicide Research, 2007. 11(4): p. 337-341.	Participants
Sunseri, P.A., Preliminary Outcomes on the Use of Dialectical Behavior Therapy to Reduce Hospitalization Among Adolescents in Residential Care. Residential Treatment for Children & Youth, 2004. 21(4): p. 59-76.	Participants

Citation	Reason excluded
Trupin, E.W., et al., Effectiveness of dialectical behaviour therapy program for incarcerated female juvenile offenders. Child and Adolescent Mental Health, 2002. 7(3): p. 121-127.	Participants
Turner, R.M., Naturalistic evaluation of dialectical behavior therapy-oriented treatment for borderline personality disorder. Cognitive and Behavioral Practice, 2000. 7(4): p. 413-419.	Participants
Twohig, M.P., S.C. Hayes, and A. Masuda, Increasing Willingness to Experience Obsessions: Acceptance and Commitment Therapy as a Treatment for Obsessive-Compulsive Disorder. Behavior Therapy, 2006. 37(1): p. 3-13.	Study design
Twohig, M.P., S.C. Hayes, and A. Masuda, A preliminary investigation of acceptance and commitment therapy as a treatment for chronic skin picking. Behav Res Ther, 2006. 44(10): p. 1513-22.	Study design
Twohig, M.P., D. Shoenberger, and S.C. Hayes, A preliminary investigation of acceptance and commitment therapy as a treatment for marijuana dependence in adults. J Appl Behav Anal, 2007. 40(4): p. 619-32.	Participants
van den Bosch, L.M., et al., Dialectical Behavior Therapy of borderline patients with and without substance use problems: Implementation and long-term effects. Addictive Behaviors, 2002. 27(6): p. 911-923.	Participants
Van Den Bosch, L., Efficacy of dialectical behaviour therapy in the treatment of female borderline patients with and without substance abuse problems: Result of a Dutch study. Tijdschrift voor Psychiatrie, 2005. 47(3): p. 127-137.	Participants
van den Bosch, L.M., et al., Sustained efficacy of dialectical behaviour therapy for borderline personality disorder. Behaviour Research and Therapy, 2005. 43(9): p. 1231-1241.	Language
Verheul, R., et al., Dialectical behaviour therapy for women with borderline personality disorder: 12-month, randomised clinical trial in The Netherlands. British Journal of Psychiatry, 2003. 182(2): p. 135-140.	Participants
Vieten, C. and J. Astin, Effects of a mindfulness-based intervention during pregnancy on prenatal stress and mood: Results of a pilot study. Archives of Women's Mental Health, 2008. 11(1): p. 67-74.	Participants
Wells, A. and S. Sembi, Metacognitive therapy for PTSD: a preliminary investigation of a new brief treatment. J Behav Ther Exp Psychiatry, 2004. 35(4): p. 307-18.	Study design
Williams, J., et al., Mindfulness-based cognitive therapy (MBCT) in bipolar disorder: Preliminary evaluation of immediate effects on between-episode functioning. Journal of Affective Disorders, 2008. 107(1-3): p. 275-279.	Outcomes
Wix, S., Dialectical behaviour therapy observed. British Journal of Forensic Practice, 2003. 5(2): p. 3-8.	Study design

6.6 Studies not included in the analysis

Table 32. Eligible studies not included in the analysis

Citation	Reason not included
Bach, P. and S.C. Hayes, The use of acceptance and commitment therapy to prevent the rehospitalization of psychotic patients: A randomized controlled trial. Journal of Consulting and Clinical Psychology, 2002. 70(5): p. 1129-1139.	Outcomes
Chadwick, P., K.N. Taylor, and N. Abba, Mindfulness Groups for People with Psychosis. Behavioural and Cognitive Psychotherapy, 2005. 33(3): p. 351-359.	Outcomes
Flessner, C.A., et al., Acceptance-enhanced behavior therapy (AEBT) for trichotillomania and chronic skin picking: exploring the effects of component sequencing. Behav Modif, 2008. 32(5): p. 579-94.	Outcomes
Goldstein, T.R., et al., Dialectical behavior therapy for adolescents with bipolar disorder: A 1-years open trial. Journal of the American Academy of Child & Adolescent Psychiatry, 2007. 46(7): p. 820-830.	Outcomes
Harley, R., et al., Adaptation of dialectical behavior therapy skills training group for treatment-resistant depression. Journal of Nervous and Mental Disease, 2008. 196(2): p. 136-143.	Outcomes
Heidenreich, T., et al., Letters to the Editor: Mindfulness-Based Cognitive Therapy for Persistent Insomnia: A Pilot Study. Psychotherapy and Psychosomatics, 2006. 75(3): p. 188-189.	Outcomes
Hesslinger, B., et al., Psychotherapy of attention deficit hyperactivity disorder in adults--a pilot study using a structured skills training program. Eur Arch Psychiatry Clin Neurosci, 2002. 252(4): p. 177-84.	Outcomes
Lynch, T.R., et al., Dialectical behavior therapy for depressed older adults: A randomized pilot study. American Journal of Geriatric Psychiatry, 2003. 11(1): p. 33-45.	Outcomes
Ma, S. and J.D. Teasdale, Mindfulness-Based Cognitive Therapy for Depression: Replication and Exploration of Differential Relapse Prevention Effects. Journal of Consulting and Clinical Psychology, 2004. 72(1): p. 31-40.	Outcomes
Michalak, J., et al., Mindfulness predicts relapse/recurrence in major depressive disorder after mindfulness-based cognitive therapy. J Nerv Ment Dis, 2008. 196(8): p. 630-3.	Outcomes
Miller, J.J., K. Fletcher, and J. Kabat-Zinn, Three-year follow-up and clinical implications of a mindfulness meditation-based stress reduction intervention in the treatment of anxiety disorders. General Hospital Psychiatry, 1995. 17(3): p. 192-200.	Follow-up data only
Nelson-Gray, R.O., et al., A modified DBT skills training program for oppositional defiant adolescents: Promising preliminary findings. Behaviour Research and Therapy, 2006. 44(12): p. 1811-1820.	Outcomes
Philipsen, A., et al., Structured group psychotherapy in adults with attention deficit hyperactivity disorder: Results of an open multicentre study. Journal of Nervous and Mental Disease, 2007. 195(12): p. 1013-1019.	Outcomes
Teasdale, J.D., et al., Prevention of relapse/recurrence in major depression by mindfulness-based cognitive therapy. Journal of Consulting and Clinical Psychology, 2000. 68(4): p. 615-623.	Outcomes
Teasdale, J.D., et al., Metacognitive awareness and prevention of relapse in depression: empirical evidence. J Consult Clin Psychol, 2002. 70(2): p. 275-87.	Outcomes
Telch, C.F., W. Agras, and M.M. Linehan, Dialectical behavior therapy for binge eating disorder. Journal of Consulting and Clinical Psychology, 2001. 69(6): p. 1061-1065.Twohig, M.P. and D.W. Woods, A Preliminary Investigation of Acceptance and Commitment Therapy and Habit Reversal as a Treatment for Trichotillomania. Behavior Therapy, 2004. 35(4): p. 803-820.	Outcomes
Twohig, M.P. and D.W. Woods, A Preliminary Investigation of Acceptance and Commitment Therapy and Habit Reversal as a Treatment for Trichotillomania. Behavior Therapy, 2004. 35(4): p. 803-820.	Outcomes
Weiss, M., J.W. Nordlie, and E.P. Siegel, Mindfulness-Based Stress Reduction as an Adjunct to Outpatient Psychotherapy. Psychotherapy and Psychosomatics, 2005. 74(2): p. 108-112.	Outcomes
Williams, J., et al., Mindfulness-based cognitive therapy reduces overgeneral autobiographical memory in formerly depressed patients. Journal of Abnormal Psychology, 2000. 109(1): p. 150-155.	Outcomes