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EMMA JUSTICE BSc (Hons), PG Cert

MINDFULNESS AND THE THERAPEUTIC ENCOUNTER

Section A: The relationship between mindfulness and empathy: a systematic review

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Section B: Does a brief mindfulness practice improve factors that are important to the  
therapeutic encounter in trainee therapists?

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## Summary of the MRP

### SECTION A

Section A systematically reviewed the literature on mindfulness and empathy, in an attempt to determine if the two variables are related, whether mindfulness can increase empathy, and what the underlying mechanisms of action might be. Thirty-three studies were found and critiqued. It was concluded that whilst mindfulness may help to reduce distress experienced in the face of another's distress, the cognitive and affective components of empathy may not change without additional compassion training. However, more well-controlled research is needed in order to be confident in these conclusions.

### SECTION B

Section B investigated the effect of a brief mindfulness practice on empathy, compassion for others, and emotion regulation, in trainee therapists. The study used an experimental design with an active control group. Between-group differences were not found for empathy and compassion for others, the mindfulness group was lower in post-test negative affect than the control group, but only for those with high baseline negative affect. Certain factors meant these findings needed to be interpreted cautiously. The study highlighted the complexities of measuring and controlling for mindfulness, which can be taken forward in future research.

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## Section A

The relationship between mindfulness and empathy: a systematic review

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### Abstract

Empathy is thought of as an important part of well-functioning relationships, and therapist empathy is a moderately strong predictor of therapy outcome (Elliott, Bohart, Watson & Greenberg, 2011). Mindfulness could theoretically increase empathy through a number of mechanisms, including stress reduction, or changes in emotion regulation. The literature was reviewed to examine the evidence base for the relationship between mindfulness and empathy. Systematic searching took place between November 2013 and March 2014, and thirty-three studies were found that matched inclusion criteria. Correlational studies found an association between measures of mindfulness and empathy, and qualitative studies supported theoretical accounts for how the relationship could work. Experimental findings were mixed, and whilst there was more evidence that mindfulness could reduce the practitioner's distress in the face of others' distress, there was less evidence that the cognitive and affective components of empathy were increased by mindfulness. However, there are methodological flaws which limit confidence in these conclusions. Suggestions for the next steps for research in this area are made.

### The relationship between mindfulness and empathy: a systematic review

Mindfulness has become popular in psychology, with a surge in research on the topic in recent years (Dimidjian & Linehan, 2003). Much of this research has focused on symptoms of distress, and less is known about the interpersonal benefits of mindfulness (Davis & Hayes, 2011). This review will attempt to contribute to this area, by analysing what current evidence says about the relationship between mindfulness and empathy. If mindfulness does influence empathy, this could make it a helpful technique for populations such as therapists, for whom empathy is an important part of their work.

### **Mindfulness**

This review will begin by looking at definitions of mindfulness, starting with outlining its origins, before looking at ways in which the term is currently used in the psychological literature. Mindfulness originated in Buddhist philosophy, and has been summarised as “to remember to pay attention to what is occurring in one’s immediate experience with care and discernment” (Shapiro & Carlson, 2009, p. 4). At the core of Buddhist philosophy is the idea that human suffering is created through holding onto an illusion of a permanent and separate ‘self’. It is thought that through mindfulness meditation one begins to more closely observe the self, which could be one step in letting go of this illusion and gaining an appreciation of the interconnectedness of all beings, which is thought to lead to the end of suffering or ‘enlightenment’ (Hanh, 2008).

Mindfulness has been secularised by authors such as Kabat-Zinn (Mindfulness Based Stress Reduction; MBSR; Kabat-Zinn, 1990) and Teasdale, Segal and Williams (Mindfulness Based Cognitive Therapy; MBCT; Teasdale, Segal & Williams, 1995). Approaches like these that involve secularised training in mindfulness are generally termed Mindfulness Based Interventions (MBIs). MBIs attempt to teach mindfulness through experiential exercises such as mindfulness of breathing, body scan, and yoga, and often involve additional

components, such as didactic teaching on topics such as stress or cognition. MBIs have been shown to reduce a number of symptoms, compared to a waiting-list control, with the largest effect sizes found for anxiety, depression and stress (Khoury et al., 2013). However, due to the multiple components of MBIs and the lack of active control groups, it is not known how much of the effect is due to mindfulness, and how much is due to non-mindfulness factors (such as teaching about cognitions) and non-specific factors (such as being part of a group). If mindfulness is effective, it is also not yet clear what the mechanisms of action might be. A number of theories have been proposed, for example, Holzel et al. (2011) suggest five key components: attention regulation, body awareness, emotional reappraisal, emotional exposure, and changes in perception of the self.

The complexity of the construct of mindfulness has led to difficulties in measuring it psychometrically. The earlier measures such as the Mindful Attention Awareness Scale (MAAS; Brown & Ryan, 2003) conceptualised mindfulness as a single factor, in this case, present-moment awareness. Later scales, such as the Kentucky Inventory of Mindfulness Skills (KIMS; Baer, Smith & Allen, 2004) included more factors. In an attempt to break down mindfulness empirically, the Five Facet Mindfulness Questionnaire (FFMQ; Baer, Smith, Hopkins, Krietemeyer, & Toney, 2006), was developed using factor analysis of five existing mindfulness measures, and identified the following facets: observing; describing; acting with awareness; non-judging of inner experience; and non-reactivity to inner experience. However, although factor analysis may produce a measure that is reliable, its validity can still be questioned (Grossman, 2011). In summary, mindfulness is a complex construct, the definition and measurement of which are debated, and the application of which needs further rigorous testing.

## **Empathy**

This review will now look in more detail at empathy, before examining the theoretical links with MBIs. Rogers (1959) was perhaps the most influential writer about empathy, defining it as, “to perceive the internal frame of reference of another person...as if one were the person, but without losing the ‘as if’ condition” (Rogers, 1959, p. 211). Empathy is theorised as having three distinct components: cognitive, affective and self-regulatory. The affective component involves experiencing the emotions of the other person on a fairly visceral level, by simulating the others’ emotional state in oneself (Elliott et al., 2011). The cognitive component in contrast is more of a perspective taking skill, where the thoughts and feelings of the other person are understood on a conceptual level (Elliott et al., 2011). It is thought that these components rely on different neuroanatomical structures, with affective empathy activating areas such as the limbic system, and cognitive empathy activating areas such as the prefrontal and temporal cortex (Elliott et al., 2011). The self-regulatory component of empathy involves the perceiver retaining a separate sense of self (Decety & Jackson, 2004), thus holding on to the ‘as if’ condition that Rogers (1959) highlighted.

These three components are reflected in one of the most widely used measures of empathy, the Interpersonal Reactivity Index (IRI; Davis, 1980), with the affective component measured by the Empathic Concern subscale, the cognitive component by the Perspective Taking subscale, and the self-regulatory component by the Personal Distress subscale. There is also the Fantasy subscale, which measures the tendency to take the perspectives of characters in books and plays. It is generally considered that lower scores on Personal Distress, and higher scores on Fantasy, Perspective Taking and Empathic Concern, relate to higher empathy. Although the IRI has adequate internal reliability, having been derived theoretically its validity can be questioned, and concerns have particularly been raised about

the validity of the Fantasy subscale (Baron-Cohen & Wheelwright, 2004), which authors often choose not to use.

In attempting to understand empathy, it can be helpful to distinguish it from similar constructs. Theory of Mind can be seen as sharing the cognitive component of empathy, but without the affective and self-regulatory components (Sharp & Venta, 2012). Mentalization similarly shares the cognitive component, but also includes the ability to understand one's own thoughts and feelings, whereas empathy does not (Choi-Kain & Gunderson, 2008). Emotional Intelligence also involves the cognitive component, but includes wider abilities such as social skills, interpersonal closeness, and cooperation (Chu, 2010). Sympathy has similar affective components as empathy, but involves a desire to change the other's emotion (Block-Lerner, Adair, Plumb, Rhatigan & Orsillo, 2007). Altruism refers to a specific act of helping another without expecting anything in return, for which empathy may be a pre-requisite (Kristeller & Johnson, 2005).

An additional construct which has some overlaps with empathy is compassion. Compassion is defined as an awareness of suffering and a desire to relieve that suffering (Neff, 2003), either towards oneself or towards others. Self-compassion is thought to include self-kindness, a sense of common humanity, and mindfulness (Neff, 2003). Compassion for others can be thought of as a combination of empathy and altruism, being an awareness of the suffering of others, combined with the desire to relieve this suffering (Kristeller & Johnson, 2005). It is currently debated as to how far self-compassion and compassion for others overlap (Neff & Pommier, 2013), however from the above definitions it can be seen that whilst empathy has some overlap with compassion, compassion contains an additional motivational component (Kristeller & Johnson, 2005). In summary empathy is, similarly to mindfulness, a multi-faceted construct with some overlaps with, and distinctions from,



neighbouring constructs. This review will now turn to ideas in the literature about how empathy and mindfulness could be linked.

### **Theoretical links**

There are a number of mechanisms through which MBI's could increase empathy.

**Stress and anxiety.** High stress may reduce the ability to respond empathically, perhaps due to an instinct to conserve personal resources by not taking on the distress of others (Burks & Cobus, 2012). MBIs can be helpful in reducing stress (Khoury et al., 2013) which in theory could increase the resources available to respond empathically (Snyder, Shapiro & Treleaven, 2012). In addition, MBIs can help reduce rumination and anxiety (Khoury et al., 2013), which could increase the attentional capacity to pay attention to another's emotions (Shapiro & Carlson, 2009).

**Re-perceiving.** MBIs teach the practitioner to view their thoughts and emotions from an observer's perspective, an ability that is termed 'meta-cognitive awareness', or 're-perceiving' (Shapiro, Carlson, Astin & Freedman, 2006). It is thought that re-perceiving enables people to move away from 'automatic pilot' and opens up choices about how to act, reconnecting people with their beliefs and values (Shapiro et al. 2006). This could be helpful for busy populations such as healthcare workers, by giving them the space to reconnect with values around helping others. It could also enhance the self-regulatory component of empathy, by helping the practitioner to tease apart which emotions are theirs and which are not.

**Emotional exposure.** MBIs may be a form of emotional exposure, an idea based on behaviour theory which states that anxiety around, and avoidance of, unpleasant stimuli can be reduced, as exposure to the stimuli increases. Mindfulness may teach the practitioner to move towards difficult feelings rather than away from them, thus reducing the potency of

negative emotions (Holzel et al., 2011). This could increase willingness to approach others' negative emotions with openness (Bruce, Manber, Shapiro & Constantino, 2007).

**Changes in perception of the self.** According to Shpairo et al.'s (2006) theory, re-perceiving could lead to changes in the perception of the self, as an awareness grows that one's present experience does not represent a stable self, but rather a flow of changing thoughts and feelings. Letting go of attachments to the self could lead to 'interconnectedness', a sense that all beings are interdependent, through which compassion and empathy could open up (Hanh, 2008).

In summary, MBIs could increase empathy through increasing available internal resources, and making changes in the way that thoughts, emotions, and the self, are viewed. Empathy is thought to be an important part of well-functioning relationships (Wachs & Cordova, 2007), and there is evidence that therapist empathy has an effect on therapy outcome (Elliott et al., 2011). If MBIs are able to increase empathy as theorised, this could have a number of implications for interventions, not least for therapists wishing to enhance the working alliance. A systematic review of the empirical literature is therefore needed, in order to answer the following questions: What is the relationship between mindfulness and empathy? Can an MBI lead to improvements in empathy? If so, can the evidence increase our understanding of what the mechanisms of action might be?

### **Method**

Searching took place between November 2013 and March 2014 and covered all records in the database up until 31<sup>st</sup> March 2014. The search terms 'mindful\* OR meditation OR mbsr OR mbct AND empathy OR theory of mind' were entered into: PsychInfo, Science Direct, Cochrane, Web of Science and Medline, searching abstracts, titles and keywords. Limits were set for the article being in the English language and published in a peer-reviewed journal; no date limits were set. Reference lists of relevant articles were hand-searched.

Articles were included if they provided research evidence involving mindfulness and empathy. Articles were excluded if the meditation practice differed from the definition of mindfulness above; for example, if there was a large spiritual component, the meditation was purely concentrative, or was predominantly compassion-focussed (loving-kindness meditation, LKM). Articles were excluded if the focus was on a different but connected construct to empathy, for example, mentalization. See Appendix 1 for flow diagram of the search process (based on PRISMA; Moher, Liberati, Tetzlaff & Altman, 2009). Articles were critiqued using CONSORT criteria (Schulz, Altman & Moher, 2010) and the Clinical Appraisal Skills Programme (CASP) guidelines (CASP, 2013).

### **Results**

Thirty-three articles met the inclusion criteria: Six were correlational studies, nine were pre-post studies (a tenth article provided follow-up qualitative data), five were controlled studies that were not randomised, five were randomised controlled trials (RCTs), and seven were qualitative studies. See Appendix 2 for a summary table. Firstly correlational studies will be reviewed, followed by an examination of the experimental literature, leading onto a discussion of the qualitative studies.

#### **Correlational studies**

Six correlational studies were found. Table 1 outlines the results for each study. Table 1 shows that there was diversity in the way the results were reported, with some authors choosing to combine various IRI subscales, and one author reporting correlations with separate subscales of the mindfulness measure (Dekeyser, Raes, Leijssen, Leysen & Dewulf, 2008). In the studies that reported it, there was a strong relationship between the mindfulness measure and the Perspective Taking subscale of the IRI, meaning that as mindfulness increased cognitive empathy increased. There was generally a strong negative relationship between mindfulness and the Personal Distress subscale, so as mindfulness increased the

tendency to feel overwhelmed by another's distress decreased. There was a weaker but generally still significant relationship between mindfulness and Empathic Concern (a measure of affective empathy), although one study (Thomas & Otis, 2010) did not find this. These findings are consistent with theories about mindfulness reducing the potency of negative emotions through exposure (Holzel et al., 2011), so people who are more mindful may be more able to approach negative emotions, potentially making it easier for them to understand another's emotions, whilst at the same time feeling less overwhelmed by them.

**Critique.** The inconsistencies in reporting results make it hard to compare across studies, particularly as the correlation of the mindfulness measure with the individual IRI subscales is not reported by all papers. It has been questioned whether the IRI subscales can be combined (Thomas & Otis, 2010) as the measure was originally designed to pick up on four distinct components of empathy (Davis, 1983). Splitting the mindfulness measure into subscales (as in Dekeyser et al., 2008) again makes it difficult to synthesise across studies, and means multiple tests were conducted, leading to the risk of Type I errors.

More generally, causation cannot be established from correlational studies, and there could be a number of explanations for why an increase on mindfulness measures might be

Table 1.

Pearson correlations for the relationship between Interpersonal Reactivity Index subscales and mindfulness measures

Study	Sample (N)	Mindfulness measure	Correlation with					
			Perspective Taking	Empathic Concern	Personal Distress	Fantasy	EC + PT	EC + PT + F
Keane (2013)	Psychotherapists (40)	FFMQ	.68**	.336*	-.50**	-.004		
Thomas & Otis (2010)	Social workers (171)	FFMQ	.36***	.004	-.47***	-.227*		
Greason & Cashwell (2009)	Counselling students (179)	FFMQ					.27**	
Beitel, Ferrer & Cecero (2005)	Undergraduates (103)	MAAS	.41**	.280*	-.49**			
Wachs & Cordova (2007) <sup>1</sup>	Married couples (29 couples)	MAAS	.49**	.380*	-.35*			
Dekeyser et al. (2008) <sup>2</sup>	Undergraduates (101)/ Parents (246)	KIMS (Observe)			-.04 /			.33** /
		KIMS (Describe)			-.10			.31***
		KIMS (Act with awareness)			-.33**/			.32** /
		KIMS (Accept without judgement)			-.34***			.05
					-.28**/			.41 /
					-.25***		.01	
					-.37***/		-.15 /	
					-.40***		-.07	

Note. <sup>1</sup>, all measures were taken as a mean across the married couple. <sup>2</sup>, data are presented in the following format: Undergraduate sample/ Parent sample. \*, p < .05. \*\*, p < .01. \*\*\*, p < .001. FFMQ, Five Facets Mindfulness Questionnaire. MAAS, Mindful Attention Awareness Scale. KIMS, Kentucky Inventory of Mindfulness Skills. EC+PT, Empathic Concern and Perspective Taking were summed. EC+PT+F, Empathic Concern, Perspective Taking and Fantasy were summed.

related to an increase on empathy measures, not least that the two may be picking up on a similar underlying construct. This brings into light questions around validity of self-report measures. In addition, these studies have generally focused on health-care workers and undergraduates, so findings may not be generalisable outside these groups. In summary, the correlational studies suggest a possible connection between mindfulness and empathy in some groups, however experimental research is needed to understand this relationship further.

### **Pre-post studies**

Nine studies used a pre-post design, with a tenth report providing additional qualitative data. The studies will be outlined, and critique relevant to individual studies will be discussed, before examining overall critiques and drawing conclusions.

Four studies will be discussed together, as all did not find a significant change in empathy, but all are affected by small sample sizes, increasing the risk of a Type II error. Beddoe and Murphy (2004) ran an MBSR course with 16 nursing students and found no significant differences between pre and post-therapy IRI scores for all subscales. Trends are reported as being in the expected directions, apart from Fantasy, which trended downwards (descriptive statistics were not available in the paper). Themes from participants' journals were examined; the article does not report any themes to have been around empathy. Bond et al. (2013) examined the effects of an 11 week Embodied Health module for medical students (N = 24). Students practiced meditation and yoga and were taught about the neuroscience of meditation. No significant differences were found pre- to post-test on the Jefferson Scale of Physician Empathy (JSPE; Hojat et al., 2001), and a small effect size was found. A content analysis of participants' experiences did not reveal themes or subthemes involving empathy. Mindfulness was not measured, so it is difficult to know if the intervention had the intended effect. Rimes and Wingrove (2011) administered an MBCT course to 20 trainee clinical

psychologists, adapting the course to move the focus from depression to stress. There were no significant differences from pre to post MBCT on the IRI (only the Empathic Concern subscale was used). In a content analysis of participant's experiences, 60% reported having "an increased understanding of what it is like to be a client" (Rimes & Wingrove, 2011, p 238). Hopkins and Proeve (2013) ran an MBCT course with 11 trainee clinical psychologists. Measures were taken at pre, post and two month follow up. The only significant change in empathy was a decrease on the Fantasy subscale, which is an effect in the unexpected direction. A theme of the interviews was that MBCT contributed to an "altered therapy experience" (Hopkins & Proeve, 2013, p. 10), in which participants talked about improvements to the therapeutic relationship, but empathy was not specifically mentioned in the summary presented. In summary, four studies failed to find an effect of an MBI on empathy using samples of healthcare workers. In all but one paper, descriptive statistics were not available to examine effect sizes, so it is not clear whether this lack of effect was due to studies being underpowered.

Galantino, Baime, Maguire, Szapary and Farrar (2005) used a larger sample and similarly did not find a significant result. MBSR was adapted for health care professionals in order to address burnout, compassion and communication, no further details are given of how these adaptations were operationalised. Sixty-nine health-care professionals completed pre and post measures, with no significant differences found on IRI subscales. A measure of mindfulness was not used, so it is difficult to say whether the adapted course had the effect of increasing mindfulness.

Two studies found a significant increase at post-test, but had made considerable adaptations to the MBI to include components that could be expected to enhance empathy in their own right. Harwani et al. (2013) gave an 11-week mind-body medicine course to 118 medical students. The course involved mindfulness meditation, autogenic training, guided

imagery, movement and writing exercises. Following the course a significant decrease was found on the Personal Distress subscale, and a significant increase on the Empathic Concern subscale. This paper is a very brief report, so there are few details about the intervention and the statistical tests. Krasner et al. (2009) recruited 70 physicians and expanded MBSR to include: didactic material around managing conflict, preventing burnout and managing boundaries; and a Narrative and Appreciative Enquiry Approach. Empathy as measured by the JSPE improved at post-test and was maintained at 15 month follow up. Qualitative data for the sample was examined in a separate paper (Beckman et al., 2012), revealing a theme of “acquiring skills of attentiveness, listening, honesty and presence” (Beckman et al., 2012, p. 2), where participants talked about how being present and listening increased empathy in their interactions with patients.

Two studies found a significant increase in empathy, with more minor adaptations to traditional MBSR. Birnie, Speca and Carlson (2010) used a community sample of 51 participants, and added a LKM component in the final two weeks. A significant increase was found in Perspective Taking and a significant decrease in Personal Distress after the course; no changes were found in Empathic Concern. Bazarko, Cate, Azocar and Kreitzer (2013) delivered traditional MBSR, but in a group telephone conference format (apart from two day-long retreats, which occurred in person). Thirty-six nurses took part. A significant difference was found at post-test on the JSPE, and this was maintained at four month follow-up.

**Overall critique.** All studies in this section did not use a control group, therefore, any significant changes cannot be definitely due to the intervention, and could have occurred naturally over time. All samples were trainee or qualified health-care workers, meaning findings may not be generalisable outside of these populations. In addition, all studies used participants who volunteered to take part in an MBI, and there may be an effect of self-selection on the outcomes. Participants are likely to have been aware of the hypotheses, so



demand characteristics could be at play. In addition, it is not certain that the IRI and the JSPE are measuring the same constructs, so comparison between studies using the two measures is difficult.

**Summary and conclusions.** Five studies did not find the expected effects on empathy from before to after an MBI. Four of these studies had low sample sizes, so results may reflect Type II errors, however in the one study where effect size was available it was small. Galantino et al.'s (2005) study, which benefited from a larger sample, also did not find an effect, however mindfulness was not measured making it hard to know if the intervention was effective. Five studies added a qualitative component, in only one (Beckman et al., 2012) was empathy explicitly mentioned, although in two (Rimes & Wingrove, 2011; Hopkins & Proeve, 2013) there was a suggestion of general improvements in the therapeutic relationship. Four studies found a significant change on some of the empathy subscales following a MBI. Three of these made adjustments to MBSR, raising the question of whether additional empathy or compassion focussed components are needed in order for an MBI to increase empathy. It is possible that whilst an MBI may allow a practitioner to change their relationship with their own thoughts and emotions, a compassion meditation may need to be added for the practitioner to take the next step of connecting with another's thoughts and emotions (Kristeller & Johnson, 2005). In contrast, Bazarko et al. (2013) did find an effect of MBSR on empathy, without a compassion component to the intervention. In summary, the pre-post studies have methodological issues, particularly a lack of control group and small sample sizes, but they raise the question of whether an MBI on its own can lead to an increase in empathy.

### **Controlled studies**

A total of ten studies used a control group: two examined differences between existing groups of meditators and non-meditators, three were controlled trials without

randomisation, and five were RCTs. Firstly, studies using self-report measures of empathy will be examined, followed by studies that used performance-based measures of empathy. Methodological issues relating to specific papers will be highlighted, then, overall methodological issues from examining the CONSORT guidelines (see Appendix 3) will be discussed. Finally the findings will be summarised and conclusions drawn.

**Self-report empathy measures.** In a non-randomized trial, Barbosa et al. (2013) offered MBSR to graduate healthcare students ( $n = 13$ ) with a matched group paid to act as an inactive control ( $n = 15$ ). Significant differences were found between groups on the JSPE at post-test, however this difference was not maintained at three week follow-up, by which point both groups' empathy had gone down compared to pre-test, which is suggested as being due to the examination period starting at four weeks post-test. Power in this study could have been affected by low participant numbers. In a larger study using randomization, Shapiro, Schwartz and Bonner (1998) looked at the effects of MBSR in 73 medical students, also in a period of stress just before exams. There was a wait-list control group, and MBSR was adapted to include LKM and experiential exercises designed to cultivate mindful listening and empathy. Empathy was measured using the Empathy Construct Rating Scale (Monica, 1981) which was shortened for the study. The results indicated significantly greater increases in empathy for the intervention group post-intervention. Methodological issues include the measure of empathy, which was adapted and therefore non-validated, and the adaptations made to the MBSR programme, which would likely have increased empathy aside from the mindfulness training.

In a non-randomized quasi-experimental study, O'Connor, Berry, Stiver and Rangan (2012) compared 98 Tibetan Buddhist meditators to a group of 438 non-Buddhist adults recruited from the community. Independent samples t-tests showed a significant difference between the groups on Personal Distress, with meditators having lower Personal Distress than

non-meditators. The Perspective Taking and Empathic Concern subscales were not significantly different. A limitation with this study is that by comparing groups who are already meditating to those who are not, additional confounds are at play, such as having been drawn to meditation practice, and having a Buddhist belief system. Sahdra et al. (2011) drew both their control and intervention groups from a population of experienced meditators, with one group acting as a waiting-list control, whilst the other attended a three month retreat (N = 59), again without randomisation. Combining three of the IRI subscales, a significant difference was found between the groups at post-test, and the control group also increased from baseline when they went through the process. However, the retreat had multiple components other than mindfulness meditation, and the sample was a group of experienced meditators, so the findings cannot be generalised to those with less experience or those who have not been drawn to meditation.

Using a similar outcome measure, Shapiro, Brown, Thoresen and Plante (2011) present data from an RCT comparing the effects of MBSR (n =15) and a wait-list control group (n =15) in an undergraduate sample. The intervention group showed greater increase in global IRI than the control group at two and 12 month follow-up (immediate pre-test data were not presented). The study has limitations around sample size and multiple statistical tests, which the authors acknowledged. Both the above studies are limited by the use of the IRI as a global measure.

**Performance-based empathy measures.** Performance-based empathy measures attempt to move away from some of the problems of validity associated with self-report measures, which may be subject to bias, and rely on participants having insight into their own levels of empathy. The Affective Sensitivity Scale (Kagan & Schneider, 1987) is a measure used by three older studies (Pearl & Carlozzi, 1994; Keefe, 1979; Lesh, 1970). Participants view 33 scenes of clients interacting with health care professionals and select one of three

options that best represents the client's feelings. Other performance-based measures of empathy include the Reading the Mind in the Eyes Task (RMET; Baron-Cohen, Jolliffe, Mortimore & Robertson, 1997), the Emotion Recognition Task (Montagne, Kessels, DeHaan & Perrett, 2007), and the Micro Expression Training Tool (METT; Ekman, 2002), which all require participants to identify subtle emotional states from photographs of human faces.

Lesh (1970) used a Zen meditation for 30 min per day for four weeks with three groups taken from a cohort of counselling psychology students: the meditation group (n = 16), the control group (n = 12) who volunteered to do the meditation but received no intervention, and a second control group (n = 11) who did not want to do the meditation but completed pre and post measures. The meditation group performed significantly better than the two control groups on the Affective Sensitivity Scale. However, participants were not randomised to condition, which the author acknowledged could have led to bias. Two studies used the same measure but with randomization, and did not find the above difference. Pearl and Carlozzi (1994) randomized participants to a meditation group (n = 24), practicing Clinically Standard Meditation (Carrington, 1979) for eight weeks, or a no-treatment control group (n = 26) and found no significant differences. This is a very brief report, so does not go into enough detail of the methodology and statistics to satisfy many of the CONSORT criteria. Keefe (1979) randomized social work students to a no treatment control (n = 17), an educational group on communication skills (n = 20), and a Zen meditation group (n = 19) who meditated for 30 min daily for three weeks. No significant difference was found, however this study had low participant numbers in each group. With all three of the above studies it was not possible to use validated measures of mindfulness, due to these being developed only in recent years, meaning it is unclear what effect the meditation practice was having.

Melloni et al. (2013) accessed a group of 'long-term meditators' (MBSR completers who had practiced for one further year), 'short-term meditators' (who had just completed MBSR) and a control group (who were on the waiting list for MBSR). Although both groups of meditators scored lower than controls on the Personal Distress subscale of the IRI, there were no between group differences on either of the performance-based measures of empathy (Emotion Recognition Task and RMET). Participants were not randomized to condition, and there was a very low sample size ( $N = 29$ ). In an RCT with a larger sample, Kemeny et al. (2011) examined the effect of an eight week meditation and emotion recognition training programme on a group of school teachers randomly assigned to the intervention ( $n = 38$ ) or a wait-list control ( $n = 38$ ). The intervention group significantly increased their abilities on the METT compared to the control group. This study meets most of the CONSORT criteria and has adequate power, and as such provides some of the most robust evidence discussed here. However an issue is the addition of emotion recognition training into the MBI which is likely to have increased empathy in its own right.

**Overall methodological issues.** All studies not using randomisation face the problem of there potentially being systematic differences between the two groups at baseline, particularly as certain characteristics may draw one to being interested in trying an MBI. RCTs go some way to overcoming this problem, but one issue across all the RCTs is the lack of active control groups. This means that non-mindfulness effects and non-specific effects were not controlled for. Added to this is the diverse array of MBIs used, with some studies adding compassion or emotion recognition training, making it impossible to say that it was mindfulness that was having the effects seen. Most studies (with the exception of Kemeny et al., 2011) do not specify how sample size was determined, which is concerning given that sample sizes are generally low, so statistical power may also be low. Although all papers specified how many participants were lost before completing the intervention, only two

(Kemeny et al., 2011; Shapiro et al., 2011) performed an intent-to-treat analysis, and not using an intent-to-treat analysis can leave the results flawed (Schulz et al., 2010). Finally, although performance-based measures of empathy are able to overcome some of the limits of self-report measures, their construct validity can still be questioned.

**Summary and conclusions.** Studies in this section have generally found that MBIs have led to increases on self-report measures of empathy compared to a wait-list control, however, results on performance-based measures are mixed, with only two out of five studies finding a significant difference. This may suggest that after an MBI participants see themselves in a more empathic light, perhaps due to having more awareness of concepts such as compassion; however it is unclear whether this actually translates into more empathic behaviour in practice. It is possible therefore that having meta-cognitive awareness is not sufficient to change behavioural aspects of empathy, which may be more unconscious. These conclusions should be viewed in the context of the methodological limitations discussed above.

### **Qualitative studies**

Seven qualitative studies were found, additionally a study discussed above (Keane, 2013) with a substantial qualitative component will be discussed here. Studies will be divided into those that explored experiences of MBIs, and those that explored experiences of integrating mindfulness into interpersonal work (see Appendix 4 for CASP table).

**Experiences of MBIs.** Cohen-Katz et al. (2005) explored the impact of MBSR on a group of 25 nurses. A thematic analysis from multiple sources (including questionnaires, a focus group and feedback emails) identified six themes; one was “impact of MBSR on relationships”, which included the subtheme of “increased empathy/ appreciation of others” (Cohen-Katz et al., 2005, p. 82). This theme was in the context of interpersonal benefits such as better communication, being less reactive in relationships, and increased confidence. This

paper gives detailed information on how data were collected and analysed, however more reflection on how the authors own position could have affected the data would be useful, along with more exploration of contradictory findings and alternative explanations.

A modified MBSR course, which used Qi Gong instead of a body scan, and with additional readings in psychology and psychiatry, was available as a course module to counselling students (Schure, Christopher & Schure, 2008). At the end of the course (15 weeks), students were asked to write journal responses to four questions. A content analysis revealed increases in empathy were “mentioned frequently” (Schure et al., 2008, p. 51). The quotations given illustrate possible mechanisms through which this may have occurred, such as having more awareness of negative judgements, and reduced anxiety in session. One critique of this paper is that there is no mention of contradictory findings, and if there were none, this could suggest students were biased by knowing their journals would be read by the course examiners.

Gokhan, Meehan and Peters (2010) offered a 12 week MBI to psychology undergraduates on clinical placements. Self-report mindfulness measures were used, which showed an increase in mindfulness in the intervention group compared to a group attending a different course that did not have the mindfulness component. Themes were taken from student’s journals along with semi-structured interviews. Under the theme of ‘compassion’ students talked about how they had increased empathy for clients, staff and themselves. The authors suggest this could be due to student’s increased awareness of their personal reactions, or increased awareness of the importance of ethics. This study attracts similar critique to Schure et al. (2008), as the researchers’ position is not considered, and contradictory findings are not discussed, an issue that becomes more problematic when students knew that their journals would be read by their tutors.

A technique called Mindfulness-Based Role-Play (MBRP) was evaluated in a study with 12 health care professionals (Andersson, King & Lalande, 2010). MBRP is a supervisory technique in which the supervisee plays out an interaction with a client, switching between chairs to take on the role of both client and therapist. When in the client role, the supervisee is asked to notice in a mindful way bodily sensations, feelings and thoughts. Participants attended an introductory module on MBRP, which included mindfulness exercises and didactic teaching, and then had one MBRP supervision session. Analysis of the themes in semi-structured interviews revealed that participants felt they had increased awareness of what was going on for the client, and increased awareness of themselves as therapists. “Almost every participant” (Anderson et al., 2010, p. 291) reported an increase in empathy for the client. The authors reflect on the impact of their position as supervisor, and attempt to increase the reliability of their findings through triangulation. One limitation of the study is that the two-chair technique is likely to increase empathy in its own right.

Bailie, Kuyken and Sonneberg (2011) looked at the long-term effects of MBCT on parents’ (who had a diagnosis of recurrent depression) relationships with their children. Sixteen semi-structured interviews were collected a year after MBCT, and analysed thematically. Empathy and acceptance was one of the primary themes, where parents described how because they could now see a number of reasons for their child’s behaviour, and could identify that ‘thoughts were not facts’, they were more empathic towards their children. For some parents this led to reduced irritability, or increased time spent with their children, leading to further bonding. This is a reflective article which incorporates necessary details about the analysis and includes contradictory perspectives. It is also useful in that it looks at the longer-term relational impact of MBCT.



Bermudez et al. (2013) carried out semi-structured interviews over 15 months after MBSR, with women who had been victims of intimate partner violence. Under the theme of ‘interpersonal improvements’ participants talked about how their relationships were characterised now more by mutual understanding and empathy. This was in the context of communicating more assertively, improvements in self-worth, and feeling that they had started to face their past traumas. This article lacked reflection from the authors on their own position and a consideration of contradictory findings. All MBSR groups add the additional confound of group effects, but this may be particularly important in this sample, where the participants emphasised the importance of talking with women with similar experiences to themselves in processing past traumas. This makes it hard to know what the role of mindfulness was in producing the outcomes seen.

**Integrating mindfulness into interpersonal work.** At an end of life care hospice, which explicitly integrated mindfulness into its approach, Bruce and Davies (2005) looked at how those who regularly practiced mindfulness used their practice, in caring for those who were dying, or in facing their own death. The authors integrated themselves into the hospice ethos, by attending mindfulness retreats and volunteering. They interviewed nine participants, seven of whom were caregivers and two of whom were living with HIV/AIDS. Participants had at least six months of regular meditation practice, and followed Buddhist traditions. Four themes occurred; under the theme of ‘abiding in liminal spaces’, participants talked about how mindfulness helped them to break down barriers between ‘self’ and ‘other’, leading to a feeling of interconnectedness and empathy. One participant described how this helped her to feel nurtured whilst caring for someone. Becoming immersed with the hospice is an interesting approach to recruitment, however, this could have been enhanced with some reflection on how the author’s position influenced the course of the study. There is detail

lacking as to how the interviews were conducted, how the data were analysed, and a lack of direct data from the interviews.

In Keane (2013), participants (40 psychotherapists) responded to a postal survey asking about the impact of mindfulness on their clinical work. In response to Likert-scale questions, 80% of participants said that mindfulness had had an impact on empathy. Twelve participants were selected for follow-up interviews. Of interest to this review, participants talked about having heightened attention in their client work, which led them to be more empathic, as they were more able to listen closely, and be aware of their own reactions. Participants also talked about the challenging side of this; that especially at the start of their mindfulness practice, they felt an increased sensitivity to the pain in client's stories, which was hard to manage at times. The author concluded that whilst mindfulness could be helpful for therapists, more research is needed into its challenges, so that appropriate support can be offered. This is the only paper to pick up on the possible negatives of mindfulness practice, which is indicative of balance in the write-up. The author also reflects on how the challenges of mindfulness may be under-reported by therapists, which leads to thinking about how permission may be given for this to be talked about in future research.

**Summary and conclusions.** The studies above give some interesting ideas about how mindfulness may increase empathy: through increased attention, increased self-awareness; increased emotional-regulation; or a change in the perception of the self. These studies therefore offer support to theoretical ideas that MBIs may increase empathy through increasing internal resources, through re-perceiving, or through emotional exposure. Two papers (Bailie et al., 2011; Bermudez et al., 2013) helpfully extend the examination of the relationship between mindfulness and empathy from the focus that the literature has had so far on healthcare workers, to other populations, which opens up ideas about further research. Two critiques that are pertinent for most papers (with the exception of Keane (2013) and

Bailie et al. (2011)) is the lack of reflection by the authors on how their own position affected the course of research, and the lack contradictory findings presented and discussion of alternative explanations. These critiques seem connected since it is likely the authors had an interest in mindfulness, and by not providing evidence of reflection on this, or an openness to contradictory findings, there is a question of bias. It is also worth noting that qualitative accounts rely on participants' self-reporting of experiences, which, as the mixed quantitative evidence reviewed above highlights, may not directly translate into changes in empathic behaviour.

### **Discussion**

The aim of this review was to examine the evidence for the relationship between mindfulness and empathy, to see whether MBIs lead to improvements in empathy, and if so to gain ideas about what the mechanisms of action might be. Correlational studies, using self report measures, suggest a relationship between measures of mindfulness and empathy, and qualitative studies offer some ideas for how these could be related, however, experimental findings and findings from studies employing performance based measures are more mixed. This review will now try to summarise where some of the significant and non-significant findings lay.

One difference that might be relevant is the way that empathy was measured, particularly, whether as a global construct (for example, using the JSPE or summing the IRI subscales) or a multifaceted construct (where the IRI subscales were examined separately). Of those studies using a global measure, seven out of eight obtained a significant result, whereas three out of 11 studies using separate subscales did not find a change on any of the subscales, and none found a change across all three subscales. One explanation for this is that MBIs may be able to affect empathy when it is given a wide definition, but do not make as much of a change on some of its specific components. Looking at the studies which have

examined the individual IRI subscales, this review has found more support for MBIs being related to reduced Personal Distress than to increased Perspective Taking or increased Empathic Concern (a significant relationship was found in eight studies compared to only five and four respectively). Although some of these studies were correlational, and therefore causation cannot be established, the idea that MBIs may be related to reduced distress in the face of other's suffering fits with theory about MBIs reducing the potency of negative emotions through exposure (Holzel et al., 2011) and re-perceiving (Shapiro et al., 2006). These processes may allow practitioners to feel more at ease not only with their own distress, but with others' too. This may not directly translate into an increase in affective or cognitive empathy, which could be more trait-like components (Walmark et al., 2012). It is also possible that an empathy training or LKM component may need to be added to an MBI to effect change on cognitive or affective empathy, as the majority of studies that found a significant effect did add components like these. Theoretically therefore, MBIs may be able to help practitioners to moderate their distress in the face of another's distress, but compassion training may be needed to encourage connection to, and understanding of, another's emotions (Kristeller & Johnson, 2005).

### **Limitations with the evidence discussed**

There are methodological issues that should be borne in mind in the above discussion. One limitation is the lack of RCTs, and where RCTs have been used, the lack of active controls. Non-active control groups are not adequate for MBIs (Mars & Abbey, 2010), due to the non-mindfulness and non-specific components. This makes knowing what was responsible for the effects seen difficult, which is compounded by the variety of MBIs used, and the lack of reporting of adherence to manuals. The only two studies that used a relatively 'pure' form of MBI (Lesh, 1970; Keefe, 1972) found mixed results, and were older studies that were unable to measure mindfulness.

In terms of measuring empathy, the majority of studies have relied on the IRI. The validity of this measure can be questioned, as it has a theoretical rather than empirical basis, and statistical analysis has failed to confirm the four factor structure (Cliffordson, 2001). It is also questionable whether people are able to self-report on their own levels of empathy. The fact that the performance-based measures of empathy have elicited mixed results suggests that conclusions drawn about the effectiveness or not of MBIs on empathy should be tentative. A similar argument follows for studies that have relied on self-report measures of mindfulness, as it has been questioned whether people are able to self-report on how mindful they are, particularly if they do not have much knowledge of the concepts involved in mindfulness (Grossman, 2011).

There are also concerns about the quality of the analyses, for example, some studies are under-powered, increasing the risk of the Type II error, with a lack of intent-to-treat analysis, and often multiple statistical tests are used inflating the chances of Type I errors. There are similar concerns about bias possibly affecting the qualitative data. This limits the reliability of the conclusions.

### **Implications for research**

This review highlights the need for active control groups in investigating MBIs. This leads to the question of what an appropriate control for an MBI is, and in order to answer this, the literature may need to reach more of an agreement on what mindfulness is. In order to understand the effect that mindfulness itself has, ‘purer’ MBIs need to be used, which do not have the additional non-mindfulness factors that MBSR has, and these need to be contrasted with a control group that closely matches the mindfulness exercise. Performance-based measures of mindfulness also need to be developed (Garland & Gaylord, 2009).

This review has highlighted the need for the wider use of well-validated measures of empathy. As with mindfulness, this may require a clearer definition of what empathy is, so

research needs to focus on defining this construct. It has been suggested that empathy is best measured from the client's perspective (Elliott et al., 2011), so studies could consider this as an outcome.

The majority of studies here are limited to looking at healthcare workers, with some looking at undergraduates, but a couple of qualitative studies extended the evidence base by looking at the effect of MBIs on empathy in different populations, and future studies could look further at samples such as parents. Few of the studies here examined the longer-term effects of MBIs, and these need to be looked at, in order to know whether the changes seen are maintained, or indeed whether empathy could increase as a longer-term effect of an MBI.

There are a number of questions for research arising out of this review. Firstly, what is the effect of a 'purer' MBI on a performance-based measure of empathy, in relation to an active control group? Secondly, is additional LKM needed on top of an MBI in order to increase the cognitive and affective components of empathy? Thirdly, do the different components of empathy change at different times as a result of an MBI, with some aspects (such as Personal Distress) perhaps being more immediately affected than others?

### **Clinical implications**

There is not enough evidence to date to recommend mindfulness to therapists as a way of enhancing empathy. Rather, the picture looks complex. However, the evidence for it being useful for stress management in therapists is more convincing than the evidence for its interpersonal benefits (Escuriex & Labbe, 2011). Therapists can therefore still hope to benefit from mindfulness, at least personally, but more research is needed into the interpersonal aspects.

### **Limitations of this review**

This review is limited by being restricted to published literature, which means it is likely there are null findings that have not been represented here. This review also had a

narrow focus on mindfulness and empathy, so other closely related variables that could have a bearing on this relationship, such as therapist presence, have not been considered.

### **Conclusion**

A review of the literature leads to the tentative conclusion that MBIs may be able to increase a practitioner's ability to tolerate other's distress, but may not increase the cognitive and affective components of empathy without additional compassion training. However more high quality research is needed in order to pull apart these complex relationships. Research should focus on deconstructing what the active ingredients of MBIs are, and should use performance-based measures where possible.

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Section B

Does a brief mindfulness practice improve factors that are important to the therapeutic encounter in trainee therapists?

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### Abstract

Mindfulness may be one way in which common factors could be enhanced in trainee therapists. Previous research has found some support for mindfulness increasing empathy, compassion and emotion regulation, but research is affected by a lack of active control groups and interventions with multiple components. An experimental design was used with a brief (15 min) mindfulness practice compared to a brief (15 min) 'mind-wandering' control group, in order to overcome some of these confounds. Participants were 48 trainee therapists who were randomly assigned to two groups of equal sizes. Measures were taken at baseline and post-induction. The predicted findings for empathy and compassion for others were not found. The mindfulness group did show lower negative affect at post-test than the control group, but only in participants who were high in negative affect at baseline, a result which should be viewed tentatively due to the small number of participants in each subgroup of the analysis. Issues limiting confidence in the results are discussed, particularly that the two groups did not report different levels of state mindfulness after the brief exercises. Implications for future research, particularly in considering appropriate active controls for mindfulness, are discussed.

Keywords: mindfulness, brief, empathy, performance-based, affect, compassion for others.

Does a brief mindfulness practice improve factors that are important to the therapeutic encounter in trainee therapists?

Whilst there is an emphasis in current research to identify the most effective type of therapy for different presenting problems (for example, Roth & Fonagy, 2005), research consistently shows that there are multiple factors involved in therapy outcome aside from the model used, including client, therapist, relationship and contextual factors (Norcross & Lambert, 2011). Whilst client and contextual factors may be more difficult to influence, research indicates specific therapist and relationship factors that should be cultivated (for reviews, see Norcross & Lambert, 2011; Beutler et al., 2004). Aspects of the therapeutic encounter that contribute to change across all therapies are termed ‘common factors’ (Blow, Sprenkle & Davis, 2004). Common factors that have been found to be important include: the therapeutic alliance, empathy, collaboration and positive regard (Norcross & Wampold, 2011).

It has been suggested that mindfulness could enhance common factors (Shapiro & Carlson, 2006). Mindfulness can be described as a way of paying attention that is non-judgemental, non-elaborative, and present-centred (Lau et al., 2006). Mindfulness is discussed in the literature in a number of ways: as a meditational or attentional practice, as personality-like trait, or as a state induced by a particular practice (Chambers, Gullone & Allen, 2009). Mindfulness Based Interventions (MBIs) involve the practice of mindfulness, and aim to make changes in either state or trait mindfulness, or both. Mindfulness Based Stress Reduction (MBSR; Kabat-Zinn, 1990) and Mindfulness Based Cognitive Therapy (MBCT; Teasdale, Segal & Williams, 1995) are two well-researched MBIs, which are 8-week courses involving didactic teaching and experiential mindfulness exercises. There is evidence that participation in MBIs can be helpful for a range of outcomes, with the largest effect sizes found for symptoms of anxiety, depression and stress (Khoury et al., 2013).

However, MBIs contain multiple components, including non-mindfulness factors (such as psycho-education on cognitions or stress), and non-specific factors (such as group and facilitator effects) making it hard to know if mindfulness is the active ingredient, particularly as comparison control groups are typically inactive.

The potential benefits of mindfulness for therapists in their work is a relatively new area of research. However, there are a number of theoretical mechanisms through which mindfulness could enhance common factors. Shapiro, Carlson, Astin and Freedman (2006) propose that mindfulness enhances re-perceiving, that is, the ability to stand back and observe thoughts and emotions. Theoretically, re-perceiving could help therapists to: approach their client's emotions with openness (Bruce, Manber, Shapiro & Constantino, 2007), regulate their own emotions (Holzel et al., 2011), and have a more compassionate view of others (Shapiro et al., 2006). Siegel (2007) proposed that mindfulness is a form of self-attunement, an idea based on attachment theory (Bowlby, 1969), which suggests that mindfulness enables the therapist to develop a secure base within themselves. This could provide the stability from which the therapist is able to explore emotions and do the work of therapy (Bateman, Brown & Pedder, 2010).

Enhancing common factors may be particularly important for therapists in training. Therapists in training can face unique stressors, such as being newly exposed to high levels of uncertainty (Rizq, 2009), and having a limited skill set to work with others' distress (Orlinsky & Ronnestad, 2005). Elevated levels of stress have been found in this population, which could impact on interpersonal functioning (Pakenham & Stafford-Brown, 2012). Whilst training courses improve trainee therapists' competencies in specific therapeutic models, relational skills develop less during training, and in some cases decrease (Dennhag & Ybrandt, 2013). Given this, and that relational skills are difficult to teach (Dennhag & Ybrandt, 2013), exploring whether mindfulness has the potential to improve common factors

for trainee therapists seems important. Whilst common factors are multiple and complex, this study will focus on three which evidence highlights, and which mindfulness could impact: empathy, compassion for others, and emotion regulation.

### **Empathy**

Empathy can be defined as the ability to understand another's internal frame of reference, whilst maintaining a separate sense of self (Rogers, 1959). It is conceptualised as having an affective component, where the emotions of the other are felt, a cognitive component, where the feelings of another are understood cognitively, and a self-regulatory component, where the other's emotions are kept separate from the self (Decety & Jackson, 2004). A meta-analysis concluded that therapist empathy is a moderately strong predictor of therapy outcome (Elliott, Bohart, Watson & Greenberg, 2011), although this was mainly based on studies relying on self-report measures of empathy.

It has been proposed that mindfulness could increase empathy through decreasing stress and thus increasing internal resources to be empathic (Snyder, Shapiro & Treleaven, 2012); or through re-perceiving, where the practitioner's willingness to approach negative emotions in others may increase (Bruce et al., 2007). Five Randomized Controlled Trials (RCTs) have looked at the relationship between MBIs and empathy. Two used a self-report measure of empathy, and found a significant increase compared to a waiting-list control following MBSR (Shapiro, Brown, Thoresen & Plante, 2011; Shapiro, Schwartz & Bonner, 1998). Three used performance-based measures of empathy, which require participants to infer others' mental states from pictures or videos, and could overcome some of the limitations of self-report measures (Dziobek et al., 2006). In comparison to a waiting-list control, one study found improvements on a performance-based measure following an MBI (Kemeny et al., 2011), whilst two did not (Keefe, 1979; Pearl & Carlozzi, 1994), leaving open the question of whether mindfulness improves empathy on these more ecologically

valid measures. A limitation of this research is the reliance on waiting-list controls, which, combined with the use of MBI's with multiple and varying components, makes it difficult to conclude whether it is mindfulness that is having the effect seen.

### **Compassion for others**

Compassion is defined as an awareness of suffering and a desire to relieve that suffering (Neff, 2003). It has been suggested that compassion is integral to the therapeutic relationship (Shapiro & Carlson, 2009). This is supported by a review concluding that therapists are more effective if they have a style characterised by warmth and a lack of hostility (Beutler et al., 2004), although the review was limited by the small quantity of research in the area. Compassion and mindfulness are thought to be linked, indeed it has been suggested that mindfulness is a pre-requisite for compassion (Neff, 2003). Research has examined the link between MBIs and self-compassion in healthcare professionals (see Irving, Dobkin & Park, 2009, for a review), however only three quantitative studies to date have examined MBIs and compassion for others. In two uncontrolled studies, adapted MBSR was used with health professionals, and no change was found on a measure of compassion for others (Brooker et al., 2013; Fortney, Luchterhand, Zakletskaia, Zgierska, & Rakel, 2013). Kemeny et al. (2011) found an MBI led to activation of compassionate related semantic networks, but not to more compassionate behaviour than a control group when resolving arguments. The question of whether MBIs lead to more compassion for others therefore needs further investigation. It has been suggested that self-compassion and compassion for others are connected (Shapiro & Carlson, 2009; Neff, 2003), however, preliminary evidence suggests this may not be the case across all groups (Neff & Pommier, 2013). When considering the benefits of mindfulness for therapists, it would therefore be useful for research to specifically measure compassion for others.

**Emotion regulation**

Therapists are exposed to high levels of negative emotion, which if they are not able to regulate, could impact on their wellbeing (Harrison & Westwood, 2009), and also on the outcome of therapy (Hayes, Gelso & Hummel, 2010). Mindfulness may provide an alternative to less helpful ways of coping with emotion (such as suppression or rumination) through meta-cognitive awareness, the understanding that mental events are simply experiences (Chambers et al., 2009). MBIs have been shown to lead to reduced negative affect, compared to a waiting list control, in a number of populations, including trainee therapists (Shapiro et al., 2007) and teachers (Kemeny et al., 2011), however these studies attract the critique above of not controlling for non-mindfulness and non-specific factors. Three studies used a short (less than 20 min) MBI, which because of the brevity of the intervention includes fewer non-mindfulness factors, and so can begin to overcome some of these confounds. The results of a short MBI have been mixed: one study found a 10 min MBI led to reduced negative affect in response to an affectively mixed film clip, in comparison to an educational talk control group (Erisman & Roemer, 2010), however, when a guided-imagery control group was used in a comparison to a 10 min MBI, no between group differences were found (Ortner & Zelazo, 2012). A 15 min MBI was compared to a 'mind wandering' control group (who were asked to let their minds wander) and a 'worry' control group (who were asked to focus on worries; Arch & Craske, 2006). At post-test the mindfulness group reported less negative affect than the worry group, but not than the mind-wandering group, leaving open the question of whether it was the negative impact of worry that led to the difference seen. These studies highlight the need for more well controlled research in this area.



### **Study aims**

In summary, MBIs could provide an avenue to improve common factors such as empathy, compassion for others, and emotion regulation, but research in this area is hindered by a reliance on self-report measures and a lack of active control groups. One way to overcome some of these challenges is to use a brief MBI, to which a control exercise can be more easily matched. A brief practice could also be a cost and time effective option, which is important for busy populations like trainee therapists.

The aim of the study was to examine whether empathy, compassion and emotion regulation were affected by a brief mindfulness practice in trainee therapists using an experimental design. A performance-based measure of empathy was used, which meant that the study focused on cognitive empathy, as this is what these measures are thought to tap into. The control exercise was matched as closely as possible to the mindfulness exercise, but without the key components of focused attention and an attitude of compassionate acceptance; this is termed an ‘unfocused attention’ or ‘mind wandering’ exercise, and has been used successfully by: Kiken and Shooke, 2011; Saunders, Barawi and McHugh, 2013; and Garland, Hanley, Farb and Froeliger, 2013. The hypotheses were that following a brief induction exercise:

1. Participants in the mindfulness condition will have higher levels of state mindfulness than participants in the control condition, when baseline levels of trait mindfulness are controlled for.
2. Participants in the mindfulness condition will have higher levels of state cognitive empathy than participants in the control condition, when baseline levels of trait cognitive empathy are controlled for.

3. Participants in the mindfulness condition will show lower negative affect than participants in the control condition, when baseline levels of negative affect are controlled for.
4. Participants in the mindfulness condition will have higher compassion for others than participants in the control condition, when baseline levels of trait compassion for others are controlled for.

## **Method**

### **Design**

A randomized controlled experiment was conducted<sup>1</sup>. Due to early data suggesting that the outcome measures varied according to year in training, participants were matched for year group as follows: year 1 of a three-year course; year 2 of a three-year course; year 3 of a three-year course; one-year course. Randomisation was matched by year group. Attempts were made to blind participants to the study hypotheses by not mentioning 'mindfulness' in the research or advertising materials. It was not possible to blind the researcher. Trait measures of variables were taken at baseline, then, following an induction exercise (a 15 min mindfulness or control practice), state measures of the same variables were taken. This allowed for comparison between groups of state changes, whilst controlling for baseline differences. It was not possible to control for state differences at baseline due to lack of appropriate measures in the literature.

### **Measures**

**Baseline measures.** The Interpersonal Reactivity Index (IRI; Davis, 1983) is a 28 item measure of empathy, answered on a 5-point Likert scale. There are four subscales: empathic concern, perspective taking, personal distress, and fantasy. The perspective taking

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<sup>1</sup> There was an additional small qualitative component to the study, and whilst it was beyond the scope of this report to examine these data, they will be analysed and published if of interest.

subscale, a measure of cognitive empathy, was used in this study. The internal reliability of the subscales ranges from .70 to .82 (Davis, 1983). The perspective taking subscale correlates more highly with a measure of cognitive empathy ( $r = .40, p < .05$ ) than the fantasy and empathic concern subscales, which correlate more closely with a measure of emotional empathy ( $r = .52, p < .05$ ;  $r = .60, p < .05$ ; Davis, 1983). The fantasy subscale is often excluded due to its poor concurrent validity with other measures (Baron-Cohen & Wheelwright, 2004). Higher empathy is thought to relate to lower scores on personal distress and higher scores on empathic concern and perspective taking.

The Five Facet Mindfulness Questionnaire – Short Form (FFMQ-SF; Bohlmeijer, ten Klooster, Fledderus, Veehof & Baer, 2011) is a brief version of the Five Facet Mindfulness Questionnaire (FFMQ; Baer, Smith, Hopkins, Krietemeyer, & Toney, 2006). It consists of 24 questions, answered on a 5-point Likert scale, and can be divided into five subscales: observing (for example, “I notice the smells and aromas of things”); describing, (for example, “I’m good at finding words to describe my feelings”); acting with awareness (for example, “I rush through activities without really being attentive to them” (reverse scored)); non-judging (for example, “I disapprove of myself if I have illogical ideas” (reverse scored)); and non-reactivity (for example, “I watch my feelings without getting carried away with them”). The factors have good internal consistency ( $\alpha = .75$  to  $.87$ ), the subscales correlate highly with the subscales of the full FFMQ ( $r = .89$  to  $.98$ ), and scores increase following an MBI (Bohlmeijer et al., 2011).

The Positive and Negative Affect Scale (PANAS; Watson, Clark & Tellegen, 1988) is a measure of affect. Participants are given a list of 20 emotions and asked to rate on a 5-point Likert scale to what extent they feel each now, or have felt each in the last week. Items load onto two factors: positive affect (including items such as ‘enthusiastic’, and ‘alert’), and negative affect (including items such as ‘afraid’ and ‘distressed’; Crawford & Henry, 2004).

The two subscales have good internal consistency ( $\alpha = .84$  to  $.90$ ) and scores remain stable over time (Watson et al., 1988). The negative affect subscale correlates positively with measures of depression and anxiety ( $r = .56$ ;  $r = .51$ ), whilst the positive affect subscale correlates negatively with these measures ( $r = -.35$ ;  $r = -.35$ ; Watson et al., 1988). Since, from the literature reviewed above, it is the regulation of negative emotion that seems most important for this sample, the scores from the negative subscale will be used. Due to its ability to be used both as a trait and a state measure (Crawford & Henry, 2004) the PANAS was used at baseline and post-induction.

The Santa Clara Brief Compassion Scale (SCBCS; Hwang, Plante & Lackey, 2008) is a brief version of the Compassionate Love Scale (Sprecher & Fehr, 2005), a measure of compassion towards non-intimate others. Participants answer 5 questions on a 7-point Likert scale. The SCBCS has high internal reliability ( $\alpha = .90$ ) and correlates well with the full length version ( $r = .95$ ,  $p < .01$ ; Hwang et al., 2008). Whilst construct validity of the SCBCS has not been examined in-depth, significant correlations have been found with measures of empathy ( $r = .65$ ,  $p < .01$ ), vocational identity ( $r = .48$ ,  $p < .01$ ), and religious faith ( $r = .27$ ,  $p < .01$ ; Hwang et al., 2008).

**Post-induction measures.** The Toronto Mindfulness Scale (TMS; Lau et al., 2006) is a measure of state mindfulness. Participants answer 13 questions about experiences during a recent mindfulness practice. The TMS loads onto two subscales: curiosity (for example, 'I remained curious about the nature of each experience as it arose') and decentering (for example, 'I experienced myself as separate from my changing thoughts and feelings'; Lau et al., 2006). The TMS has good internal consistency ( $\alpha = .95$ ; Lau et al., 2006) and scores increase with mindfulness practice (Lau et al., 2006).

The Movie for the Assessment of Social Cognition – Multiple Choice (MASC-MC; Dziobek et al., 2006) is an ecologically valid (Dziobek et al., 2006) measure of cognitive

empathy, in which participants watch a movie about four people getting together for a dinner party. The movie is stopped at 45 points and multiple choice questions asked, requiring participants to identify characters' subtle emotional and cognitive states. Six additional 'control' questions check that participants have followed the plot of the movie. The MASC-MC has good discriminant validity in distinguishing people with Asperger's (normally associated with cognitive empathy deficits; Dziobek et al., 2006). The MASC-MC correlates with performance on a task involving recognising emotions from faces ( $r = .72, p < .01$ ), has high test-retest reliability ( $r = .97$ ), and good internal consistency ( $\alpha = .84$ , Dziobek et al., 2006). Based on means from the general population, it was not thought that ceiling effects would be observed in trainee therapists (I. Dziobek, personal communication, May 4, 2012).

A literature search did not identify an existing state measure of compassion for others, therefore a measure was developed for this study. The SCBCS (Hwang et al., 2008) was examined and adaptations to it were made in order to make it suitable for use as a state measure. The questions in the new measure were tied to the characters in the movie that had just been viewed (as part of the MASC-MC) to try to make the measure as ecologically valid as possible. The new measure was named the Movie Compassion measure. Participants were asked to rate how much compassion they felt for each of the four characters in the movie, and the four sub-scales were summed giving an overall total. The Movie Compassion measure was piloted with a small sample of trainee Clinical Psychologists, and no ceiling or floor issues were found, with qualitative feedback demonstrating that the participants had found the measure easy to use and understand. The measure was therefore not altered for the main project. In the main sample, the Movie Compassion measure correlated well with the SCBCS ( $r = .54, p < .01$ ), suggesting good construct validity. Internal consistency was poor ( $\alpha = .39$ ), and would not have been significantly improved by deletion of any item. Low Chronbach's alpha can be caused by short test-length, in which case it can be helpful to examine

correlations between each item and the total score (Tavakol, 2011). All four items correlated significantly with the total score, providing some support for the reliability of the measure (see Appendix 7).<sup>2</sup>

Reported adherence to the induction instructions was measured using a 7-point Likert scale, in response to the question, ‘during the instructions that you just heard, how far did you follow them?’.

### **Participants**

The main measure of interest (MASC-MC) has previously elicited large effect sizes (Dziobek et al., 2006). A power calculation using Gpower2 (Faul, Erdfelder, Lang, & Buchner, 2007) indicated that an ANCOVA with an effect size of .8 and an alpha of .05 would require 52 participants, 26 in each group. Eighty-one participants registered for the study, with 24 completing in each group (see Figure 1 for flow of participants through the study). Participants were included if they were on an accredited training course and had therapeutic contact with clients, and were excluded if they were in the principle researcher’s year group.

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<sup>2</sup> The original proposal for the project also included a measure of memory, however, since time did not allow for the measure to be properly validated, the variable was not included in the write-up.

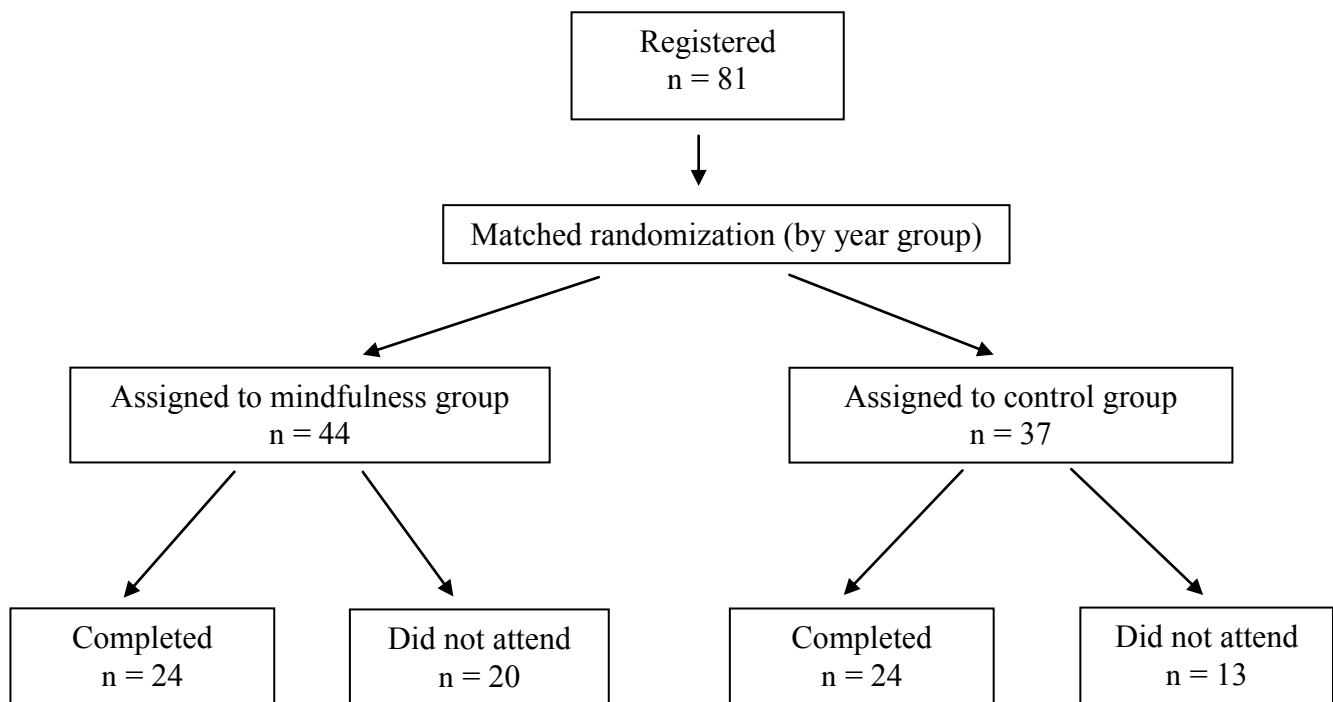


Figure 1. Flow of participants through the study

### Procedure

Recruitment took place between January and December 2013 (see Appendix 9 for materials). Three Universities (Salomons Centre for Applied Psychology, University of Surrey and University of East London) were approached and approval was sought from the Research Directors to run the project. Advertising posters were put up. An email was sent to eligible year groups with the information sheet and consent form attached; where University policy prohibited group emails to trainees about research, flyers were placed in trainees' pigeon holes. The emails and flyers were followed by an announcement from the principle researcher in trainees' lectures, where a brief outline of the project was given and a sign-up sheet was provided. A prize draw of one £50 shopping voucher was offered to thank trainees for their time.

Once trainees who had registered had been randomly allocated to group, an email was sent inviting attendance. The project took place at the trainees' Universities in groups of one

to five participants. If no response was received, a second email was sent. To those who confirmed their attendance, a reminder email was sent two days before the project ran.

The project took 1 hr 40 min to run on average. On arrival, participants read the information sheet and signed the consent form. Participants then completed the baseline measures (IRI; FFMQ; PANAS; SCBCS). Once completed, participants listened to the 15 min induction instructions as a group, these were recorded in advance to ensure consistency. The induction instructions (see Appendix 10) were based on Arch and Craske (2006) and Kiken and Shooke (2011). In summary, the mindfulness induction invited participants to focus their attention on their breathing, and when they noticed their mind had wandered to bring their attention back to their breath with kindness and curiosity. The control induction asked participants to let their mind wander freely, with the emphasis being on thinking about whatever they wanted, in an unfocused way. The two inductions were matched as closely as possible for number of words and length of pauses. Following the induction, participants completed the TMS and the reported adherence measure. Participants then watched the MASC-MC as a group. After every third question, the MASC-MC was paused for 10 seconds and participants were reminded of the induction instructions, for example, the mindfulness group were asked to bring their attention back to their breath, and the control group were asked to let their mind wander. This was intended to maintain the induced state throughout the movie. Following the MASC-MC participants completed a second reported adherence measure, and a second TMS to check that the induced state was maintained. Finally the second PANAS and the Movie Compassion measure were completed, and participants filled in demographic information. Participants were debriefed and thanked for their time.

### **Ethical considerations**

Consideration was given to trainees participating without feeling coerced, therefore trainees from the principle researcher's year group were excluded, and a prize draw voucher



was offered rather than individual payments. Trainees were informed about what the study involved and could withdraw at any time. Ethical approval was sought and granted by two Universities' panels.

### **Analysis**

Analysis was conducted using IBM SPSS version 21.0. Assumptions were checked for all analyses, with reference to information from Field (2009) and Howell (2002) (see Appendix 11). For the main analysis, Analysis of Covariance (ANCOVA) was conducted for the four dependent variables, with baseline data entered as the covariate. *Partial  $\eta^2$*  was used as a measure of effect size according to the following conventions: .01, small; .06, medium; and .14, large (Cohen, 1988). Unplanned analyses were carried out where it seemed these could lead to further understanding where hypothesised differences had not been found.

## **Results**

### **Demographic data**

The mean age was 31 years (mindfulness:  $M = 30$ ,  $SD = 5.1$ ; control:  $M = 32$ ,  $SD = 8.0$ ), the age range was 22-55 years (mindfulness: 24-43 years; control: 22-55 years), there was not a significant difference between groups age,  $t(44) = 1.13$ ,  $p = .26$ . Categorical demographic data are shown in Table 1. Between group differences were calculated using Fisher's exact test. There were no significant differences between groups in terms of gender ( $p = 1.0$ ), ethnicity ( $p = .26$ ), type of training course ( $p = .84$ ), or year of training ( $p = .91$ ). Participants had therefore been successfully matched on year in training.

Table 1.

Number of participants in each demographic category				
<u>Characteristic</u>	<u>Sub-category</u>	<u>Overall</u>	<u>Mindfulness</u>	<u>Control</u>
Gender	Male	9	5	4
	Female	39	19	20
Ethnicity	White British	36	19	17
	White Other	7	2	5
	Asian	2	0	2
	Not specified	3	3	0
Course	Clinical Psychology	30	16	14
	Counselling Psychology	5	2	3
	Cognitive Behavioural Therapy	13	6	7
Year	One-year course	13	6	7
	Year 1	18	8	9
	Year 2	12	6	6
	Year 3	6	4	2

Table 2 presents data on how much participants reported practicing mindfulness in the last year, and whether they had a spiritual practice. Mindfulness was split into two categories: stationary mindfulness, which included mindfulness of breathing, body scan and loving-kindness meditations; and mindful movement practices, which included yoga, tai chi and other martial arts. Fisher's exact test indicated that there were no significant differences between the groups in terms of mindfulness (stationary) practice ( $p = .10$ ), mindfulness (movement) practice ( $p = .35$ ), or prayer/ spiritual practice ( $p = .11$ ).

Table 2.

Frequency with which participants practiced in the last year				
<u>Practice</u>	<u>Frequency</u>	<u>Overall</u>	<u>Mindfulness</u>	<u>Control</u>
Mindfulness: stationary	None	9	4	5
	Less than monthly	16	12	4
	Monthly	10	4	6
	Fortnightly	3	1	2
	Weekly	6	3	3
	Daily	4	0	4
Mindfulness: movement	None	20	11	9
	Less than monthly	14	8	6
	Monthly	1	1	0
	Fortnightly	3	0	3
	Weekly	10	4	6
	Daily	0	0	0
Prayer/ spiritual practice	None	39	21	18
	Less than monthly	1	1	0
	Monthly	0	0	0
	Fortnightly	1	1	0
	Weekly	4	0	4
	Daily	3	1	2

### Pre-analysis tests

**Examination of reported adherence.** Table 3 shows data for reported adherence to the induction instructions. There were no significant differences between groups on reported adherence at Check 1,  $t(46) = .75$ ,  $p = .46$ , or at Check 2,  $t(46) = -1.79$ ,  $p = .08$ . On average participants scored 5 – 6, indicating that they ‘mostly’ followed the instructions.

Table 3.

Descriptive statistics for reported adherence			
<u>Check</u>	<u>Group</u>	<u>Mean</u>	<u>Standard deviation</u>
Check 1	Mindfulness	5.46	1.22
	Control	5.71	1.08
Check 2	Mindfulness	5.96	1.04
	Control	5.38	1.21

**Examination of changes on TMS over time.** Table 4 shows descriptive data for the TMS at the two time points. There was no significant difference between the TMS post induction and the TMS post MASC-MC, for either the mindfulness group,  $t(23) = 1.56$ ,  $p = .13$ , or the control group,  $t(23) = .58$ ,  $p = .57$ , suggesting that if a state had been induced by the induction exercises, this was maintained over time in both groups.

Table 4.

Descriptive statistics for TMS at two time-points

<u>Group</u>	<u>Variable</u>	<u>Mean</u>	<u>Standard deviation</u>
Mindfulness	TMS post induction	40.13	6.67
	TMS post MASC-MC	37.33	9.07
Control	TMS post induction	40.33	7.62
	TMS post MASC-MC	38.96	7.92

Note. TMS, Toronto Mindfulness Scale. MASC-MC, Movie for the Assessment of Social Cognition – Multiple Choice.

The TMS post-induction will be used in the remaining analyses, as this is likely to be the most accurate representation of any effect of the induction.

### Main analysis

Descriptive statistics for all measures not already discussed, including the individual subscales used in the analyses, are shown in Table 5 for both groups. Normative data is also provided, where available in the literature, for comparison.

Table 5.

Means and standard deviations (in parentheses) for all subscales used in the analysis, for the mindfulness group, the control group, and normative data

<u>Measure</u>	<u>Mindfulness</u>	<u>Control</u>	<u>Normative data</u>
IRI (Perspective Taking subscale)	20.33 (3.55)	20.92 (2.12)	17.96 (-)
IRI (Empathic Concern subscale)	22.17 (2.81)	21.38 (3.23)	21.67 (-)
IRI (Personal Distress subscale)	10.25 (3.99)	10.42 (3.71)	12.28 (-)
FFMQ – SF	83.25 (7.37)	79.54 (10.22)	84.7 (3.63)
PANAS (Negative subscale); baseline	14.00 (4.72)	13.63 (4.42)	14.8 (5.4)
SCBCS	26.42 (3.36)	27.04 (3.21)	27.6 (3.4)
MASC – MC	36.04 (3.61)	37.04 (3.24)	34.8 (2.7)
PANAS (Negative subscale); post-induction	11.58 (1.91)	13.04 (3.93)	-
Movie Compassion measure	17.88 (3.42)	19.04 (3.54)	-

Note. IRI, Interpersonal Reactivity Index. FFMQ-SF, Five Facets Mindfulness Questionnaire – Short Form. PANAS, Positive and Negative Affect Scale. SCBCS, Santa Clara Brief Compassion scale. MASC – MC, Movie for the Assessment of Social Cognition – Multiple Choice. (-), not available in literature.

**Comparison to normative data.** Table 5 indicates that this sample performed slightly above the normative sample on all measures of empathy. Participants in this sample had slightly lower negative affect than the normative data, and slightly lower mindfulness. Compassion for others was approximately the same.

**Relationship between covariate and dependent variable.** The relationship between the covariate and the dependent variable was significant for: mindfulness,  $F(1, 45) = 7.94$ ,  $p = .007$ ,  $partial \eta^2 = .15$ ; negative emotion,  $F(1, 44) = 63.42$ ,  $p < .001$ ,  $partial \eta^2 = .59$ ; and compassion for others,  $F(1, 45) = 17.63$ ,  $p < .001$ ,  $partial \eta^2 = .28$ . The relationship between the covariate and the dependent variable was not significant for empathy,  $F(1, 45) = 1.17$ ,  $p = .290$ ,  $partial \eta^2 = .03$ .

The main findings from the ANCOVAs will be reported according to the original hypotheses.

**Hypothesis 1.** Contrary to hypothesis 1, there was no significant effect of group on post-induction mindfulness after controlling for baseline mindfulness,  $F(1, 45) = .48$ ,  $p = .49$ ,  $partial \eta^2 = .01$ .

**Hypothesis 2.** Contrary to hypothesis 2, there was no significant effect of group on performance on the MASC after controlling for baseline Perspective Taking,  $F(1, 45) = 1.25$ ,  $p = .27$ , *partial*  $\eta^2 = .03$ .

**Hypothesis 3.** Hypothesis 3 stated that participants in the mindfulness condition would show lower negative affect than participants in the control condition, when baseline levels of negative affect were controlled for. The assumption of homogeneity of regression slopes was not met for the PANAS, as there was a significant interaction between group and baseline negative emotion [ $F(1, 44) = 24.39$ ,  $p < .001$ , *partial*  $\eta^2 = .36$ ]. There was a significant effect of group on post-test negative emotion after controlling for baseline negative emotion, with a large effect size,  $F(1, 44) = 14.16$ ,  $p < .001$ , *partial*  $\eta^2 = .24$ . In a review of the use of ANCOVA, Miller and Chapman (2001) conclude that violation of the assumption of homogeneity of regression slopes is not problematic, as long as any significant main effect is understood in the context of a significant interaction between the independent variable and the covariate; disregarding data that has violated this assumption can lead to inappropriately dismissing significant findings (Todman & Dugard, 2007). Simple effects analysis was used, along with a regression graph, to understand the interaction further. Participants were split into three groups (along the 33<sup>rd</sup> and 67<sup>th</sup> percentile) based on baseline negative affect. The effect of group on post-test negative affect, at the different levels (low, medium and high) of baseline negative affect was examined. Table 6 shows descriptive data for this analysis. There was no effect of group on post-test negative affect if participants had low negative affect at baseline,  $F(1,42) = .03$ ,  $p = .874$ , *partial*  $\eta^2 = .01$ , or medium negative affect at baseline,  $F(1,42) = .140$ ,  $p = .244$ , *partial*  $\eta^2 = .03$ . When participants had high baseline negative affect, at post-test they had significantly less negative affect if they were in the mindfulness group than if they were in the control group,  $F(1,42) = .10.08$ ,  $p = .003$ , *partial*  $\eta^2 = .19$ . This supports the pattern that can be seen in Figure 2.

Table 6.

Descriptive statistics for mindfulness and control groups in post-test negative affect, at different levels of baseline negative affect.

Baseline negative affect	n		Mean (and SD) T1 PANAS		Mean (and SD) T2 PANAS neg.	
	M	C	neg.		M	C
			M	C		
Low	10	9	10.50 (.53)	10.33 (.50)	10.60 (1.35)	10.78 (.67)
Medium	7	10	12.57 (.79)	13.00 (.94)	11.29 (1.38)	12.70 (2.50)
High	7	5	20.43 (3.64)	20.80 (4.49)	13.29 (2.06)	17.80 (5.81)

Note. T1 PANAS neg., baseline score on Negative scale of Positive and Negative Affect Scale. T2 PANAS neg., post-test score on Negative scale of Positive and Negative Affect Scale. M, mindfulness group. C, control group.

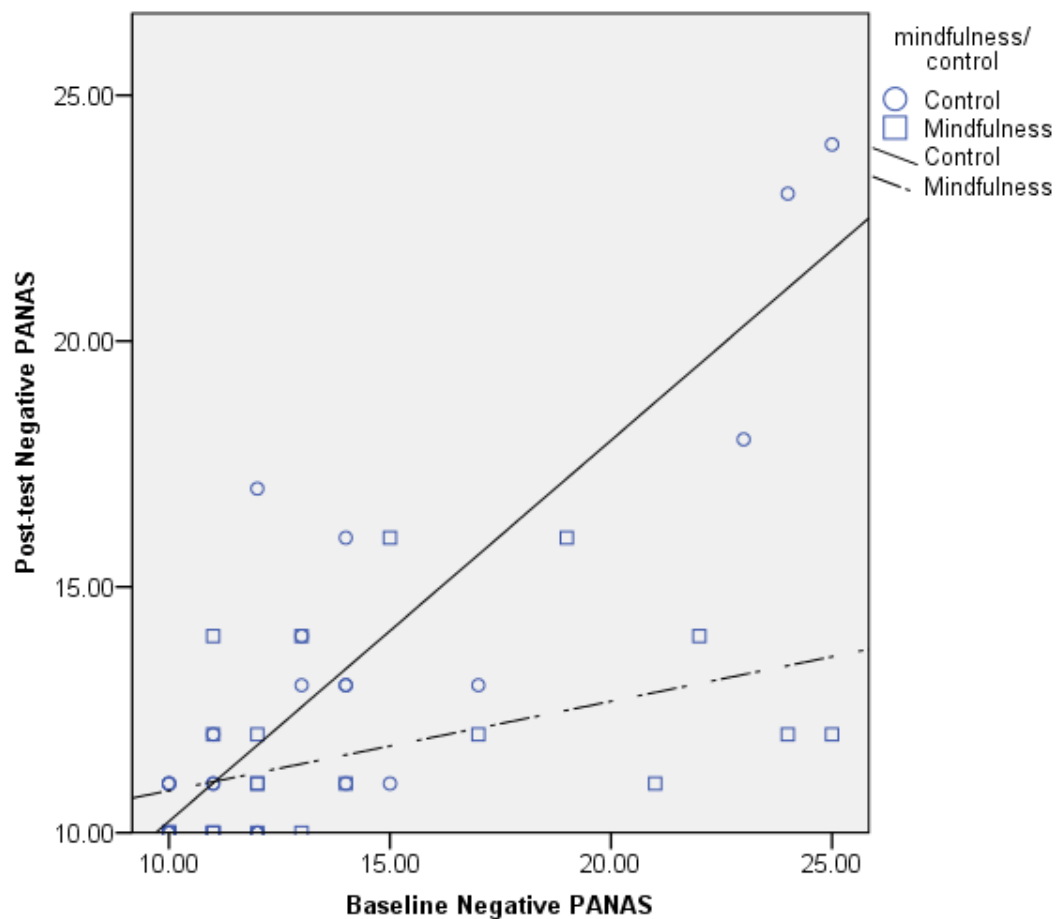


Figure 2. The effect of group on post-test negative affect (as measured by the Positive and Negative Affect Scale; PANAS) at different levels of baseline negative affect (as measured by the Positive and Negative Affect Scale; PANAS).

In the high negative affect group, the means for both the control and mindfulness groups went down at post-test compared to baseline. Within-subjects t-tests revealed that this

was a significant reduction for both groups (mindfulness:  $t(6) = 3.72$ ,  $p = .01$ ; control:  $t(4) = 3.59$ ,  $p = .02$ ). Therefore, at high baseline negative affect, both groups had significantly reduced negative affect at post-test, but the mindfulness group decreased significantly more than the control group.

**Hypothesis 4.** Contrary to hypothesis four, there was no significant effect of group on post-induction compassion for others after controlling for baseline compassion for others,  $F(1, 45) = .89$ ,  $p = .35$ , *partial*  $\eta^2 = .02$ .

### Unplanned analyses

**Measures of empathy.** Since Perspective Taking on the IRI was not related to performance on the MASC-MC, correlations were examined between the MASC-MC and the remaining IRI subscales. Two-tailed Spearman's correlations showed there were not a significant relationship between the MASC-MC and Empathic Concern ( $r_s = .02$ ,  $p = .89$ ) or between the MASC-MC and Personal Distress ( $r_s = .26$ ,  $p = .08$ ).

**Correlations with TMS.** In order to see if there was a relationship between mindfulness state induced and the outcomes of interest, two-tailed Spearman's correlations were examined between the TMS and the dependent variables. No significant relationship was found between the TMS and: the MASC-MC ( $r_s = -.13$ ,  $p = .39$ ), post-test negative PANAS ( $r_s = -.15$ ,  $p = .31$ ), or the Movie Compassion measure ( $r_s = -.01$ ,  $p = .93$ ).

**Effect of mindfulness practice.** To investigate whether prior experience of mindfulness influenced how the induction exercises were used, participants were split according to self-reported mindfulness practice in the last year. 'High' indicated weekly or more practice, 'low' indicated fortnightly or less; both stationary and mindful movement practices were included. A factorial Analysis of Variance (ANOVA) was run with the independent variables as: practice (high, low) and group (mindfulness, control); TMS was the dependent variable. Table 7 shows descriptive statistics for the analysis.



Table 7.

Descriptive statistics for Toronto Mindfulness Scale according to previous mindfulness practice and group.

Practice	N	Mean (and standard deviation) for each group		
		Overall	Mindfulness	Control
Low	31	38.48 (6.11)	39.17 (6.84)	37.53 (5.04)
High	17	43.41 (7.80)	43.00 (5.73)	43.64 (8.99)

Practice had a significant effect on TMS with a medium effect size,  $F(1, 44) = 5.37, p = .03, \text{partial } \eta^2 = .12$ . There was no main effect of group,  $F(1, 44) = .05, p = .82, \text{partial } \eta^2 = .001$ , and no significant interaction,  $F(1, 44) = 13.15, p = .60, \text{partial } \eta^2 = .01$ . Therefore, if participants had a regular mindfulness practice their scores on the TMS were higher than those without a regular mindfulness practice, regardless of whether they were asked to do a control or mindfulness exercise in the study. Practice was not related to baseline mindfulness as measured by the FFMQ,  $t(46) = -1.05, p = .30$ .

To determine whether prior mindfulness practice had an influence on the outcome variables, one-way ANOVAs were conducted with practice (high, low) as the independent variable. There was no significant effect of practice on negative PANAS post-induction [high:  $M = 12.53, SD = 3.34$ ; low:  $M = 12.19, SD = 3.08$ ;  $F(1, 46) = .12, p = .73, \text{partial } \eta^2 = .003$ ], or on Movie Compassion [high:  $M = 18.06, SD = 3.71$ ; low:  $M = 18.68, SD = 3.47$ ;  $F(1, 46) = .34, p = .56, \text{partial } \eta^2 = .01$ ]. For empathy the result neared significance and a medium effect size was obtained, but the effect went in the opposite direction to expected, with the low practice group scoring slightly higher on the MASC-MC ( $M = 37.16, SD = 3.14$ ) than the high practice group ( $M = 35.41, SD = 3.73$ );  $F(1, 46) = 2.98, p = .09, \text{partial } \eta^2 = .06$ .

### Discussion

The aim of this study was to use a well-controlled experimental design to investigate the effect of a brief mindfulness practice on mindfulness, empathy, compassion for others and

negative emotion. The main analysis did not find the predicted relationship between group and measures of: mindfulness, empathy, and compassion for others. The hypothesis for emotion regulation was partly supported: those high in negative affect at baseline showed significantly less negative affect at post-test if they were in the mindfulness group than the control group. This finding should be viewed in the light of having small numbers of participants in each subgroup, which can increase the risk of Type I and Type II errors. Unplanned analyses revealed that TMS scores were not correlated with the dependent variables. However prior practice of mindfulness did predict TMS scores, with those who had a weekly or higher practice scoring higher on the TMS, regardless of which practice they were given in the study. Repetition of the main analyses with prior practice as the grouping variable revealed no effect on compassion for others, or negative emotion. An effect nearing significance, with a medium effect size, was found in an unexpected direction for empathy: those with a regular practice scored slightly lower on the MASC-MC than those without. This should be viewed in the light of the increased risk of a Type I error with multiple unplanned comparisons.

Each outcome will be examined in relation to the literature, beginning with the TMS. The two studies that used very similar induction exercises and the TMS (Saunders et al., 2013; Garland et al., 2013) both found an effect of group. From examining normative data (Lau et al., 2006), the means for both groups in the current study were in the range expected from participants who had completed a mindfulness practice, suggesting that the control practice may have induced something similar to a mindful state in this sample. It is interesting that both Garland et al. (2013) and Saunders et al. (2013) used a sample of undergraduates, who are likely to have had little or no prior knowledge of mindfulness. All trainee therapists would have a good working knowledge of the concept of mindfulness, even without much prior practice, due to its dominance in current psychological literature.

Therefore, having completed the FFMQ-SF participants could have recognised that the study was about mindfulness, and so may have used the control exercise accordingly. The finding that prior experience of mindfulness was related to higher TMS scores could fit with this; if participants had been attempting to use the control exercise in a mindful way, those with more experience of mindfulness may have been more able to achieve this goal. Alternatively, thinking the study was about mindfulness could have led to response bias on the TMS.

Another explanation comes from an examination of the mind-wandering literature. It has been proposed that mind-wandering can be beneficial if the individual regulates the content of their mind-wandering to include productive future focused thoughts and reduced rumination; indeed mind-wandering of this kind can lead to states of relaxation, creativity and wellbeing (Smallwood & Andrews-Hanna, 2013). There could be characteristics of trainee therapists compared to undergraduates that allowed them to use the mind-wandering exercise in a constructive way; perhaps a greater self-reflectivity, for example.

The remainder of the main analyses need to be interpreted in the context of the lack of expected group differences on the TMS. Therefore, in terms of empathy and compassion for others, it is not certain that mindfulness did not have an effect, as it is not certain that one group was 'mindful' and the other was not. Some of the unplanned analysis could help to understand the relationship between mindfulness and these variables further. Firstly, we might expect those who reported themselves to have achieved a deeper state of mindfulness to have higher empathy or compassion for others, but this correlation was not found. Secondly, although those with more prior experience of mindfulness reported higher scores on the TMS, they did not have higher empathy or compassion for others; indeed the empathy scores were slightly lower compared to the low practice group (this was a non-significant finding, with a medium effect size). These two findings add some support to the idea that a brief MBI may not effect change on measures of empathy or compassion for others. This

should be viewed tentatively, as it is based on unplanned analysis in which spurious results are possible, and in which participants were no longer randomly allocated to group, so a number of factors could have been at play. There may also have been reliability and validity issues with the compassion for others measure, which was developed for this study. Previous studies have not found an effect of group on a performance based measure of empathy (Keefe, 1979; Pearl & Carlozzi, 1994), or an effect of an MBI on compassion for others (Brooker et al., 2013; Fortney et al., 2013). The only RCT to date to have found a change on performance-based measures of empathy and compassion for others had added emotion recognition training to the MBI (Kemeny et al., 2011). It has been suggested that extra components such as compassion-focused training may be required in order for empathy and compassion to increase, as mindfulness may allow the practitioner to step back from their own thoughts and feelings, but without compassion-focussed training they may not take the next step of connecting with another's thoughts and feelings (Kristeller & Johnson, 2005). More research is needed into this theory.

The lack of between-group differences on the TMS means it is not certain that it was mindfulness that led to the difference seen between groups in negative affect (for those high in baseline negative affect). This result also needs to be interpreted cautiously due to the low participant numbers in each subgroup. However, looking at the psychometric properties of the TMS could be helpful, as it has been questioned whether the TMS can pick up on the breadth and complexity of mindfulness, given its two-factor structure (Tanay & Bernstein, 2013). It could be that although the control exercise might have induced something close to a mindful state, there may have been subtle differences to the state induced in the mindfulness group, that were too slight to be picked up by the TMS. Perhaps, whilst both practices had the effect of causing a state of relaxation, it was only mindfulness that enabled re-perceiving, and that this ability to stand back and observe emotions became more helpful than relaxation,

only when negative emotion was high. This idea should be viewed tentatively, as the unplanned analysis found that those with high previous mindfulness practice did not show lower post-test negative affect, which might be expected if mindfulness had had the proposed effect. Therefore, due to the mixed findings found here, and the mixed findings in the existing literature (Ortner & Zelazo, 2012; Arch & Craske, 2006) more research is needed before conclusions can be drawn.

**Limitations.** This study was slightly underpowered to detect the hypothesized effects. Examining the effect sizes, this does not seem to have been a problem in the main analysis, as all non-significant findings had low effect sizes. Power may have been an issue in the unplanned analysis, particularly as the low and high practice groups were unequal in size.

In addition to the issues with measurement already mentioned, the IRI and the MASC-MC were not correlated and therefore do not seem to be measuring the same constructs, meaning that it is unlikely that baseline differences in empathy were effectively controlled for, and calls into question whether empathy was measured as an outcome. It can be questioned whether cognitive empathy is the most relevant measurement for trainee therapists, as it has been suggested that empathy as perceived by the client is most related to therapy outcome (Elliott et al., 2011). The administration of the MASC-MC was altered slightly, to include brief pauses to remind participants to return to the induced state, which could alter its reliability and validity. Although the negative PANAS was used to indicate how well emotion had been regulated, emotion regulation is a complex process that does not just involve reduction in negative affect (Chambers et al., 2013). Although one performance based measure was used, there was still a reliance on self-report measures, and it has been questioned whether people can accurately self-report on mindfulness (Grossman, 2011).

There are certain limits to the generalisability of the study. Firstly, this was a sample of trainee therapists, and processes might be different in experienced therapists and different

populations. Secondly, the nature of the brief intervention meant that it could not encompass the whole complexity of mindfulness, and we would not expect it to replicate the effect of having a regular practice for many years. It has also been suggested that mindfulness is depleted when it is extracted from its Buddhist context, and when it is not studied in depth (Grossman, 2011). Demographic characteristics such as age and gender could influence scores on mindfulness questionnaires (Baer, Samuel & Lykins, 2011), and there is evidence that empathy differs across gender, with women scoring higher than men (Davis, 1980). Therefore since this sample was largely female and with a mean age of 31 years, the findings may not be generalisable to a sample that was largely male, or to participants from a different age range.

**Implications for research.** It is important that future research in mindfulness uses active control groups. This study raises the question of whether the control group used was appropriate for this sample, and it may be that different active controls are needed for different participant groups, which could be looked into further. As control practices begin to more closely match mindfulness practices, measures of state mindfulness will need to become more sophisticated in order to detect subtle differences between groups. In order for this to be possible, the literature may need to reach more of a consensus on the definition of mindfulness. Future measures of mindfulness could also be performance-based (Garland & Gaylord, 2009).

Similarly, the definition of empathy needs further empirical exploration, particularly as the main measure used in the literature (the IRI) is based on theoretical rather than empirical analysis. Where possible, performance-based measures of empathy should be used and further validated. Measures of compassion for others are still in their infancy and need to be further developed and validated, particularly state measures. The construct of compassion for others could be more clearly defined, particularly looking at whether self-compassion is a

prerequisite for compassion for others, as Buddhist philosophy suggests (Neff & Pommier, 2013).

Future research in this area could usefully investigate the question of whether a compassion-meditation component is needed in addition to an MBI, for empathy and compassion for others to increase. This would not only have useful practical implications but would add to our theoretical understanding of whether mindfulness on its own (without an additional compassion component) encourages inward-focused attention to the detriment of other-focused attention.

It would be interesting to look at whether levels of baseline negative emotion effect how useful a mindfulness practice is, as it may be more helpful for those with higher negative affect at the outset. It would also be interesting to use a relaxation exercise as the control group to test the hypothesis coming out of this study that mindfulness may be more helpful than relaxation only when negative affect is high, as it is then that re-perceiving becomes most important.

Another question is whether different interpersonal outcomes change at different times, and at different doses of mindfulness, for example, it could be possible that emotion regulation alters first, which after some length of practice, could lead to changes in empathy or compassion for others. If there is a dose effect, discerning the dose needed for changes in different variables would be helpful for service providers in knowing how long an intervention to provide.

**Clinical implications.** It is difficult to draw conclusions about the usefulness of a brief mindfulness practice for empathy, negative affect and compassion for others for trainee therapists based on this study. It is possible that doing a brief mindfulness exercise prior to seeing a client could help a trainee, who had high negative affect, to regulate this. However, more research is needed in order to be confident in this conclusion. Looking at the findings of

this study alongside previous research in the area, it may be that, if the aim is to enhance common factors in trainee therapists, thought could be given to what extra components could be added to an MBI, such as compassion meditation or emotion recognition training.

### **Conclusions**

This study has attempted to investigate variables that are important to the therapeutic encounter, that previous MBIs have found evidence for, in an experimentally rigorous way. Due to a lack of differences between groups in post-test state mindfulness, it is difficult to draw conclusions about whether mindfulness is related or not to the outcomes of interest. It is possible that mindfulness may be more helpful than mind-wandering for those high in baseline negative affect, in terms of helping to regulate this, and it may be that empathy and compassion for others take longer to change, or additional compassion-meditation is needed. However, limitations of the study limit confidence in these conclusions, which should be viewed as tentative. The study highlights the importance of developing well-matched control groups for mindfulness, and the complexity in its measurement, which future research should take forwards.



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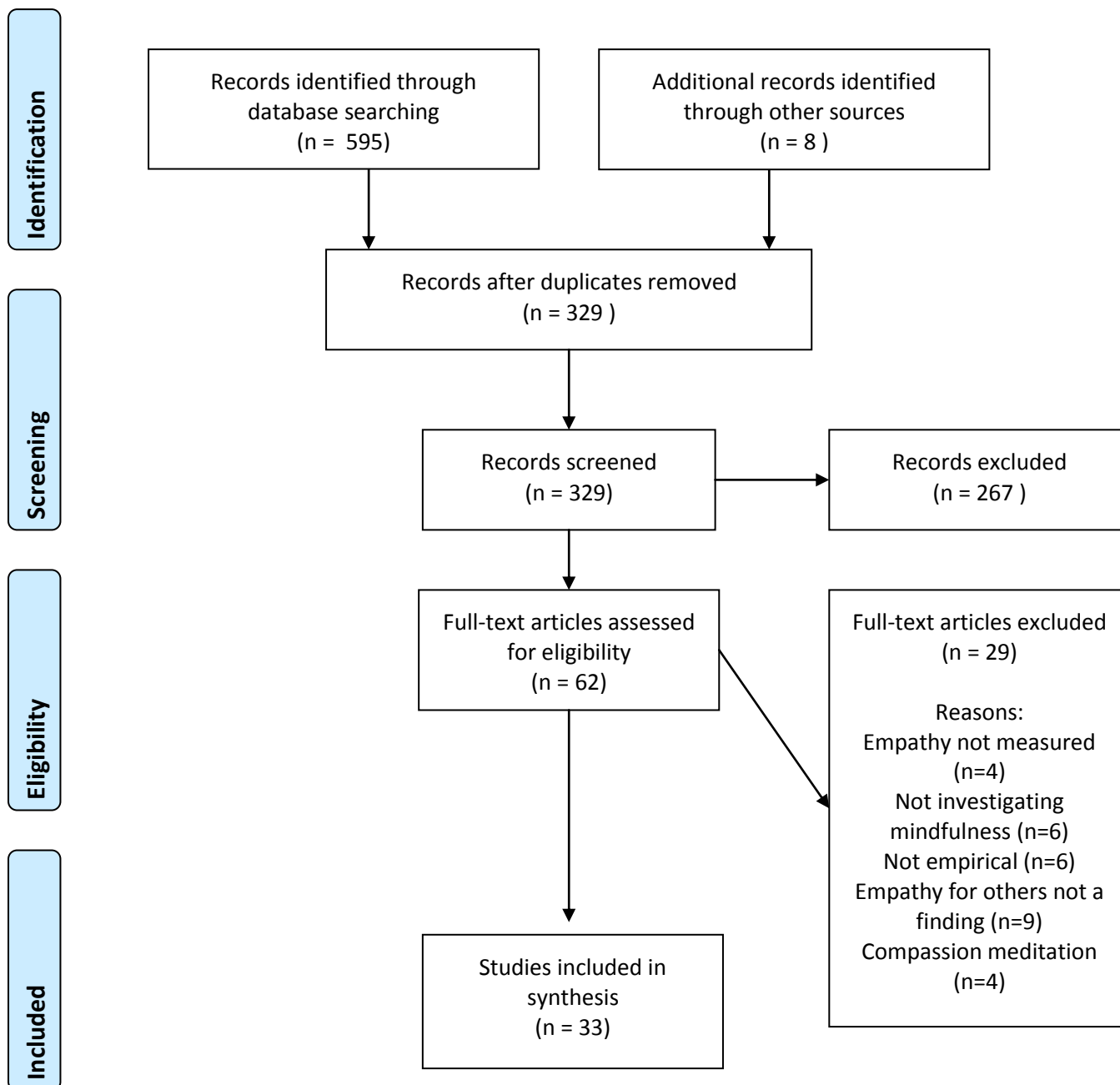
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**Section C: Appendices of supporting material**

**Appendix 1:Flow diagram showing search strategy**



## Appendix 2: Summary of all studies in literature review

Table A1.

Summary of all Randomized Controlled Trials in literature review

Paper	Participant group, N	Groups and intervention	Mindfulness/ empathy measures and when taken	Relevant findings	Comments, critique
<b>Kemeny et al., 2011</b>	Female school teachers, 76	Mindfulness: 8 week training programme in meditation and emotion skills (included education on emotion recognition in self and others). Control: Waiting list	Empathy: Micro Expression Training Tool (METT) Pre, post, 5 month follow up	Compared to control group, mindfulness group had increased mindfulness, increased abilities on METT.	No baseline measure of empathy  Programme included education on emotion recognition Small sample
<b>Shapiro, Brown, Thoresen &amp; Plante, 2011</b>	Under-graduates, 30	Mindfulness: 8 week MBSR Control: Waiting list	Mindfulness: MAAS Empathy: IRI (all subscales combined) Pre, post, 2 month and 12 month follow-up	MBSR group significantly increased mindfulness and IRI scores compared to controls, at 2 month and 12 month follow up (post-treatment data not presented) . Those higher in trait mindfulness at start of MBSR showed larger increase in empathy	Post-test stats not presented Self-report
<b>Shapiro, Schwartz, &amp; Bonner, 1998</b>	Medical and pre-medical students, 78	Mindfulness: 7 weeks Stress Reduction and Relaxation Program. Programme was adapted to include exercises to increase empathy and listening skills. Control: Waiting list	Mindfulness: None Empathy: Empathy Construct Rating Scale (adapted for this study) Pre and post.	Significant main effect of group on all measures. Result was replicated when the control group went through the intervention.	Control group was inactive and during time of stress – was it social support component that helped? Mindfulness not measured Unusual outcome measures
<b>Pearl &amp; Carlozzi, 1994</b>	University students and staff, 50	8 weeks “Clinically standard meditation”. Control: no intervention	Mindfulness: Measured anxiety Empathy: Affective sensitivity scale Pre and post.	Significant differences for anxiety but not for empathy	Brief report Old study, no measure of mindfulness. No details on meditation exercise
<b>Keefe, 1979</b>	Social work masters students, 58	Mindfulness: 30min daily meditation for 3 weeks. Therapy skills course: 3hrs per week of teaching on therapeutic communication Control: No intervention	Mindfulness: Level of meditative experience obtained Empathy: Affective Sensitivity Scale. Pre and post	Neither meditation or therapy skills group improved significantly more than the control group. Meditation level attained did correlate with increase in empathy scores.	Good attempts at measuring meditation and empathy, although meditation measure unvalidated. Practice effects meant all groups increased.

Table B2.

Summary of all studies using a control group (not randomised)

<b>Paper</b>	<b>Participant group, N</b>	<b>Groups and intervention</b>	<b>Mindfulness/ empathy measures and when taken</b>	<b>Relevant findings</b>	<b>Comments, critique</b>
<b>Melloni et al., 2013</b>	Unclear, 29	Long term meditators (Completed MBSR and continued practice for >1 yr, M=4.5 years) Short term meditators (attended MBSR group) Control: On waiting list for MBSR	Mindfulness: None  Empathy: IRI, Reading the Mind in the Eyes Task (RMET), Emotion Recognition Task	IRI: LT meditators and ST meditators scored lower on PD than controls. No other significant differences RMET: No significant differences between groups Emotion Recognition: No overall group differences	LT meditators don't seem very LT – what is rationale for duration? Small N (<10 per group) Subcategories examined: multiple tests
<b>O'Connor, Berry, Stiver &amp; Rangan, 2012</b>	Buddhist meditators (98) Control: Non meditators (438)		IRI (PT, EC, PD subscales)	Meditators had lower PD, no differences in PT or EC.	Confound of religious beliefs and choice to meditate
<b>Sahdra et al., 2011</b>	People interested in Buddhist meditation retreat, 59	Mindfulness: 3 month long retreat involving 6 hr daily meditation Control: Waiting list	FFMQ IRI (EC+PT+PD (reverse scored)) Pre, mid, post	Significant changes at post-test compared to control. Control made significant changes when went through retreat.	Control group inactive. IRI as combined subscales. Sample already meditated. LKM component.
<b>Barbosa, Raymond, Zlotnick, Wilk, Tommey &amp; Mitchell, 2013</b>	Graduate healthcare students, 28	Mindfulness: MBSR Control: Waiting list	Empathy: Jefferson scale of physician empathy Pre, post and 3 week follow up	Significant difference between groups at post-test, however not maintained at 3 week follow up. All groups had decreased in empathy from start. Due to exams the next week?	Changes not sustained in face of stress Inactive control
<b>Lesh, 1970</b>	Counselling psychology trainees, 39	Zazen meditation group (30 min per day for 4 weeks), control group (no activity)	Mindfulness: Level of meditative experience obtained Empathy: Affective Sensitivity Scale Pre and post.	Meditation group showed sig greater increase in empathy scores than control group	Old study, no measure of mindfulness.

Table C3.

Summary of all pre-post studies used in literature review

<b>Paper</b>	<b>Participant group, N</b>	<b>Intervention</b>	<b>Mindfulness/ empathy measures and when taken</b>	<b>Relevant findings</b>	<b>Comments, critique</b>
<b>Bazarko, Cate, Azorca &amp; Kreitzer, 2013</b>	Nurses, 36	Telephone MBSR: Involved two in-person retreats and eight 1.5 hr weekly sessions delivered in group telephone format	Jefferson Scale of Physician Empathy Pre, post	Significant increase in empathy post-test; maintained at 4 months	Self selecting – highly motivated
<b>Bond et al., 2013</b>	Medical students, 24	Embodied Health course: 11 weeks elective course, 1.5 hr classes. Teaching on neuroscience of meditation, also meditation, yoga and breathing exercises.	Jefferson Scale of Physician Empathy Pre, post	No significant increase in empathy.	Small N
<b>Harwani, Motz, Graves, Amri, Harazduk &amp; Haramati, 2013</b>	First year medical students, 118	11 week mind-body medicine course: Includes mindfulness meditation, autogenic training, guided imagery, movement and writing exercises	IRI (separate subscales) Pre, post	Significant decrease in PD, significant increase in EC.	Brief report therefore no further details given
<b>Hopkins &amp; Proeve, 2013</b>	Trainee clinical psychologists, 11	MBCT	IRI (separate subscales) Qualitative data Pre, post and 2 months follow up	Decrease in Fantasy subscale. No other changes in empathy. Increase in mindfulness. Themes included coping with stress, and increased emotional awareness during therapy.	Small N Fantasy scale thought to lack validity.
<b>Rimes &amp; Wingrove, 2011</b>	Trainee clinical psychologists, 20	MBCT: parts specific to depression were altered to focus on stress	IRI (EC only) FFMQ Qualitative data Pre, post	No significant change in empathy. Content analysis: 85% said had impact on clinical work.	
<b>Birnie, Speca &amp; Carlson, 2010</b>	Community sample, 41	8 week MBSR	IRI (separate subscales) Pre, post	Significant changes on PT and PD subscales. EC not affected	
<b>Krasner, Epstein, Beckman, Suchman, Chapman, Mooney &amp; Quill, 2009</b>	Primary care physicians, 70	An intensive educational program in mindfulness, communication, and self-awareness (8 weeks)	Jefferson Scale of Physician Empathy Qualitative data (Beckman et al. 2012)	Significant improvement in empathy	Intervention not only about mindfulness No control group Self-report

<b>Galantino, Baime, Maguire, Szaparay, Farrar, 2005</b>	Health care professionals, 84	Adapted MBSR (8 weeks)	Pre and post IRI (individual subscales) Pre, post	No significant difference on any of IRI subscales	No measure of mindfulness
<b>Beddoe &amp; Murphy, 2004</b>	Student nurses, 16	MBSR	IRI (individual subscales) Pre, post	No significant difference on any of IRI subscales	Low N

Table D4.

Summary of all studies using a correlational design in literature review

<b>Paper</b>	<b>Participant group, N</b>	<b>Mindfulness/ empathy measures</b>	<b>Relevant findings</b>	<b>Comments, critique</b>
<b>Keane, 2013</b>	Psychotherapists, 40	FFMQ IRI (four subscales and overall scale using three subscales)	All subscales of FFMQ correlated with PT. Most correlated negatively with PD. Only one (Observe) correlated with EC.	Low N Multiple correlations between subscales examined
<b>Thomas &amp; Otis, 2010</b>	Social workers, 171	FFMQ IRI (all subscales)	Increased mindfulness associated with increased PT, decreased F, decreased PD. No significant correlation with EC.	Issues with F subscale
<b>Greason &amp; Cashwell, 2009</b>	Counselling students, 179	FFMQ IRI (PT plus EC)	Mindfulness correlated with EC plus PT.	Summing of subscales
<b>Dekeyser, Raes, Leijssen, Leysen, Dewulf, 2008</b>	Psychology undergraduates and parents, 359	KIMS IRI (PT plus EC plus F; PD separate)	PD negatively correlated with the following subscales: describe, act with awareness, accept without judgement. Empathy (PT+EC+F) positively correlated with the observe subscale but not the others.	Summing of subscales Multiple correlations between subscales examined
<b>Wachs &amp; Cordova, 2007</b>	29 married couples	IRI (PT, EC, PD (reverse scored)) MAAS Scores were collapsed for husbands and wives by taking the mean	Couple-level mindfulness positively correlated with EC, PT, PD.	MAAS doesn't capture all aspects of Mindfulness – focus on bare attention Summing to achieve couple level score
<b>Beitel, Ferrer &amp; Cecero, 2005</b>	Undergraduates, 103	MAAS IRI	MAAS positively correlated with EC and PT, and negatively correlated with PD	

Summary of qualitative studies:

Schure, Christopher and Christopher, 2011, Trainee counsellors taught 15 week programme including MBSR and mindful movement eg yoga and Qigong. Participants reported interpersonal changes including an increase in empathy, and being more attentive to the therapy process.

Gokhan, Meehan and Peters, 2010. Psychology undergraduates on clinical placements; one group offered 12 week mindfulness training module, other served as control for mindfulness measures. Overall mindfulness went up on self-report measures. A reported benefit of a mindfulness component to a training course was empathy. Suggestion that this could occur through heightened attention to personal reactions, and greater awareness of the importance of ethics.

Cohen-Katz et al., 2005: MBSR with nurses. Greater relaxation and self-care and improvement in work and family relationships were among reported benefits.

Bailie , Kuyken and Sonnenberg, 2011 Parents completing MBCT course report greater empathy in relationship with their children.

Berneudez et al., 2013: Experiences of MBSR for women with PTSD and with history of intimate partner violence.

(Keane, 2013): Psychotherapists report increased empathy with mindfulness practice

Andersson, King and Lalande, 2010: Using a mindfulness approach in supervision increased empathy towards clients

Bruce and Davis, 2005. Use of Zen meditation by staff in hospice care home. Participants felt that an understanding of the independent nature of beings was integral in empathy and compassion.

## Appendix 3: CONSORT ratings for Randomized Controlled Trials

Table E5.

## Ratings for all Randomised Controlled Trials according to CONSORT criteria

Authors	Identification as a randomised trial in the title	Structured summary of trial design, methods, results, and conclusions	Scientific background and explanation of rationale	Specific objectives or hypotheses	Description of trial design (such as parallel, factorial) including allocation ratio	Important changes to methods after trial commencement (such as eligibility criteria), with reasons	Eligibility criteria for participants	Settings and locations where the data were collected	The interventions for each group with sufficient details to allow replication, including how and when they were actually administered	Completely defined pre-specified primary and secondary outcome measures, including how and when they were assessed
<b>Kemeny et al., 2011</b>	N	Y (no headings)	y	Y	Y	None stated	Y	y	y	y
<b>Shapiro, Brown, Thoresen &amp; Plante, 2011</b>	Y	Y (no headings)	y	Y	Y	N – why was the other meditation group not analysed?	N – or were there no limitations?	y	N (but standardised)	y
<b>Shapiro, Schwartz &amp; Bonner, 1998</b>	n	Y (no headings)	y	Y	y	None stated	Y	y	y	y
<b>Pearl &amp; Carlozzi, 1994</b>	n	n	n	N	Y	N	Y	n	n	y
<b>Keefe, 1979</b>	n	Y (no headings)	y	Y	Y	None stated	Y	n	n	y

<b>Authors (cont.)</b>	Any changes to trial outcomes after the trial commenced, with reasons	How sample size was determined	When applicable, explanation of any interim analyses and stopping guidelines	Method used to generate the random allocation sequence	Type of randomisation; details of any restriction (such as blocking and block size)	Mechanism used to implement the random allocation sequence (such as sequentially numbered containers), describing any steps taken to conceal the sequence until interventions were assigned	Who generated the random allocation sequence, who enrolled participants, and who assigned participants to interventions	If done, who was blinded after assignment to interventions (for example, participants, care providers, those assessing outcomes) and how	If relevant, description of the similarity of interventions
<b>Kemeny et al., 2011</b>	n/a	y	n/a	y	Y	N	n	n	n/a
<b>Shapiro, Brown, Thoresen &amp; Plante, 2011</b>	n/a	n	n/a	n	N	N	n	n	n/a
<b>Shapiro, Schwartz &amp; Bonner, 1998</b>	n/a	n	n/a	n	Y - matched	N	n	Y - experimenters	n/a
<b>Pearl &amp; Carozzi, 1994</b>	n	n	n	n	N	N	n	n	n/a
<b>Keefe, 1979</b>	n/a	n	n/a	y	N	N	n	Y - judges	n/a



<b>Authors (cont).</b>	Statistical methods used to compare groups for primary and secondary outcomes	Methods for additional analyses, such as subgroup analyses and adjusted analyses	For each group, the numbers of participants who were randomly assigned, received intended treatment, and were analysed for the primary outcome	For each group, losses and exclusions after randomisation, together with reasons	Dates defining the periods of recruitment and follow-up	Why the trial ended or was stopped	A table showing baseline demographic and clinical characteristics for each group	For each group, number of participants (denominator) included in each analysis and whether the analysis was by original assigned groups	For each primary and secondary outcome, results for each group, and the estimated effect size and its precision (such as 95% confidence interval)
<b>Kemeny et al., 2011</b>	y	y	Y – no flow chart	N – reasons not given	N	n	n	y	N (mean and sd given)
<b>Shapiro, Brown, Thoresen &amp; Plante, 2011</b>	y	y	Y – no flow chart	y	N	n	n	y	Yes but no CI
<b>Shapiro, Schwartz &amp; Bonner, 1998</b>	y	y	Y – no flow chart	y	N	n	n	y	N (mean and sd given)
<b>Pearl &amp; Carozzi, 1994</b>	y	n	Y – no flow chart	n	N	n	n	y	N (mean and sd given)
<b>Keefe, 1979</b>	Y	y	n- no drop-outs identified	n	n	n	n	y	Y – no CIs

<b>Authors (cont.)</b>	For binary outcomes, presentation of both absolute and relative effect sizes is recommended	Results of any other analyses performed, including subgroup analyses and adjusted analyses, distinguishing pre-specified from exploratory	All important harms or unintended effects in each group	Trial limitations, addressing sources of potential bias, imprecision, and, if relevant, multiplicity of analyses	Generalisability (external validity, applicability) of the trial findings	Interpretation consistent with results, balancing benefits and harms, and considering other relevant evidence	Registration number and name of trial registry	Where the full trial protocol can be accessed, if available	Sources of funding and other support (such as supply of drugs), role of funders
<b>Kemeny et al., 2011</b>	n/a	y	n/a	Y	y	y	n	y	y
<b>Shapiro, Brown, Thoresen &amp; Plante, 2011</b>	n/a	y	n/a	Y	y	y	n	y	n
<b>Shapiro, Schwartz &amp; Bonner, 1998</b>	n/a	y	n/a	Y	y	y	n	n	n
<b>Pearl &amp; Carlozzi, 1994</b>	n/a	n/a	n/a	N	n	n	n	n	n
<b>Keefe, 1979</b>	n/a	y	n/a	N	n	n	n	n	n

## Appendix 4: CASP ratings for qualitative papers

Table F6.

Ratings for all qualitative studies according to CASP criteria

Authors	Was there a clear statement of the aims of the research?	Is a qualitative methodology appropriate?	Was the research design appropriate to address the aims of the research?	Was the recruitment strategy appropriate to the aims of the research?	Were the data collected in a way that addressed the research issue?	Has the relationship between researcher and participants been adequately considered?	Have ethical issues been taken into consideration?	Was the data analysis sufficiently rigorous?	Is there a clear statement of findings?	How valuable is the research?
<b>Cohen-Katz et al., 2005</b>	y	y	Y – good use of mixed methods	Y	Y	N	y	Y – good use of data. Description of changes to coding book	Little mention of contradictory findings/ explanations	Useful ideas around how empathy may have increased
<b>Anderson, King &amp; Llande, 2010</b>	y	y	y	Y	Questions asked are not made clear	Y – issue of being supervisor discussed	y	Little reflection on impact of own position	Y – good attempt at triangulation	2-chair technique is likely to affect empathy
<b>Bruce &amp; Davis, 2005</b>	y	y	y	Y – included carers and patients. Formed relationships to help recruitment	Questions asked are not clear	n- influence of own role not considered: clear interest in Mindfulness	y	n- unclear how it was analysed, lack of contradictory data, and not enough presented	Attempts at reliability – e.g. Checking with researchers. But – not balanced discussion	Immersed in the hospice
<b>Schure, Christopher &amp; Christopher, 2008</b>	y	y	y	Y	Y	Y – issue of assignment discussed	y	Little reflection on impact of own position	No mention of contradictory findings.	

<b>Keane 2013</b>	y	Y	y	Y – participants selected for follow up interview to get a range of perspectives	Y	Challenges may have been under-reported	y	y	Y – only paper to explore challenges of mindfulness	Useful to pick up on challenges
<b>Bailie, Kuyken &amp; Sonneberg, 2011</b>	y	Y	y	Y – all participants used	Some detail of questions given but more on request	Y – good reflection	y	Y: inter-rater reliability, contradictory data discussed	Y – limitations discussed	Useful as not much research on MBCT and relationships
<b>Gokhan, Meehan &amp; Peters, 2010</b>	Y	Y – although their question implies quantitative could have been helpful alongside.	y	Unclear why one group chosen over the other	Clear on questions asked, but not on why these strategies used	N – unclear who was interviewing/ looking at journals – could have biased students if markers	Not mentioned explicitly. Did the control group get to take part after?	Not enough detail about analysis. Own position not discussed.	Clear statement. No alternative explanations / credibility for data discussed.	Suggests some paths through which mindfulness may increase empathy; but given recent date, could have advanced more.
<b>Bermeudez et al., 2013</b>	Y	Y	y	Y	Y	N	?	y	No alternative explanations	Useful and unusual population to examine

**Appendix 5: Journal's notes for contributors: Mindfulness**

This has been removed from the electronic copy

**Appendix 6: Copies of measures**

These have been removed from the electronic copy

### Appendix 7: Reliability of Movie Compassion measure

Table G7.

*Pearson correlation between test item and total test score, and Chronbach's alpha if item were to be deleted, for Movie Compassion measure*

<u>Test item</u>	<u>Correlation with total test score</u>	<u>Alpha if item deleted</u>
Compassion for Sandra	.49*	.41
Compassion for Michael	.51*	.46
Compassion for Betty	.75*	.05
Compassion for Cliff	.62*	.30

\* indicates significant at  $p < .001$

**Appendix 8: Ethics approval letters**

These have been removed from the electronic copy



Appendix 9: Recruitment materials

Advertising poster and flier

**Are you a trainee Clinical/ Counselling Psychologist  
or trainee CBT therapist?**

**Are you interested in how we develop  
the **therapeutic relationship**?**

**And in **new and simple ways** we could  
improve this?**

**An exciting new **research** project  
needs your help...**

**“The impact of attention on  
therapeutic skills in trainee  
therapists”**

**Participation will involve no more than  
1 hour 40 minutes of your time  
and **will take place at your University****

**You can be entered into a prize draw  
**to win a £50 voucher****

**Please contact: Emma Justice  
[elj17@canterbury.ac.uk](mailto:elj17@canterbury.ac.uk)**

**Or I will be around after lectures if you wish to sign up**

## Information sheet



Salomons Campus at Tunbridge Wells

### **Information about the research: “The impact of attention on therapeutic skills in trainee therapists”**

My name is Emma Justice and I am a trainee clinical psychologist at Canterbury Christ Church University. I would like to invite you to take part in a research study.

#### **What is the purpose of the study?**

The aim of this study is to identify whether how we direct our attention affects our work with clients.

#### **Why have I been invited?**

You have been invited because you are on a therapy training course. Approximately 50 trainee therapists will be involved in this study.

#### **Do I have to take part?**

It is up to you to decide to join the study. If you agree to take part, I will then ask you to sign a consent form. You are free to withdraw at any time, without giving a reason. This would not have any impact on your training.

#### **What will happen to me if I take part?**

The research will take up to 1 hour 40 minutes and will take place at your University. You will be randomly assigned to one of 2 groups. When you arrive you will be asked to fill out some brief questionnaires. You will then spend 15 minutes sitting silently and following instructions about how and where to place your attention. Once the 15 minute exercise has finished you will be asked to watch a 15 minute film, which will be stopped at various intervals and multiple choice questions will be asked. You will have an answer sheet to write your answers on. Once the film has finished you will be asked to fill out some more brief questionnaires.

We will be conducting the research in groups due to time constraints, so you will be doing the experiment in a room with other people. However you will carry out the experiment by yourself and it will not involve interacting with the other people.

#### **Expenses and payments**

If you have had to travel somewhere specifically to take part then your expenses will be refunded up to £10. If you take part you will have the option to enter into a prize draw to win a £50 voucher.

#### **What are the possible disadvantages and risks of taking part**

Some people may find it uncomfortable to sit in silence for 15 minutes and follow guidance regarding the focus of their attention. This can sometimes bring into awareness difficult emotions or uncomfortable physical sensations.

**What are the possible benefits of taking part?**

You may experience what it is like to focus your attention in different ways, and this could be something you find beneficial or feel you want to take away with you. The outcome of the study could potentially help us to identify ways for trainee therapists to enhance the way they relate to their clients.

**What if there is a problem?**

Any complaint about the way you have been dealt with during the study or any possible harm you might suffer will be addressed. The detailed information on this is given in Part 2.

**Will my taking part in the study be kept confidential?**

Yes. We will follow ethical and legal practice and all information about you will be handled in confidence.

**What will happen if I don't want to carry on with the study?**

You are able to withdraw at any time without explanation. If you withdraw from the study, we would like to use the data collected up to your withdrawal. However, you retain the right to decide whether we can do this or not.

**Complaints**

If you have a concern about any aspect of this study, you should ask to speak to me and I will do my best to answer your questions. I will be present on the day throughout the session. If you remain unhappy and wish to complain formally, you can do this through following the Canterbury Christ Church University complaints procedure, or by contacting Paul Camic, Research Director, paul.camic@canterbury.ac.uk.

**Will my taking part in this study be kept confidential?**

Your data will be collected on written record sheets which will be stored in a locked cabinet. The data will be anonymous and will only be coded to tell me which of the two groups you were in. The data may be viewed by people connected with the research e.g. the lead supervisor, but it will always be anonymized before this happens.

There are some limits to confidentiality. Regarding the open-ended questions at the end of the session, anonymized quotations of what you have written may be used in the write-up of this experiment. If in this section there is something that makes me feel concerned that you are practicing in a way that breaches professional conduct, I will have to pass this information on to my supervisor and to relevant professionals.

**What will happen to the results of the research study?**

It is not possible to give feedback on individual performance on questionnaires. Questionnaire data collected in research settings is not suitable for individual use; in addition, the data will be anonymized so it would not be possible to identify which results are yours.

You may wish to access summary group data or to know the outcome of the study. If this is the case you will be asked to leave your email address with the researcher at the end of the study and a summary will be sent to you once the research is complete.

**Who is organising and funding the research?**

Canterbury Christ Church University is organising and funding the research.

**Who has reviewed the study?**

This study has been reviewed and given favourable opinion by [REDACTED]

and [REDACTED].

**Further information and contact details**

If you would like any information about this study or if you have any concerns please contact me on [elj17@canterbury.ac.uk](mailto:elj17@canterbury.ac.uk).

*Emma Justice  
Trainee Clinical Psychologist  
January 2013*

**Consent form**

Salomons Campus at Tunbridge Wells

**CONSENT FORM**Title of Project: **The impact of attention on therapeutic skills in trainee therapists**

Name of Researcher: Emma Justice

Please initial box

1. I confirm that I have read and understand the information sheet dated January 2013 for the above study. I have had the opportunity to consider the information, ask questions and have had these answered satisfactorily.

2. I understand that my participation is voluntary and that I am free to withdraw at any time without giving any reason, without my rights being affected.

3. I understand that data collected during the study may be looked at by the supervisors, Dr Fergal Jones and Dr Clara Strauss. I give permission for these individuals to have access to my data.

4. I agree that anonymous quotes from the open-ended section of the questionnaire may be used in published reports of the study findings

5. I agree to take part in the above study

Name of Participant \_\_\_\_\_ Date \_\_\_\_\_

Signature \_\_\_\_\_

Name of Person taking consent \_\_\_\_\_ Date \_\_\_\_\_

Signature \_\_\_\_\_

## Recruitment emails

### 1. Advertising email

Dear all,

I'm a third year Salomons trainee and I just wanted to take a moment of your time to tell you a bit about my research project, which involves trainee therapists.

I'm looking at how the way we direct our attention affects the relationship we form with clients. Taking part in the project would take no more than 1hour40min and would involve: Filling in some questionnaires, doing a short attentional practice, and watching a movie. If you choose to take part, you can be entered into a prize draw to win a £50 voucher, and I will also let you know about the project's findings, which hopefully would feel relevant for you. I have attached the information sheet and consent form which contain more information.

I was thinking of running the project at [University] on [dates and times] but am equally happy to fit in with a time that you suggest. I can reimburse travel expenses incurred specifically for the project up to the cost of £10.

If you are interested, please email me on [elj17@canterbury.ac.uk](mailto:elj17@canterbury.ac.uk) to register your interest, and please also get in touch if you would like a bit more information or to ask any questions.

Thank you very much for your interest and your time  
Warm wishes  
Emma

Emma Justice  
Trainee Clinical Psychologist  
Canterbury Christ Church University, Salomons campus

### 2. Email following sign-up

Dear [name],

Thank you very much for signing up for my research project, your interest is much appreciated!

The project is about how the way we direct our attention affects the relationships we form with clients. It involves completing some questionnaires, watching a movie, and doing a brief attentional practice. There is a prize draw to win a £50 shopping voucher for those who take part.

I have attached the information sheet and consent form, if you would like to look over them.

I was wondering whether [date] at [time] would suit you as a date to take part? If this

date doesn't suit, please let me know when would be better and I will be happy to adapt.

Thanks very much for your time and interest  
Warm wishes  
Emma

Emma Justice  
Trainee Clinical Psychologist  
Canterbury Christ Church University, Salomons campus

### 3. Email if no response received

Dear [name],

You expressed an interest in participating in a research project about how the way we direct our attention can affect the relationships we form with clients.

I have not heard back from you confirming a date and time, but I just wanted to let you know that the project is going ahead on [date]. If you are able to take part, you will be entered into a prize draw to win a £50 voucher, as well as contributing to clinically relevant research, and you can opt to get feedback about the findings!

I'm sure that things are busy, but if you find that you will be able to come along do let me know. Your interest is much appreciated.

Thanks for your time  
Warm wishes  
Emma

Emma Justice  
Trainee Clinical Psychologist  
Canterbury Christ Church University, Salomons campus

### 4. Reminder to those confirmed

Dear [names],

I'm looking forward to meeting you all for my research project going ahead on [date]. See you in room [number] at [time]. Once again I really appreciate your taking the time to participate.

I'm able to reimburse travel expenses you incurred specifically for the project. If this applies to you please let me know before-hand so I can have the right money for you on the day.

I've attached the information sheet and consent form in case you want to remind yourself what the project involves.

If you find that you can no longer attend it would really help with my time planning if you could let me know before the day.

Many thanks again for your time,  
Warm wishes,  
Emma

Emma Justice  
Trainee Clinical Psychologist  
Canterbury Christ Church University, Salomons campus

**Debrief**

DEBRIEF

Thank you for taking part in this study. The aim of the study is to determine whether mindfulness can improve empathy, compassion, distress tolerance and memory in trainee therapists.

There were two groups in this study, a mindfulness group and a control group. You were in the (mindfulness/ control) group. This meant that you (practiced a 15 minute mindfulness exercise/ practiced a 15 minute “mind-wandering” exercise, which is considered to be an appropriate contrast to mindfulness). You then watched a movie and answered questions about it, which is considered to be a measure of empathy. The other questionnaires you filled out are related to the variables of compassion, empathy, distress tolerance, memory and mindfulness.

If you would like more information on how to access mindfulness materials please contact Emma Justice on [elj17@canterbury.ac.uk](mailto:elj17@canterbury.ac.uk).

Since we are still in the process of collecting data, and will be using trainees from your and other cohorts, we would appreciate it if you could refrain from discussing your experiences here with other trainees.

If you have any questions about the project or would like to discuss it with someone please contact Emma Justice on [elj17@canterbury.ac.uk](mailto:elj17@canterbury.ac.uk).

If you would like to hear about the results of this project please leave your email address here:

.....

If you would like to be entered into the prize draw to win a £50 shopping voucher please leave your email address here:

.....



**Appendix 10: Induction instructions**

These have been removed from the electronic copy

## Appendix 11: Examination of assumptions

### Main analysis

#### Assumption of normality and homogeneity of variance

The assumption of normality was checked by examining histograms of the main outcome measures, across the two groups.

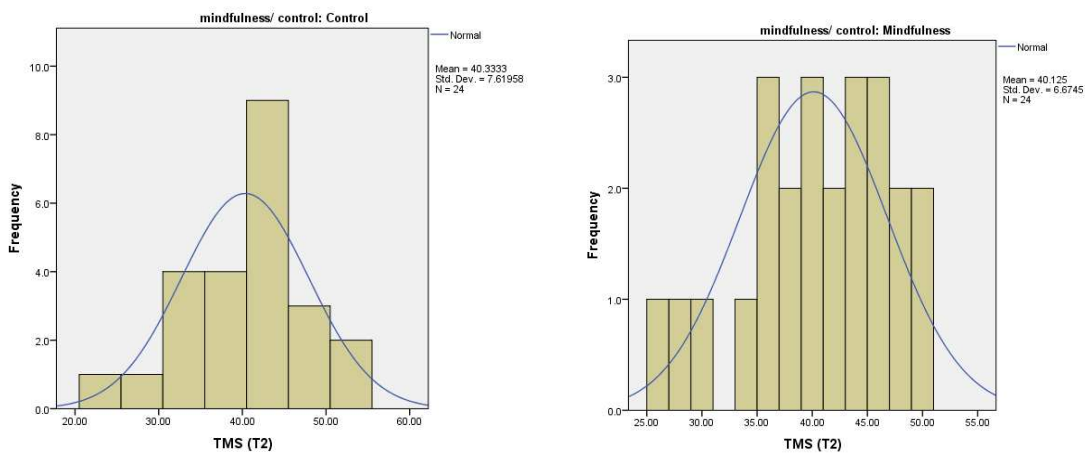


Figure A1. Histograms showing distribution of Toronto Mindfulness Scale (TMS) scores for both control and mindfulness groups

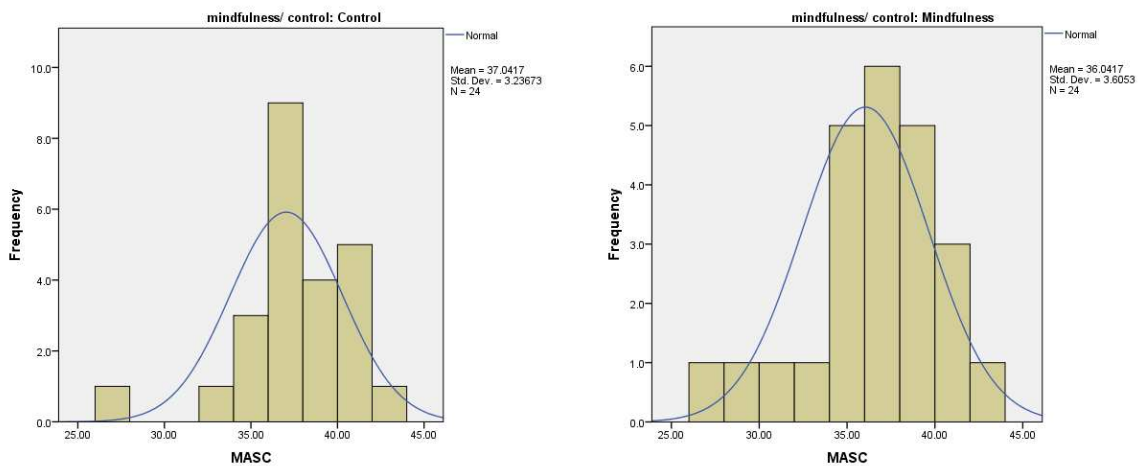


Figure B2. Histograms showing distribution of Movie for Assessment of Social Cognition (MASC) scores for both control and mindfulness groups

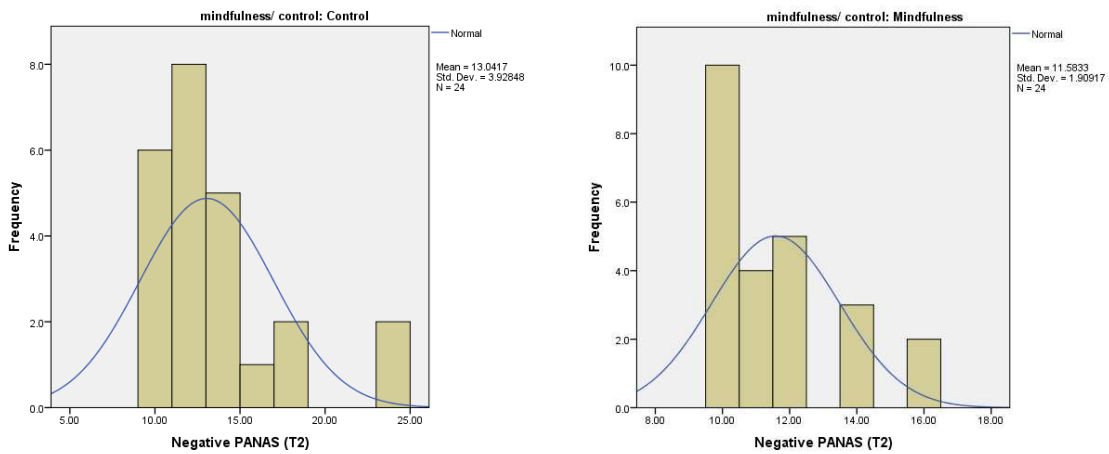


Figure C3. Histograms showing distribution of post-test Positive and Negative Affect Scale (PANAS; Negative scale) for both control and mindfulness groups

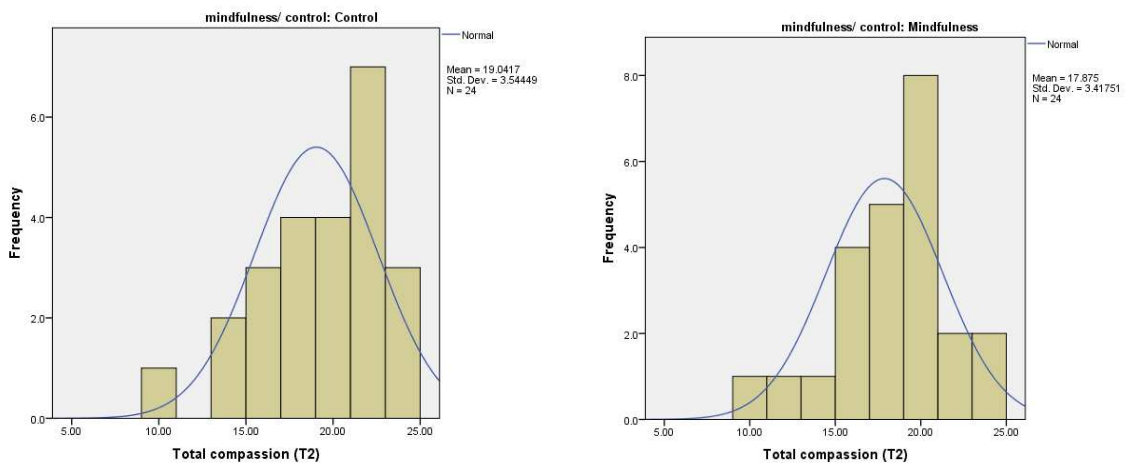


Figure D4. Histograms showing distribution of Movie Compassion measure scores for both control and mindfulness groups

Outliers were checked for by calculating z scores, and taking a z-score of 3.29. to indicate an outlier (Field, 2009), none were detected.

Homogeneity of variance was examined using Levene’s test, illustrated in Table H8.

Table H8

*Levene’s statistic showing differences between variances for mindfulness and control groups, for each variable*

Variable	Levene statistic	df	p
TMS	.388	1, 46	.536
MASC	.545	1, 46	.464
PANAS (negative subscale)	4.800	1, 46	.034*
Movie Compassion measure	.144	1, 46	.706

Note. \* indicates significant at  $p < .05$

The histograms indicate that there are some deviations from normality. Table H8 shows that all measures met assumptions of homogeneity of variance apart from the negative PANAS, for which Levene's test was significant. When group sizes are equal, analysis of variance is robust to deviations from normality and homogeneity of variance (Field, 2009). Given this, and that transformations could cause problems in the majority of the variables that did meet assumptions (Field, 2009), it was decided that transformations would not be applied.

### **Assumption of covariate independent to treatment effect**

The assumption that the covariate was independent from the treatment effect was tested by conducting two-tailed, independent samples t-tests, to examine whether there were differences between groups on the baseline measures (see Table I9). All tests were not significant ( $p > .05$ ), indicating that this assumption was met.

Table I9.

Differences between the mindfulness and control groups for each covariate

<u>Variable</u>	<u>T</u>	<u>df</u>	<u>p</u>
Five Facet Mindfulness Questionnaire	-1.442	46	.156
Perspective Taking (Interpersonal Reactivity Index)	.691	46	.493
Negative Affect (Positive and Negative Affect Scale)	-.284	46	.778
Brief Compassion Scale	.659	46	.513

### **Assumption of homogeneity of regression slopes**

The assumption of homogeneity of the regression slope was examined by plotting regression slopes, and by examining statistically the interaction between the covariate and the independent variable.

**FFMQ.** The interaction between group and the covariate, FFMQ, was not significant,  $F(1, 44) = .47, p = .50, \text{partial } \eta^2 = .01$ , meaning the assumption of homogeneity of regression lines was met.

**IRI Perspective Taking.** The interaction between group and the covariate, IRI Perspective Taking, was not significant,  $F(1, 44) = .06$ ,  $p = .81$ ,  $partial \eta^2 = .01$ , meaning the assumption of homogeneity of regression lines was met.

**Baseline negative PANAS.** The interaction between group and the covariate, baseline negative emotion, was significant,  $F(1, 44) = 24.39$ ,  $p < .001$ ,  $partial \eta^2 = .36$ , meaning the assumption of homogeneity of regression lines was not met. The violation of this assumption will be discussed with the main analysis.

**SCBCS.** The interaction between group and the covariate, baseline compassion for others, was not significant,  $F(1, 44) = .28$ ,  $p = .60$ ,  $partial \eta^2 = .01$ , meaning the assumption of homogeneity of regression lines was met.

### Unplanned analysis: correlation of empathy measures

#### Assumption of normality

The normality of the MASC-MC has been examined above. To check for normality of Empathic Concern and Personal Distress, histograms were plotted.

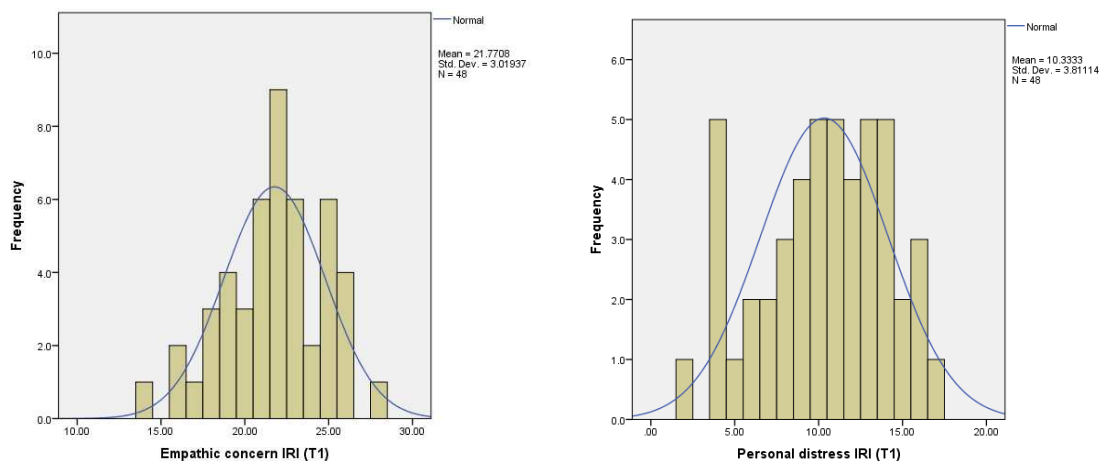


Figure E5. Histograms for the Empathic Concern and Personal Distress subscales

Since Personal Distress showed some departure from the normal distribution, Spearman's correlation coefficient was used.

### Assumptions for unplanned analyses: correlations with TMS

Normality has been examined for these variables in Figures A1 to D5. Since there are some deviations from normality, Spearman's correlation coefficient was used.

### Assumptions for unplanned analyses: ANOVAs

#### Assumptions of normality and homogeneity of variance

Histograms were plotted for each outcome measure for both the high mindfulness group and the low mindfulness group.

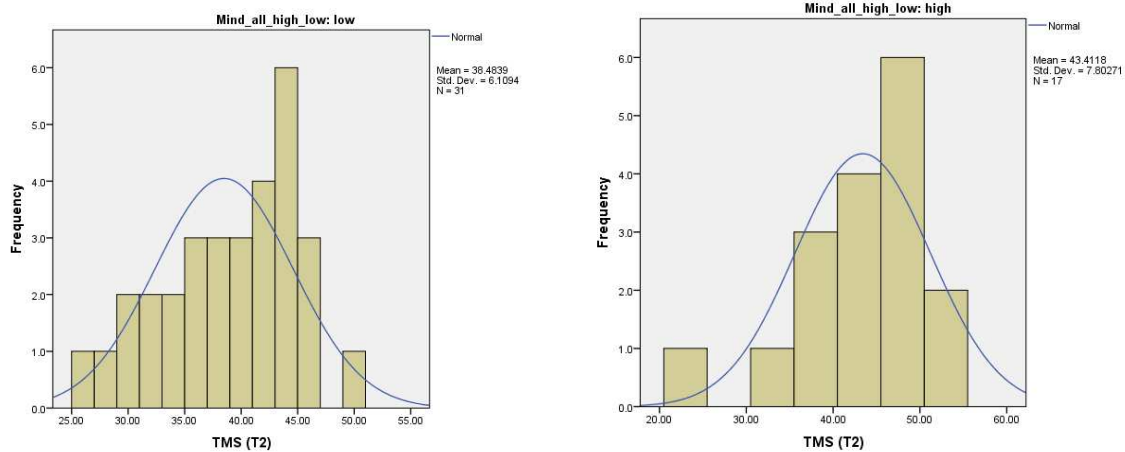


Figure F6. Histograms showing distributions of Toronto Mindfulness Scale (TMS) scores for high practice and low practice groups

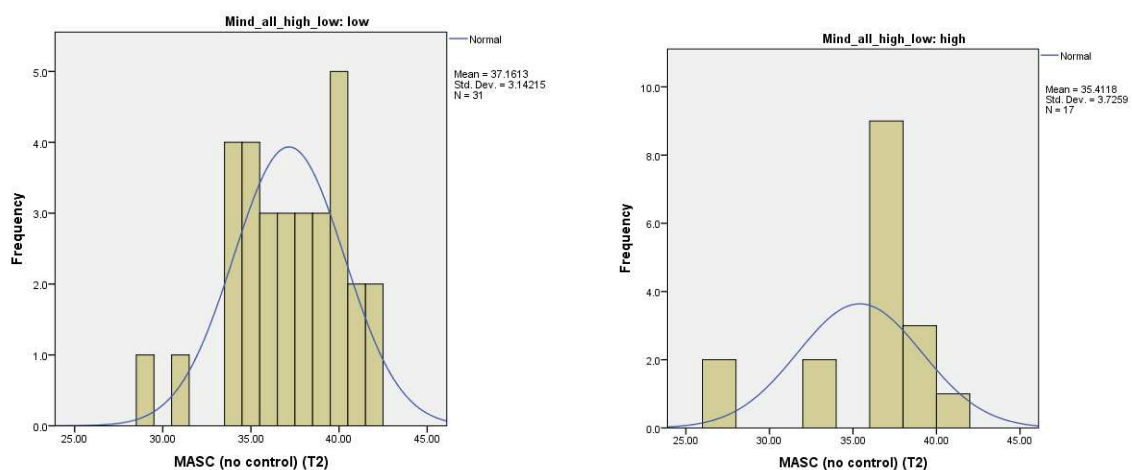


Figure G7. Histograms showing distributions of Movie for Assessment of Social Cognition (MASC) scores for high practice and low practice groups

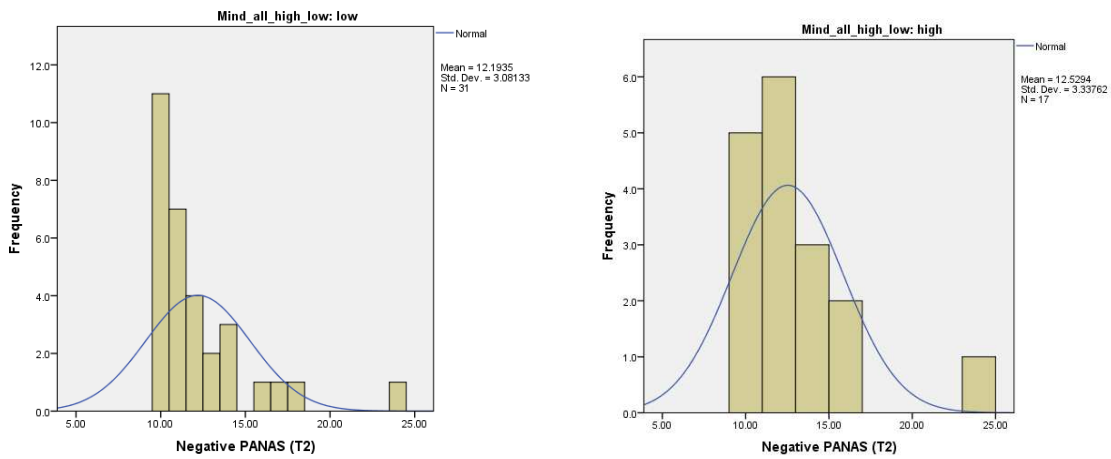


Figure H8. Histograms showing distributions of Positive and Negative Affect Scale (PANAS; Negative scale) scores for high practice and low practice group

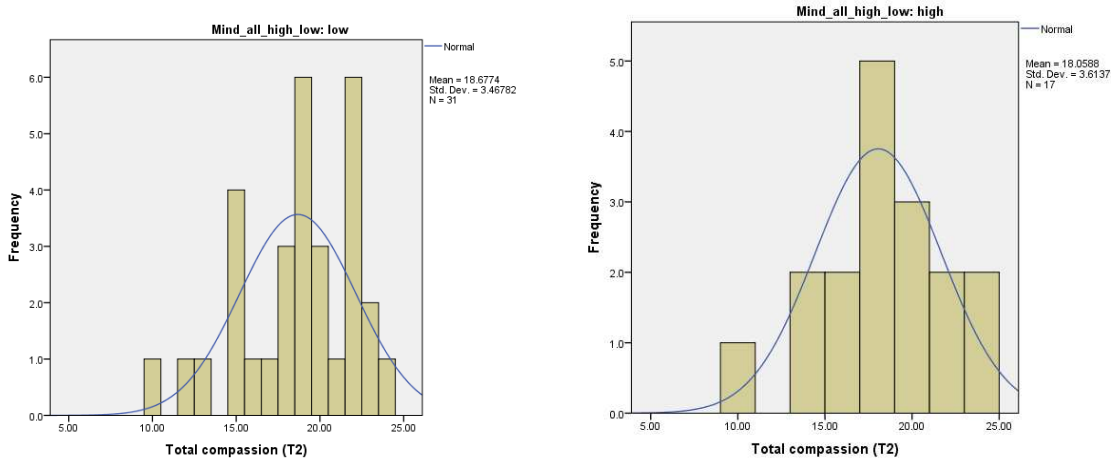


Figure I9. Histograms showing distributions of Movie Compassion measure scores for high practice and low practice groups

Levene’s test was used to examine differences between the variances for high previous practice and low previous practice groups, as illustrated in Table J10.

Table J10.

*Levene’s statistic showing differences between variances for high and low practice groups, for each variable*

<u>Variable</u>	<u>Levene statistic</u>	<u>df</u>	<u>p</u>
TMS	.383	1, 46	.539
MASC	.143	1, 46	.707
PANAS (negative subscale)	.077	1, 46	.783
Movie Compassion measure	.008	1, 46	.930

The histograms indicate that there were some deviations from normality. Table J10 indicates that the assumption of homogeneity of variance was met. In ANOVA of unequal group sizes, violation of the assumption of homogeneity of variance is more problematic than violation of the assumption of normality, indeed ANOVA is generally robust against deviations from normality despite different group sizes (Howell, 2002). Therefore since the assumption of homogeneity of variance had been met, it was decided to proceed with ANOVA.



## Appendix 12: Output for main analysis

### Tests of Between-Subjects Effects

Dependent Variable: TMS (T2)

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Corrected Model	354.608 <sup>a</sup>	2	177.304	3.978	.026	.150
Intercept	120.594	1	120.594	2.705	.107	.057
T1_FFMQ	354.087	1	354.087	7.944	.007	.150
group	21.338	1	21.338	.479	.493	.011
Error	2005.871	45	44.575			
Total	80043.000	48				
Corrected Total	2360.479	47				

a. R Squared = .150 (Adjusted R Squared = .112)

### Tests of Between-Subjects Effects

Dependent Variable: MASC (no control) (T2)

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Corrected Model	25.647 <sup>a</sup>	2	12.824	1.097	.343	.046
Intercept	1478.871	1	1478.871	126.455	.000	.738
T1_IRI_PT	13.647	1	13.647	1.167	.286	.025
group	14.598	1	14.598	1.248	.270	.027
Error	526.270	45	11.695			
Total	64646.000	48				
Corrected Total	551.917	47				

a. R Squared = .046 (Adjusted R Squared = .004)

### Tests of Between-Subjects Effects

Dependent Variable: Negative PANAS (T2)

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Corrected Model	312.339 <sup>a</sup>	3	104.113	30.143	.000	.673
Intercept	151.111	1	151.111	43.750	.000	.499
group	48.889	1	48.889	14.155	.000	.243
T1_PANAS_Neg	218.999	1	218.999	63.405	.000	.590
group * T1_PANAS_Neg	84.232	1	84.232	24.387	.000	.357
Error	151.974	44	3.454			
Total	7741.000	48				
Corrected Total	464.313	47				

a. R Squared = .673 (Adjusted R Squared = .650)

### Univariate Tests

Dependent Variable: Negative PANAS (T2)

T1_PANAS_Neg_Category		Sum of Squares	df	Mean Square	F	Sig.
low	Contrast	.150	1	.150	.025	.874
	Error	247.713	42	5.898		
medium	Contrast	8.236	1	8.236	1.396	.244
	Error	247.713	42	5.898		
high	Contrast	59.438	1	59.438	10.078	.003
	Error	247.713	42	5.898		

Each F tests the simple effects of mindfulness/ control within each level combination of the other effects shown. These tests are based on the linearly independent pairwise comparisons among the estimated marginal means.

### Tests of Between-Subjects Effects

Dependent Variable: Total compassion (T2)

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Corrected Model	173.255 <sup>a</sup>	2	86.627	9.729	.000	.302
Intercept	8.092	1	8.092	.909	.346	.020
T1_BCS	156.921	1	156.921	17.625	.000	.281
group	7.904	1	7.904	.888	.351	.019
Error	400.662	45	8.904			
Total	16928.000	48				
Corrected Total	573.917	47				

a. R Squared = .302 (Adjusted R Squared = .271)

## Appendix 13: Feedback summary and covering letters

### Feedback summary



Salomons Campus at Tunbridge Wells

### The impact of the direction of attention on therapeutic skills in trainee therapists: Summary

#### Background to research

There is a body of research suggesting that factors common across all therapeutic models, such as relationship and therapist qualities, make a substantial contribution to therapy outcome (Norcross & Wampold, 2011). This study focused on the relationship between mindfulness and three factors important for therapy outcome: empathy, compassion for others, and emotion regulation. It has been proposed that mindfulness could improve these factors, perhaps through allowing the therapist to step back and observe their thoughts and emotions, which could increase their ability to approach negative emotions in others (Shapiro & Carlson, 2009). Previous research has found some support for mindfulness increasing these variables (Irving, Dobkin & Park, 2009), but conclusions are limited about how much of this effect is due to mindfulness, as the interventions often have multiple components, and control groups are often inactive (i.e. waiting-list).

#### Method

An experimental design was used. Forty-eight trainee therapists were recruited and randomly assigned to one of two groups: mindfulness or control. Participants completed baseline measures of the variables of interest: mindfulness, empathy, compassion for others, and negative affect. Participants then took part in a 15 minute induction exercise: the mindfulness group did a brief (15 min) mindfulness practice, and the control group did a brief (15 min) 'mind-wandering' practice, which is used in the literature as a contrast to mindfulness. Participants then completed post-test measures of the four variables.

#### Results and discussion

ANCOVA was used to look at the effect of group on the post-test variables, whilst controlling for baseline differences. The 15 min exercises did not lead to a difference between groups in self-reported levels of mindfulness, with both groups scoring high on this measure, suggesting the control practice may have been used in a 'mindful' way by this sample. The predicted differences between groups in empathy and compassion for others were not found. The mindfulness group did show lower negative affect at post-test than the control group, this was only in participants who were high in negative affect at baseline. However, the above findings should be interpreted in the light of the 15 min exercises potentially not producing one group who was mindful and one who was not, thereby limiting

the conclusions that can be drawn about mindfulness from this data. Relating the findings back to the wider literature, it is possible that mindfulness may help to regulate high levels of negative affect, but that changes in empathy and compassion may require the therapist to include compassion-focussed meditation in their mindfulness practice. These conclusions are tentative as more research is needed in the area. This study highlights the complexity of controlling for and measuring mindfulness, which future research can build on.

#### References

- Irving, J. A., Dobkin, P. L., & Park, J. (2009). Cultivating mindfulness in health care professionals: A review of empirical studies of mindfulness-based stress reduction (MBSR). *Complementary Therapies in Clinical Practice*, 15(2), 61-66. doi: <http://dx.doi.org/10.1016/j.ctcp.2009.01.002>
- Shapiro, S. L. & Carlson, L. E. (2009). *The art and science of mindfulness*. Washington D C: American Psychological Association.
- Norcross, J.C. & Wampold, B. E. (2011). Evidence based therapy relationships: Research conclusions and implications for clinical practice. *Psychotherapy*, 48(1), 98-102. doi: 10.1037/a0022161

**Covering letter: Ethics panel**

3 Canning St  
Brighton  
East Sussex  
BN2 0EF

15/04/2014

Dear [name of Chair],

RE: The impact of the direction of attention on therapeutic skills in trainee therapists

Thank you for your approval of the above project in (date). Please find enclosed a summary of the project and its findings for your records. For your information, this summary has also been sent to those participants that opted-in to receive it, and to the Research Directors at the Universities where the project took place. If you would like any further information please do not hesitate to contact me on [elj17@canterbury.ac.uk](mailto:elj17@canterbury.ac.uk).

Many thanks for your input,

Yours sincerely,

Emma Justice  
Trainee Clinical Psychologist  
Canterbury Christ Church University, Salomons campus.

**Covering email: Research directors**

Dear [name],

RE: The impact of the direction of attention on therapeutic skills in trainee therapists  
(supervised by Dr Fergal Jones and Dr Clara Strauss)

Thank you for allowing the above project to run at [name] University. The project is now complete, please find enclosed a summary of its findings for your records. I hope you find this informative but if you would like any further information please do not hesitate to contact me (elj17@canterbury.ac.uk). Can I take this opportunity to thank you for allowing recruitment to take place at [name of University], which has been an invaluable part of this project coming to completion. Can I also please pass my gratitude to your administrative staff, particularly [name] who went out of their way to support me with this.

Yours sincerely,

Emma Justice  
Trainee Clinical Psychologist  
Canterbury Christ Church University, Salmons campus

**Covering email: Participants**

Dear all,

RE: The impact of the direction of attention on therapeutic skills in trainee therapists

You may remember some time ago that you took part in the above project. The project is now complete and as promised I am writing to you with a summary of the results. Please find this attached, I hope you find this interesting, and if you have any questions or wanted to discuss the findings further please do not hesitate to get in touch. Can I take this opportunity to thank you once again for your generosity with your time in participating, as without this the project would not have been possible.

Very best wishes,

Emma

Emma Justice  
Trainee Clinical Psychologist  
Canterbury Christ Church University, Salomons campus.